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STUDY OF DISEASE IN CHILDREN CLINICAL
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DISCUSSION ON NON-SPECIFIC DISTURBANCES OF HEALTH DUE TO VITAMIN DEFICIENCY.

Dr. Leonard Williams.

The gist of what I venture to put before you amounts to a plea for an extension of the meaning of the word "vitamin" far beyond the limits of the three accessory food factors to which the term is now commonly applied. If we consent to confine the meaning of the word to its present application, we shall be caught in this matter, as we have so often previously been caught, in a vicious circle of misleading nomenclature. A very superficial study of the question brings one face to face with the fact that it is impossible to place a vitamin in a watertight compartment; it must be considered in its relation to its fellows, and its fellows must be sought outside the members of the orthodox trinity of fat soluble A, water soluble B, and anti-scorbutic C. What are these fellows?

The first of these supplementary substances of which I would remind you are the mineral salts. The mineral salts are of course recognized, but only as the "country cousins" or "poor relations" of the great proximate principles. McCarrison was the first to point out that a deficiency of vitamins, commonly so-called, was of little practical consequence so long as the essential mineral salts were adequately represented and properly balanced, for the reason that in a well-balanced diet vitamins must of necessity be present. Dr. John Boyd Orr[1] pursued the subject in a most instructive paper, from which I quote the following pertinent passage:—

"The mineral may be present in a combination from which it cannot be easily dissociated in the alimentary canal. The diet, therefore, might contain a sufficient amount, as determined by chemical analysis, but, owing to the difficulty of assimilation, only a very small proportion might pass through the wall of the intestine into the metabolic field where it could be utilized. In the case of most minerals, both absorption from the intestine and excretion by the kidney are influenced by the amounts of other minerals present. Bunge showed that the addition of potassium salts to the diet caused an increased excretion of sodium salts in the urine. The results of some recent work by Dr. Richards, which will be published shortly, indicate that the potassium-sodium ration in the diet is a factor in determining the percentage of ingested calcium and phosphorus which can be assimilated and retained by the growing animal. These results confirm the conclusion reached by Zander and others over forty years ago.

"Hart and Steenbock have shown that excess of magnesium interfered with the utilization of calcium. Phosphorus and calcium are mutually dependent upon each other for utilization. So also, to some extent, are calcium and iron. The balance of the base and the acid radicles affects both absorption and excretion. These examples of the interdependence of the different mineral constituents of the diet to each other are of as much importance as the absolute amounts."

Where, I would ask—parenthetically—are we to obtain this balance except in Nature's laboratory? Certainly not by burning a lump of sugar in a test-tube and calling it a calorie. You might as well "stick a feather in your cap and call it macaroni."

Then there are the catalysts—the curious substances in Nature which, by their presence alone, manage to effect chemical changes without themselves taking part

[1] *Brit. Med. Journ.*, September 20, 1924, p. 505.

in these changes, or being themselves altered thereby. These were described at considerable length and with great lucidity by the late Sir William Bayliss, and a study of his work suggests very strongly to my mind that catalysts, as found in Nature, are essential to the proper utilization not only of the mineral salts, but also of the proteins, fats and carbohydrates, in the economy. And in the category of catalysts it is necessary to include the enzymes, which Bayliss defines as "catalysts produced by living organisms." And in association with these living organisms we have to recognize the paramount importance of the friendly bacteria, for it is they who produce the enzymes. Again, let me ask, parenthetically, where are we to obtain catalysts and enzymes save in Nature's laboratory? These delicacies do not emerge from kettles or casseroles.

Then, again, there is the much neglected and yet very obvious influence of chlorophyll. All life depends ultimately upon chlorophyll, and it is by the vegetable kingdom alone that it is manufactured. The subject of chlorophyll naturally suggests the question of sunshine, upon which it is completely dependent. Now, just as properly balanced mineral salts have been shown to be an adequate substitute for vitamins commonly so-called, so has sunshine. Sunshine protects from some of the recognized deficiency diseases just as surely as do the vitamins themselves. But the matter does not stop there. Professor Arthur Thomson says (*Empire Review*, January, 1925):—

> "A very remarkable discovery recently made is that, if rats kept on a deficient diet, and not fortified by ultra-violet light, are fed with the flesh of other rats which have been treated with ultra-violet rays, they thrive and escape rickets. Still more remarkable is the discovery just announced by Steenbock and Black that if the food ration known to be deficient in the rickets-preventing vitamin, is exposed to ultra-violet light before being fed to the rats, it mysteriously becomes adequate and sufficient."

Moreover, we have to recognize the part which sunshine plays in germinating grain; that is, of converting a body which is devoid of vitamins into a sprouting substance which is bursting with them. Grain does not germinate over a kitchen fire.

And, finally, there is the question, a very dangerous one at the moment, especially in this hall, so that I hardly like to mention it, but I must. I refer to electricity. It is so dangerous a question that I make my statements concerning it categorically —as facts concerning which there is no dispute, even among the factious. Thus, mineral salts are electrolytes; colloids have an electrical discharge; enzymes are colloids.

In pleading for inclusion in the ambit of the vitamins, mineral salts, catalysts, enzymes, chlorophyll, violet rays and electricity, I am asking you to recognize concerning the whole group what is obvious about most of them, namely, that each and every one is either completely destroyed by heat, or so profoundly altered thereby as to be no longer recognizable as a product of Nature's laboratory. Now these vitamins, substances and forces, as provided by Nature, constitute our powers of resistance to morbid influences, and when they have been altered and devitalized by the meddlesome hand of man they cease to operate as our reinforcements; in some cases they may even become our enemies. As a contribution to the present discussion, therefore, I would go so far as to say that all disease, whether microbic or metabolic, is what mathematicians call "a function" of cookery and concentration.

I have been accused of living on carrots alone, raw carrots, and of advising others to do so. I am the last person in the world to object to a gibe against myself, but I protest that I am not a vegetarian nor a fruitarian. All I do is to advocate the obvious. The obvious is that the substances and forces which I have mentioned must be represented in our dietaries and that they cannot be adequately represented and balanced so long as we fail to recognize that raw fruits and raw vegetables are essential to us. It matters very little what else we eat, but

these we must eat, and we are not eating them—hence the deficiency diseases. It is not the trinity of vitamins alone which is generally necessary to salvation; it is the other things to which I have referred which act in conjunction with the vitamins, and without which the vitamins are powerless. These others are like the berries on the mistletoe, and we know that "it's the berries as does it."

Lieut.-Colonel R. McCarrison, C.I.E., I.M.S.

The Effects of Vitamin Deficiency on the Gastro-intestinal Tract.

I propose to direct your attention to the results of certain experiments carried out in monkeys in 1919, which led me to suggest that food deficient in vitamins is an important factor in the causation of a number of the gastro-intestinal disorders so common at the present day. The disorders to which I allude include irregular action of the bowels whether in the direction of intestinal fluxes or of constipation, infection of the gastro-intestinal tract, chronic digestive disturbances with impaired absorption and assimilation of food, gastro-intestinal stasis, mucous disease in children, and colitis in adults. I will preface my remarks by a reference to three points: First, that vitamin deficiency of lesser degree when protracted over longer periods of time, may give rise to effects similar to vitamin deficiency of greater degree when protracted over a shorter period of time. Thus, while a diet of washed polished rice, which is deficient in vitamin A as well as in vitamin B, will cause *polyneuritis columbarum* in as short a period as twenty days, a diet of rice which is less polished may require as long as 120 days in which to produce this malady. There is this difference, however, in the two cases, that while in the first the animals die from acute vitamin starvation, in the second they are prone to be carried off by some intercurrent infection, commonly of intestinal origin, during the prolonged period they are under the influence of chronic vitamin starvation; and here I would emphasize again, as I did some years ago, that long before the grosser evidences of vitamin deficiency manifest themselves the health of the gastro-intestinal tract has begun to suffer.

The second point is the extraordinary variation in individual idiosyncrasy of animals of the same species and age to chronic vitamin starvation. Thus of 100 pigeons of the same strain and of approximately the same age, obtained from the same locality, about twenty-five were found to flourish during a period of 100 days on an exclusive diet of parboiled rice (which I have shown to be deficient in vitamin A). Fifty remained at maintenance level, while the remaining twenty-five went steadily downhill and frequently died of diseases due to pathogenic agents of which they happened to be the "carriers" at the time the experiment was initiated. It is this 25 per cent. of individuals, which fail to maintain a reasonable standard of health on a diet of which the vitamin content suffices to maintain the majority in fair or even in good health, which have, I think, a particular interest for us. The third point is that the results of experiments such as those to which I am about to draw your attention vary with the animals used and with the pathogenic flora of their intestine—a variation which is dependent not only on the food habits but on the place of origin of the animals.

It is always difficult to decide how far the results of nutritional experiments in animals are applicable to man, but in general it may be said that they afford a fair indication of "the way the wind blows." This difficulty is increased the further removed the animals are from man in the phylogenetic scale. Furthermore, in experiments in which synthetic diets, deficient only in a particular vitamin, are used, the difficulty is enhanced; for in nature few diets that are deficient in vitamins are perfect in all other respects. The effects of the vitamin deficiency are, as I, and others, have shown, enhanced by want of balance of the food in respect to starch, fats, suitable protein or mineral elements. To avoid these difficulties as far as possible I used wild monkeys which a few days before the commencement of the

experiments had been living their natural life in the jungles of Madras. I fed my control animals on a diet of wholemeal bread, milk, groundnuts, onions, fresh butter, plantains and water ; and those animals in which I desired to observe the effects of vitamin deficiency I put on the same diet after it had been subjected to a sufficient degree of heat in the autoclave to reduce its vitamin value very markedly. Some were given fresh onion in addition to this autoclaved food, in order to ensure a sufficient supply of vitamin C ; some were given onion and fresh butter in order to ensure a sufficient supply of both vitamin C and vitamin A ; the food of all of them was deficient in vitamin B. The control animals, which received this food in its natural state, remained in good health ; a few of them had some jaundice early in the course of the experiment, due, no doubt, to sudden restriction of their active habits and to the over-eating of groundnuts. But those that received the autoclaved food, whether with onion alone or with butter and onion, soon began to show signs of impaired health. They survived for periods ranging from fifty-one to one hundred days. Careful history sheets of the symptoms they exhibited were kept. During this period they lost weight and their appetites became impaired. The most constant and one of the earliest symptoms from which they suffered was diarrhœa either with or without mucus and blood in the stools. Dysentery without preliminary diarrhœa developed in five of them ; diarrhœa alone in four ; so that of the eleven animals fed in this way, nine or 81 per cent. exhibited a severe intestinal flux, while the eight control animals, living in the same room and having the same food which had not been autoclaved, remained free from these disorders and indeed in good health generally (after a dose or two of Epsom salts had freed them of their jaundice). Of the five animals which suffered from dysentery some were found to have cystic and active forms of *Entamœba histolytica* in the stools, the active forms containing ingested blood-corpuscles. Others had amœbæ also, but unless these were found to contain blood-corpuscles the diagnosis of amœbic dysentery was not made. Examination of the eight controls showed that two of them were " carriers " of entamœba cysts. The conclusion was accordingly reached that the animals which presented amœbic dysentery had also been " carriers " of this organism when the experiment was begun, and that the amœba had remained harmless to the well-fed animals while it attacked those the food of which was deficient in vitamins. No bacteriological examination was made to ascertain the nature of the dysentery, which was not diagnosed as amœbic, or of the diarrhœa ; but it can hardly be doubted that bacterial agents were concerned in their production. The first effect, then, of food deficient in vitamins is to render the intestinal tract, and more especially certain parts of it, susceptible to invasion by bacterial or protozoal agents. It renders pathogenic to the ill-fed host organisms that may be non-pathogenic to the well-fed host, a conclusion which I reached in 1914 when I observed the greatly increased susceptibility of pigeons to infection by *B. suipestifer* when fed on polished rice.

In due time these monkeys died, or more often they were killed when about to die. The gross anatomical lesions present were as follows: (1) Enlargement of the mesenteric glands ; (2) dilatation of the stomach ; (3) congestion of the gastro-intestinal tract with ecchymoses scattered over its peritoneal surface, more commonly present in the colon ; (4) ballooning of various parts of the tract, especially of the colon ; (5) marked thinning of the muscular walls of the colon and partial disappearance of its longitudinal muscular bands. On opening the tract the following pathological changes were observed: (1) Congestion and ecchymosis of the stomach, especially in the region of the pylorus, were present in six cases ; shallow ulcers of the size of a threepenny-piece were present in two cases ; in another papillary downgrowths of epithelium, suggestive of a commencing carcinoma, were found in the region of the pylorus. (2) Congestion and ecchymosis involving part or whole of the small bowel were occasionally present. (3) In the colon the changes consisted in greater or lesser degrees of colitis, sometimes involving the

whole viscus, sometimes part of it: colitis was the most marked and one of the most constant of the morbid anatomical changes present; in my experience with monkeys, colitis is the most common consequence of food deficient in vitamins. On histological examination the changes found in various parts of the tract included: (1) Congestion and hæmorrhage; (2) atrophy of the muscular coats of the bowel, especially prominent in the colon; (3) degenerative changes in the myenteric plexus of Auerbach, sometimes limited to a few ganglia; (4) atrophic, necrotic and inflammatory changes in the mucous membrane of the entire tract, particularly marked in the colon; (5) bacterial invasion of the bowel walls frequently extending deep into the coats of the viscus; (6) the opening up of portals of entry by which bacteria might enter directly into the blood-stream; and (7) pronounced atrophy of the lymphoid elements of the intestinal mucosa.

Most, but rarely all, of these changes were present in all deficiently-fed animals. In some they were more marked than in others—the rule that each individual is a law to itself in regard to the effects of vitamin want being observed also in these experiments. In general they were more pronounced and more frequent in those animals whose food was deficient in vitamin A as well as in vitamin B. Thus, colitis was present in all six monkeys fed on autoclaved food deficient in both vitamins, but present only in two out of five animals whose food was deficient only in vitamin B. Shallow ulcers in the region of the pylorus were present in two of the former group and in none of the latter; marked dilatation of the stomach in four of the former and in two of the latter; enlargement of the mesenteric glands in all six of the former and in four of the latter; atrophy of the pancreas in four of the former and in two of the latter.

More recently Dr. Cramer, using rats fed on synthetic diets deficient either in vitamin A or in vitamin B, has found that digestion of fats is less complete or less rapid in the absence of vitamins; that the effect on fat-absorption is an immediate one; and that vitamins A and B, especially the latter, produce a definite stimulating action on the processes of intestinal digestion and absorption. He attributes to want of vitamin A alone the loss of the protective mucus covering the mucous membrane; this is an important factor in facilitating the bacterial invasion of the mucosa which he also observed in his animals; and he attaches to this deficiency an important share in causing the impairment of the absorptive function of the mucous membrane. He regards the atrophy of the lymphoid elements of the intestinal mucosa, to which I have referred, as the specific effect of want of vitamin B.

Dr. Gross, working also with rats, and using synthetic diets deficient in vitamin B, has demonstrated *in vivo* and *in vitro* that pronounced impairment of the motor functions of the intestinal tract, due either to atrophy or other qualitative changes in the muscular or neuro-muscular apparatus, results in consequence of want of this vitamin.

In a recent paper published in the *Journal of Experimental Medicine*, December, 1924, Pappenheimer and Larimore record the occurrence of gastric lesions in 55 per cent. of rats fed on synthetic diets in which the only known deficiency was want of vitamin A. They found that the addition of cod-liver oil to these diets afforded complete protection against these gastric lesions. The authors discuss various pros and cons in connexion with their observations, amongst others that the ingestion of some infective agent with the animals' hair (which they habitually ate), or irritation from the hair itself, are factors concerned in the production of these lesions. The kernel of their observations, however, consists in this, that the incidence of these gastric lesions is definitely related to deficiency in the diet of some undetermined substance which for practical purposes may be classed with the vitamins. The authors comment on the fact that the gastric lesions resulting in their rats from food deficiency bear a remarkably close resemblance to those described by Fibiger as resulting from infestation by the nematode *Spiroptera neoplastica*, which was never

present in their rats. They point out that in the only reference to the diet of Fibiger's rats it is stated that they were fed exclusively on white bread. In this connexion I may refer to the effect, which I observed in pigeons, of diets deficient in vitamins, in favouring the action of an invisible virus capable of causing epithelial new-growth and of the disease known as *epithelioma contagiosum;* and point to the similarity of the epithelial hyperplasia produced by this virus to the papillary outgrowths and downgrowths of epithelium figured in some of Pappenheimer's gastric lesions in rats and comparable to the downgrowths of epithelium, resembling commencing carcinomatous change, encountered in one of my monkeys.

If, then, it be remembered that lesser degrees of vitamin deficiency when protracted over longer periods of time are likely to give rise to effects similar to those of greater degrees of deficiency when protracted over shorter periods of time, the derangements in the digestive, absorptive and motor functions of the gastro-intestinal tract and its greatly increased susceptibility to attacks by bacterial and other pathogenic agents, which have been shown to result from vitamin deficiency in monkeys and in rats, provide reasonable grounds for the belief that vitamin deficiency—using the term "vitamin" in its widest possible sense—is to be regarded as an important factor in the causation of the common gastro-intestinal disorders of mankind.

Dr. W. Cramer.

In discussing the non-specific impairments of health due to vitamin deficiencies I meet from the outset with the difficulty that I find no justification for the fundamental distinction between specific and non-specific impairments of health due to vitamins.

[Dr. Cramer here paid a tribute to the pioneer work of Colonel R. McCarrison [14] on the aspect of the vitamin problem which was under discussion that evening. He said he was in agreement with most of Colonel McCarrison's conclusions. Minor differences in their observations could be attributed to the fact that he (the speaker) had worked mainly with single vitamin deficiencies operating on a synthetic diet with properly balanced inorganic salts, while Colonel McCarrison had observed the effects of the deficiency of more than one vitamin in natural diets where other factors such as proteins of low biological value and a disproportion of inorganic salts were additional factors. He (Dr. Cramer) protested against the extension of the vitamin conception as unjustified; he pleaded for a restriction of the term vitamin to the three food accessory factors, A, B and C. Contrary to what had been stated light could not replace any of the vitamins. Several observers, including himself, had succeeded in rearing animals in the absence of light, but nobody had as yet been able to rear animals in the absence of vitamins no matter how much light was applied. All that light could do in those circumstances was slightly to delay the fatal issue.] He then continued:—

I shall attempt to show that both are manifestations of specific pathological changes and that in fact the so-called specific deficiency diseases are merely the last links in a chain of events of which the so-called non-specific impairments are the earlier ones.

At the present time the existence of non-specific impairments of health is either denied or is disregarded as not being of much account. The reason for this is the peculiar conception which has been formed of the ætiology of the so-called deficiency diseases. When vitamins are withheld from the diet the growth of the experimental animals is at first retarded and then ceases, so that the animal for a time maintains its weight. It is asserted that an animal in that condition, as long as it maintains its weight, is nevertheless in a perfectly normal state of health. For instance, in the recent edition of Bayliss's "Principles of General Physiology," revised by Drummond, it is stated "that a diet wanting in something necessary for growth may nevertheless be capable of keeping up the general condition in a normal manner." Later, the weight declines and then obvious symptoms of disease appear and the vitamin deficiency is believed to produce its effect on health from the period when the weight of the

animal declines. It is asserted that all cells require a certain small amount of vitamins. Normally a store of vitamins is present in the body so that an animal can live for a time without an external supply of vitamins by using the store of vitamins in the body and that only when this store is exhausted does the disease make its appearance. It is, as you see, a biochemical conception of disease in which the cells of the body are titrated, so to speak, against the vitamins and in which the specific disease plays the part of the indicator. To a meeting of clinicians I need not labour the point that cessation of growth, and even diminution of growth in a young, growing organism, is a symptom of disease, or at any rate, of ill-health. There is no evidence whatever for the strange doctrine that a young animal, which merely maintains its weight and does not increase its weight, as it would normally do, is in a perfectly normal state of health. From this biochemical conception of disease another important conclusion has been drawn. It is stated that as a practical problem of nutrition the question of an adequate supply of vitamins is of no importance in civilized countries, because the absence of the specific diseases, such as beri-beri, keratomalacia and scurvy, is in itself evidence that we must be getting an adequate supply of vitamins. For reasons which I shall give I hold this conclusion to be a grave error. I hope to show that when vitamins are withheld from the diet the health of the animal declines at once, and as the deficiency continues there is a progressive series of pathological changes which ultimately develop into the specific diseases. This can be illustrated most clearly in the case of vitamin B. When vitamin B is withheld from the diet a number of changes make their appearance at a time when the animal is still maintaining its weight. There is a fall of temperature, loss of appetite, constipation, a delayed passage of food from the intestinal tract, a progressive atrophy of the lymphoid tissue which manifests itself by a progressive lymphopenia and impaired absorption of food. (This was illustrated by lantern slides.) Conversely, when these changes have appeared the administration of vitamin B immediately leads to their disappearance. Dr. J. C. Mottram and myself were even able to show that in a perfectly normal animal the content of the food in vitamin B greatly modifies the food absorption from the intestinal canal. We see, then, that substances rich in vitamin B have an intensely stimulating effect on the functional activity of the whole digestive tract, and of the lymphoid tissue, to which, for reasons given in our paper, we attribute an important function in maintaining the nutrition of the body, a view recently confirmed by Carrel. All these observations were made with yeast as a supply of vitamin B, and if such observations had been made before the conception of vitamins was formulated, nobody would hesitate to conclude that yeast has a powerful stimulating drug-like action on the processes of digestion and assimilation.

It has long been known, clinically, that yeast is a very efficient laxative, and observations are on record by French clinicians on its value as a laxative both in normal persons and in intestinal infections. These have recently been confirmed in a careful investigation by Murlin and Mattil, but, as you see, the action of yeast which is rich in vitamin B is much more comprehensive than that of merely a laxative. The specific stimulating action of vitamin B on the functions of the digestive tract reminds one of the specific stimulating actions of hormones, and the functional atrophy resulting from the absence of vitamin B corresponds to the functional atrophy in specific tissues resulting from the absence of certain hormones—for instance, the atrophy of the uterus in the absence of the ovarian hormone, the atrophic changes in myxœdema and Addison's disease, and so on. There is a complete analogy between the action of this vitamin and that of hormones.

The diseases resulting from the absence of hormones, such as myxœdema or Addison's disease, do not appear suddenly, but develop gradually. The early stages are difficult to identify and in the same way we have as a result of the absence of vitamins early stages with but slight symptoms, which gradually lead into the more

severe impairments of health which we call a disease and which have definite, easily recognizable, symptoms.

In the case of vitamin B, the absence ultimately leads to a complete marasmus; at any rate in rats, the animals, after passing into a deep state of emaciation, die practically of inanition. One important point to which I wish to draw your attention is, that these animals, though they are in this miserable state of nutrition, practically never develop infections spontaneously. They are, however, more susceptible to experimental infections produced by the injection of bacteria subcutaneously. This is due to a weakening of their humoral defences against infection and possibly also to the fall of temperature. On this point Dr. Findlay will speak, and I therefore do not wish to go into further details. But I wish to emphasize a distinction, the importance of which has not been sufficiently recognized hitherto, namely, the distinction between the resistance to spontaneous infection and the resistance to experimental infections. Spontaneous infections enter, as a rule, by the mucous membranes of the digestive tract or of the respiratory tract. Experimental infections circumvent these portals, and, as we shall see presently, this accounts for the apparent paradox.

In the effect of a vitamin A deficiency we have exactly the opposite condition so far as bacterial infections are concerned. These animals always develop bacterial infections spontaneously and die as a result of these infections. They are not always in a state of inanition or marasmus, so that this diminished resistance cannot be accounted for by the general state of nutrition. Their temperature is normal unless it is modified by the infection, so that a lowered temperature cannot be made responsible for the diminished resistance to infections. One would expect that in these animals the humoral defences as measured by the serological reactions would be distinctly impaired and that these animals would also be more susceptible to experimental infections.

Several workers have studied this question, and all are agreed that, curiously enough, these animals are just as resistant to most experimental infections as normal animals, and that their serological reactions, such as the formation of hæmolysins, agglutinins, opsonins, are just as effective as in a normal animal. This paradoxical result leads to the important conclusion that the resistance of an animal to infections is not represented entirely by its humoral defences. How then can this condition be explained? The infections which spontaneously occur in these animals affect the respiratory tract, the digestive tract and the eye. This is associated with atrophic changes in these organs. In the intestinal mucous membrane there is a functional atrophy of the mucus-secreting cells, as I have shown. In the respiratory tract Mori [15] showed practically the same condition. In the eye there is an atrophy of one of the para-ocular glands, and Findlay has shown that by bathing with normal tears the eyes of animals kept on a vitamin A-free diet the onset of the xerophthalmia can be prevented. It follows, therefore, that these portals of entry to bacterial infection have local defences, represented by the tears in the case of the eyes, and probably by the mucus in the case of the mucous membranes of the respiratory and digestive tracts. When these local defences are weakened by vitamin A deficiency we get at first a local infection of these tissues which remains confined to the tissues as long as the humoral defences remain unimpaired. From these infected tissues bacteria enter the blood-stream and are dealt with there by the normal general defences, such as the clumping of bacteria by platelets and by the formation of agglutinins. Kingsbury and I found, for instance, that vitamin A deficient animals have in their serum an agglutinin against *Bacillus coli*, indicating that coli-bacilli have passed from the intestine into the blood-stream and there elicited the normal reaction of defence. There is also a progressive diminution in the number of platelets, which are playing their part by attaching themselves to the bacteria and clumping them. A similar thrombopenia accompanying certain

infections in man has been demonstrated recently in the case of a common cold and in several cases of pneumonia. If the deficiency persists sufficiently long the constant invasion of bacteria from the mucous membranes of the respiratory and digestive tracts weakens the humoral defences and may eventually lead to a septicæmia.

Eventually the animals die from the direct or indirect results of the infections. The most common condition is a septic broncho-pneumonia in rats. Another change frequently resulting is an anæmia. Recently, suppuration of the middle ear associated with a nasal sinusitis has been described as of frequent occurrence in vitamin A-deficient rats. Less frequently there is enteritis. It is therefore quite arbitrary to call the eye disease the specific disease for vitamin A deficiency and to look upon the infection of the respiratory and digestive tracts as non-specific. The reason why eye infection has been regarded as the specific infection is merely that in the living animal it is most obvious. In fact, in so far as the usual cause of death is lung infection, one would, clinically, be inclined to stress the importance of the lung infection.

How arbitrary is this distinction from the clinical point of view will appear from the following statement. In various schools and institutions there have been outbreaks of disease due to vitamin A deficiency long before the vitamins were discovered. They were not understood. The last outbreak reported in the literature occurred in 1913 in a school near Edinburgh; this was investigated by Dr. Macgowan and Dr. McNeil, but they did not describe this as an outbreak of an eye disease. They described it as an epidemic of pneumonia which differed from ordinary pneumonia and which therefore they called a "distorted pneumonia." They only mentioned incidentally that a granular conjunctivitis was also associated with this condition in most children. A few years later a similar outbreak occurred in Denmark in an institution in a group of children receiving a varied diet, but in which no milk fat was given, the children receiving skimmed milk and margarine. At that time the experimental production of xerophthalmia had been demonstrated by McCollum. Dr. Bloch, who observed the condition in Denmark, recognized its identity with the experimental condition and in his account this epidemic is described as an epidemic of xerophthalmia. And he only mentions incidentally that the children suffered from various catarrhal conditions of mucous membranes (broncho-pneumonia, pyuria). Subsequently an inquiry was instituted in Denmark into the occurrence of xerophthalmia throughout the country and was reported upon by Dr. Blegvad [2] in a most instructive paper. He showed that when this condition terminated fatally the cause of death was in the great majority of the cases broncho-pneumonia and in the remainder enteritis. It has long been known that xerophthalmia occurs in association with other infections, and from what I have just shown it is of course easy to understand why that should be so. But those who believe in the orthodox biochemical conception of vitamin-deficiency diseases offer a very different explanation. Bloch, in a recent paper, explains the association of xerophthalmia with infections by saying that infections lead to an increased consumption of vitamin A, and, when children are already on a restricted intake of vitamin A, this produces a deficiency which leads to xerophthalmia. There is not a shred of evidence for the statement that infection leads to an increased consumption of vitamin A, and the whole explanation is simply made *ad hoc* in order to make the facts conform to the dogma of the biochemical conception of deficiency diseases. I mention this, because it illustrates very clearly how the biochemical conception of specific deficiency diseases limits the outlook on the vitamin problem and indeed distorts it. The fact is that the eye infection occurs frequently after general infections. According to the view represented by Bloch the infection contributes to the xerophthalmia. In fact he speaks of general infections as ætiological factors in xerophthalmia. My contention is that in these cases the inadequate supply of

vitamin A is just as much responsible for the broncho-pneumonia as for the eye infection.

The experimental evidence which I have briefly summarized is, I hope, sufficient to show that the substances present in many food-stuffs which we call vitamins have a positive stimulating drug-like action on specific tissues and organs. These tissues and organs are dependent for their functional integrity on the continued stimulation provided by the intake of vitamins with the food. There is thus a close analogy between the action of these substances and that of hormones. The latter are drug-like substances provided by the body: "autacoids" in Schäfer's nomenclature. The vitamins are drug-like substances provided by the food: one might call them "trophacoids." In other words: we are not self-contained, but dependent partly on stimuli provided by the environment. If the stimulus provided by these substances is diminished or withheld we get specific impairments of function of those organs which are functionally dependent on them. The severity of impairment of function varies with the degree to which the stimulus provided by the specific hormone is weakened and with the length of time over which this weakening of the stimulus is allowed to act, but however slight the impairment of function may be, it is specific. With the thyroid hormone we have all degrees of ill-health resulting from a diminution in the secretion of this hormone, from slight degrees of hypothyroidism with symptoms which are not very obvious, to complete atrophy of the thyroid with a fully developed condition of myxœdema. But we do not distinguish between myxœdema as the specific disease and other less obvious and earlier symptoms as non-specific. And so we have all degrees of impairment of health, from a restricted intake of vitamins with comparatively slight and not very obvious symptoms to the complete deficiency with its so-called specific deficiency disease. It is quite unnecessary to invent a new pathology for the diseases resulting from dietetic deficiencies and to postulate a factor for each disease as it occurs: a growth-promoting factor, an antineuritic factor, and so on. We do not postulate in the thyroid hormone an anti-myxœdematous factor and a metabolism-promoting factor. Nor do we assert that every individual who has not myxœdema has a normally functioning thyroid.

Up to the present the extensive work on vitamins has concerned itself almost exclusively with the effects produced by a more or less complete absence of vitamin, and too little attention has been paid to the effects produced by the complete degrees of restriction in the intake of vitamin—"vitamin-underfeeding" as it may be called. Though as a general problem of nutrition this condition is of far greater importance, its existence is denied by most authorities. What evidence is there that such a condition of vitamin underfeeding exists, in which health is impaired without any obvious symptom of disease?

My attention was drawn to the existence of such a condition owing to the fact that in our laboratories rats had been kept for many years on a diet of a uniform composition which enabled them to grow and to breed in what appeared to be a satisfactory manner. The diet consisted of bread and water, boiled maize and rice. The diet was constructed in this way, purely empirically, because it was cheap, easily given, and enabled the animals to grow and reproduce themselves. So far as one could judge by appearances the animals were perfectly well. When I began my observations on vitamins and, as a result, recorded the growth curves I was struck by the fact that these animals grew much more slowly than animals which were kept on an artificial diet which is always given as a control and which contains an abundant supply of vitamins in the form of marmite and cod-liver oil. On considering the vitamin content of the laboratory diet it was obvious that this diet, although it contained a certain amount of the vitamins necessary, was not very abundant in them. By adding vitamins in the form of cod-liver oil and marmite to the ordinary natural laboratory diet, the general condition of the laboratory stock

was greatly improved. The animals grew more rapidly; they seemed to breed sooner and the young were in much better condition. On the original vitamin-restricted diet the older animals developed with great frequency a septic broncho-pneumonia: this condition has become much rarer since vitamins have been added to the diet. The difference between the two diets is particularly striking when one studies the effect on a pregnant animal.

The question of the nutrition of the pregnant organism has been the subject of numerous elaborate and careful investigations, all of which were carried out before the discovery of vitamins. The detailed account of the contradictory results obtained will be found in a chapter in Marshall's "Physiology of Reproduction." In the light of the vitamin conception the result of all the work that has been done can now be summarized as showing that on a vitamin-rich diet the mother not only retains enough nitrogen and inorganic salts to satisfy the needs of the fœtus, but actually stores it in excess, presumably as a preparation for the process of lactation. If, however, the diet does not contain an abundant supply of vitamins, the mother loses and the fœtus grows at the expense of the mother without itself being very much affected in its growth during pregnancy. After birth the new-born suffer also as the result of such a diet, since it affects the milk. There is therefore reason to suspect that in the metabolic disturbances of pregnancy which are related to the condition of acidosis an inadequate supply of vitamin may be an important ætiological factor.

How profoundly the whole make-up of the animal may be affected by such dietetic conditions has been shown recently in an experiment the result of which one may truly describe as astonishing. The experiment was carried out by Webster [17] in the Rockefeller Institute in the course of experiments on the various factors governing epidemics. It was carried out on mice and it was shown that differences in the race of mice and differences in the virulence of bacteria had considerable effects. Using one race of mice which had been kept in the laboratory and bred there on a diet of bread and a low grade milk which had been pasteurized, and infecting them by means of a stomach-tube with measured doses of a strain of mouse-typhoid bacteria, Webster obtained a mortality curve which gave him 80 to 100 per cent. of mortality. When he took the same strain of bacteria and the same race of mice but kept the mice on a diet which differed from the ordinary laboratory diet mainly by the addition of a large amount of vitamin A in the form of butter fat, Webster obtained a very different mortality curve. Instead of getting a mortality of 80 to 100 per cent. he got a mortality from 10 to 20 per cent. and that, although the two batches of mice fed on the two different diets were to all external appearances equally normal. A difference of this kind between apparently normal animals seems at first quite incomprehensible. But when we remember the effect of vitamin A in stimulating the mechanism responsible for the local defences of the intestine against infections, we can understand the significance of this result. An individual on a properly balanced diet may be the carrier of a pathogenic organism in his intestinal or respiratory tract without suffering in health as the result, until owing to a dietetic error his local defences are weakened. Such a condition was illustrated by Colonel McCarrison in his experiment on monkeys to which he referred. In the light of such observations it is difficult to overestimate the ætiological importance of vitamin-underfeeding on the intestinal and respiratory tracts.

It may be objected that these results were obtained only in animals living on a uniform diet; that where vitamin-underfeeding occurred in man it was in institutions with prescribed and similarly uniform diets and that such a condition is not likely to occur in the general population of a civilized community free to choose their food. This objection is negatived by the experience of Denmark. That country is mainly an agricultural country which has a main source of its wealth in the production and

export of dairy produce. It has been possible to show from statistics that from 1909 till 1917 there was an increasing export of milk fat with an increased substitution of margarine for butter and a decreased consumption of milk fat. During the end of 1917 the importation of margarine was stopped owing to the blockade. As a result the people in Denmark had to increase their own consumption of milk fat in order to satisfy their own needs. In order to guarantee a sufficient amount to everybody the Danish Government rationed butter, so that during 1918 and 1919 even the poorest classes who had previously consumed margarine and skimmed milk now received half a pound of butter a week per head at a fixed low price. The investigation of Blegvad [2] on the occurrence of xerophthalmia in Denmark shows that from 1909 to 1917 this disease increased in frequency in a manner exactly parallel to the diminished consumption of milk fat. During the years 1918 and 1919 when everybody received butter, the disease suddenly disappeared almost completely, to appear again in 1920 when rationing had ceased. At the same time there was during 1918 and 1919 a sudden diminution in the general mortality of infants.

The experience of Denmark shows clearly that as the result of economic factors underfeeding does occur in the population of a civilized community living on freely chosen diets. From what I have said before, it is clear that the extent to which the health of the general population suffered is not measured by the few hundred cases of xerophthalmia. They are merely the high water mark indicating the maximum to which economic pressure affects the dietary of the people.

The same factors are operative in this country and indeed in every civilized country. Butter is replaced by margarine, skimmed milk is used instead of full milk and even full milk is partly deprived of its content in vitamin A by being pasteurized or boiled. What is true for vitamin A applies equally to vitamin B. For the poorer classes this vitamin ought to be supplied mainly by cereals—in other words, by bread. But white bread is practically free from vitamin B. The children of the poorer classes in whose dietaries bread and margarine play a large part are cut off from their main supplies of vitamins A and B.

I have tried to collect the evidence which I have placed before you from as many different sources as possible. We have seen that a restriction of the vitamin intake must be made responsible for numerous conditions of ill-health not generally suspected as being caused by dietetic errors. There is further the possibility disclosed both by my observations and those of Webster [17] that differences in the vitamin intake need not even manifest themselves by obvious symptoms of ill-health but may impress themselves upon an apparently normal healthy organism as a lasting weakness which discloses itself only when the organism is exposed to a strain. If you consider this evidence is it not clear that the discovery of these dietetic factors has placed in our hands a therapeutic and prophylactic weapon of quite unsuspected possibilities in improving the health of the community?

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Dr. G. M. Findlay.

The most important of the non-specific disturbances of health due to vitamin deficiencies is undoubtedly an increased liability to suffer from and succumb to certain bacterial infections, for this decreased resistance occurs not only when one or more

vitamins are totally lacking in the diet, but even when these factors are present in less than optimal amounts.

In studying the reasons for this breakdown in the defence mechanism of the body it is essential to investigate not only the barriers to infection, but also the condition of the leucoblastic tissues, more especially those concerned with the formation of polymorphonuclear leucocytes.

In animals fed on diets deficient in vitamin A it is rare to find lesions in the leucoblastic bone-marrow though in a very few adult animals maintained for many months on a diet deficient in vitamin A, an aplastic condition of the marrow has occurred. Correlated with this absence of degeneration in the marrow is the fact that there is no reduction in the number of leucocytes in the blood-stream in this form of deficiency. In fact after the onset of keratomalacia or broncho-pneumonia it is not uncommon to meet with a polymorphonuclear leucocytosis. At the same time there is a fall in the bactericidal power of the blood against pyogenic organisms. The bactericidal power of whole blood is largely due to the action of the polymorphonuclear leucocytes. A reduction however in the bactericidal power of the blood occurs in all acute infections, so that the lack of vitamin A in the food does not directly cause the fall in the bactericidal power of the blood. The infections which are associated with a deficiency of vitamin A in the diet are thus solely related to a local breakdown at the sites of infection—the conjunctiva and the epithelial lining of the respiratory and intestinal tracts. In this connexion it is interesting to note that Fleming [1] has found in the secretion of the tears and respiratory mucous membrane a substance, lysozyme, which possesses a strong lytic action on certain bacteria. In rabbits suffering from keratomalacia as a result of vitamin A deficiency the amount of lysozyme in the tears is greatly reduced [2].

In animals fed on diets wholly deficient in vitamin B there occur numerous small hæmorrhages in the bone-marrow. This lesion first noticed by McCollum and his co-workers [3] in rats, is found also in guinea-pigs and pigeons. When the diet is not wholly lacking in vitamin B, a condition of gelatinous degeneration occurs in the bone-marrow, the leucoblastic cells being reduced in number. In acute vitamin-B deficiencies there is a reduction in the total number of leucocytes in the blood-stream, though this reduction, as pointed out by Cramer, Mottram and Drew [4] only affects the lymphocytes. While the opsonic power of the blood and the capacity to form agglutinins, hæmolysins, bacteriolysins, immune body, and antitoxin remain unimpaired, it is usual to find a fall in the bactericidal power of the blood in animals fed on diets deficient in vitamin B. This fall in the bactericidal power of the blood appears to be correlated in some way with the fall in body-temperature which occurs as the result of diets lacking in vitamin B—the fall in body-temperature taking place some time before the death of the animal. Animals with reduced body-temperature lose their resisting power to certain bacterial infections, a fact first demonstrated by Pasteur, who found that if chicken are kept on ice for some hours they become susceptible to infection with anthrax. Under normal conditions pigeons are totally immune to infection with the pneumococcus and meningococcus, but if their body-temperatures are reduced to below 40° C., either by the action of some antipyretic drug or by feeding on a diet deficient in vitamin B, then the intraperitoneal injection of pneumococcus or meningococcus is followed by a fatal septicæmia.

In the case of vitamin C, guinea-pigs with acute scurvy show many small hæmorrhages in the bone-marrow. In chronic scurvy in the guinea-pig, a condition characterized by no definite clinical symptoms, the bone-marrow undergoes either gelatinous degeneration or a fibrotic change which is not confined to the epiphyseal ends of the bones. If healthy guinea-pigs, and guinea-pigs with chronic scurvy, are given small intraperitoneal injections of organisms such as pneumococcus, *Bacillus coli* or staphylococcus, both groups survive, but if the number of organisms is increased, then the animals with chronic scurvy die, while the controls survive [5].

Thus when vitamins B and C are deficient in the diet the capacity to withstand pathogenic organisms is decreased as the result of an interference with the leucocytic response to infection.

This interference with the central defence-mechanism of the body does not necessarily imply, however, that in deficiently fed animals all bacterial infections run a more acute course than in normal animals. Thus, in animals inoculated with *Bacillus tuberculosis*, and fed on diets deficient in vitamins B or C the tuberculous process is not more extensive than in control animals. The same is true of animals fed on diets deficient in vitamin A, but in these, secondary bacterial infections are very common. The therapeutic use of cod-liver oil in human tuberculosis is thus probably due, at least in part, to its high content of vitamin A, whereby the patient is protected against the risks of secondary bacterial infection.

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The Royal Society of Medicine.
President—Sir StClair Thomson, M.D., F.R.C.S.

DISCUSSION ON NON-SPECIFIC DISTURBANCES OF HEALTH DUE TO VITAMIN DEFICIENCY.[1]

Sir StClair Thomson (President)

read the letter which the late Sir Clifford Allbutt sent to the *Times* in his 88th year on the subject of white versus whole-meal bread, in which he emphasized the value of the vitamin content of the whole-meal. It was (the President said) a very interesting contribution from a wise old man.

Dr. William Hunter

said that the chief feature of the presentment of the subject by the opening speakers—the one that appealed most to him, and had carried most conviction to his mind—was the great pathological basis of facts contained in the accounts given by Colonel McCarrison, Dr. Cramer, and Dr. Findlay. These they had made clear by the many lantern slides with which they had illustrated the exposition of their pathological findings.

These findings seemed to lift the whole subject of vitamins up and away from the hypothetical standpoint at which, probably, it was viewed by the majority of onlookers. To these the subject of vitamins A, B, and C, might appear to be connected with the nutritive values of cod-liver oil or butter (vitamin A), or with the subject of the antiscorbutic value of lime-juice (vitamin C), about which there had long been no doubt; or it might appear to be concerned with the merits of white bread or of unmilled bread, to which the attention of the public had been, from time to time, drawn by their respective advocates (vitamin B).

The General Issue Involved in the Subject of Vitamins.

The actual facts connected with the subject were, however, of another and very different character, and these facts had never been more clearly described in relation to each other than in the opening addresses of the speakers referred to.

The issue presented was that defined by Dr. Cramer, viz., that when vitamins were withheld from the diet, at once the health of the animal declined; and, as the deficiency increased, there was a progressive series of pathological changes. These might ultimately develop into specific diseases, such as that represented by the disease beri-beri, the nature of which had been so clearly elucidated by the work of Colonel McCarrison.

But, as the title chosen for this discussion indicated, another—and in relation to disease, an even more important—issue was involved in the subject. This was represented by the widespread disturbances of a non-specific nature that were equally, and far more commonly, producible by vitamin deficiencies—disturbances possibly predisposing to, or affecting, the severity of all other diseases, and particularly those connected with infections of various kinds (e.g., streptococcal, pneumococcal or tuberculous).

Vitamin Deficiencies as Causal Factors in Disease.

The chief interest of the actions of vitamins was in relation to their operation as causal factors in disease: (1) by influencing profoundly the efficiency of healthy processes of nutrition and assimilation in health, and by their deficiency or absence,

[1] Adjourned from February 16, 1925.

promoting corresponding deficiencies in these processes; (2) thereby correspondingly lessening the powers of resistance of the body to all other causal factors of disease, more especially to the chief factor of all, that of infection; and (3) by disturbing the endocrine relations of the tissues to each other.

Such was the general issue involved in the subject of these properties of food, which were only recognizable by their action, but not yet further definable in their physical, chemical, or biological characters.

Analogies with the Actions of Hormones or Endocrines.

In respect of their undefined characteristics, vitamins might seem to possess analogies with the equally mysterious (X Y Z) properties and activities of the great group of metabolic products termed hormones or endocrines, derived from the tissues of the body, whose reactions on each other so powerfully influence the activities of each one of these body-tissues.

But there was this important difference between vitamin deficiencies and hormone disturbances as potential causal factors in disease, viz., that no factor of disease, infective or non-infective, was connected with the formation of vitamins in the vegetable organism (food vitamin B and antiscorbutic vitamin C), or the animal organism (animal vitamin A of oil or butter). On the other hand, in the case of hormones or endocrines, while they equally existed as products of healthy metabolism, and while disturbances might—quite apart from the existence of any infection factors—arise from faulty relationships between tissues in respect of the deficiency or excess in their formation in one tissue as compared with another, the hormonic (X Y Z) disturbances might be likewise produced, and in many cases were produced or contributed to, by causal factors, especially those of infective nature introduced from without.

The thesis involved in the rôle of vitamins A B C in relation to endocrines X Y Z might be stated as follows: their deficiency is an important factor in (1) bringing about disturbances in nutrition and assimilation, and (2) thereby favouring infection. The disturbances in nutrition and assimilation and the consequent promotion of infection might correspondingly involve the hormonic or endocrine relationships of the tissues to each other.

The Pathological Basis of the Subject of Vitamins.

The most important outcome of the record of their work now presented in this discussion by Colonel McCarrison, Dr. Cramer and Dr. Findlay, was their striking demonstration of pathological changes produced by vitamin deficiency.

These changes were of three kinds:—

(1) The gastro-intestinal changes of congestion and catarrh, produced in the digestive tract.

(2) The atrophy of lymphoid tissue of the intestinal mucosa.

(3) The striking aplastic changes produced in the bone-marrow.

(1) *Gastro-intestinal Changes of Congestion and Catarrh Produced in the Digestive Tract.*

(1) The gastro-intestinal changes showed clearly how this vitamin deficiency, quite apart from its action in disturbing nutrition and assimilation, could be a great causal factor in disease (*a*) by producing or by favouring the production of definite pathological conditions of catarrh, congestion, inflammation, ulceration and other changes in the intestinal tract, especially mucous colitis; or (*b*) by producing marked changes in the blood-forming tissues, viz., atrophy of lymphoid tissue in the intestinal mucosa (vitamin B), or, as shown by Dr. Findlay, equally pronounced aplastic changes in the blood-forming bone-marrow (vitamin C).

(2) *Changes in the Lymphoid Blood-forming Tissues.*

The series of pathological changes described, to which he attached a special interest and importance, were those described by Colonel McCarrison and Dr. Cramer as taking place in the lymphoid tissue, viz., a progressive atrophy in the lymphoid tissue, and the blood-condition noted by the title of progressive lymphopenia (deficiency in lymphocytes).

These two pathological changes, among many others of a more striking and obvious nature, might seem to be side-issues in the total effect produced by vitamir deficiency—to be altogether subordinate to the more glaring catarrhal, congestive and inflammatory changes in the mucosa already referred to. This would appear to be their character in the minds of those to whom the term "intestinal mucosa" mainly conveyed the picture of glandular epithelium, supported by a layer of tissue chiefly lymphoid in character. But in actual fact the character of this lymphoid tissue had an entirely different significance. It really constituted three-fourths or more of the structural and functional efficiency of the whole intestinal mucosa, in connexion with the two great processes of nutrition—absorption and assimilation; it might be termed the first line of defence against infection from the digestive tract.

Any causal factor specially affecting this large mass of lymphoid cells was thus in his opinion invested with an altogether special importance, when this could be established by definite pathological changes and blood changes, as distinct from mere clinical assumption. It was in this relation that the data supplied by Colonel McCarrison and Dr. Cramer seemed to him to be so valuable. Their observations regarding the atrophy of the lymphoid tissues and the existence of lymphopenia as a feature of this deficiency in vitamin B, denoted the direct influence of vitamin B on the blood-forming tissues responsible for the production of lymphocytes in the blood (constituting 2,000 of the total 7,500 leucocytes per c.mm. in the blood).

(3) *Changes in the Bone-marrow Blood-forming Tissues.*

This evidence of direct action of vitamin deficiency on the blood-forming tissue was independently confirmed by the even more striking changes recorded by Dr. Findlay in connexion with the action of the vitamin C on the other great blood-forming tissue—the bone-marrow, the tissue concerned with the formation of not only the red cells but also equally with the formation of the other great group of leucocytes in the blood, viz., the polymorphonuclear leucocytes (5,000 out of the total 7,500 leucocytes).

The marrow changes produced experimentally by withholding vitamin C were, in his opinion, the most striking and convincing pathological evidence he had seen adduced as to the vital importance of this class of bodies in health and disease.

The marrow changes showed a replacement of the active blood-forming erythroblasts and polymorphic leucoblasts of the red marrow by a pale gelatinous aplastic tissue.

VITAMIN DEFICIENCY IN RELATION TO INFECTION.—GASTRO-INTESTINAL CHANGES.

Colonel McCarrison's detailed pathological and experimental studies with vitamin B clearly demonstrated how the pathological conditions produced in the intestinal canal by deficiency in vitamin B might well undermine the resistance of the intestinal mucosa to infections, such as amœbic dysentery, already present, but previously dormant. These pathological and clinical conditions, producible by vitamin deficiency, now described were pre-eminently fitted to lower resistance to infection.

Dr. Cramer's results supplemented those obtained by Colonel McCarrison, by showing how this lowered resistance was also clinically manifested by the fall in temperature, the loss of appetite, the want of tone, with the delay of the passage of food along that tract—all these fitting in with the distension and atrophy described

by Colonel McCarrison—the impaired absorption of food, and ultimately the inanition and marasmus which followed deficiency in vitamin B. The connexion between the two was shown by the fact that all of them rapidly disappeared on the administration of the vitamin concerned.

Their degree, as described by Colonel McCarrison, indicated clearly to his (the speaker's) mind that while the earlier catarrhal and congestive changes might well be caused by vitamin deficiency alone, the later inflammatory, and even in some cases the ulcerative, changes presented were essentially those of an infective character, most likely streptococcal, generally dormant, but now awakened into activity by the lowered resistance produced by the vitamin deficiency.

The interesting point in these changes was that the gastro-intestinal changes described were closely similar to, and even identical with, the character of those gastro-intestinal changes—the varying conditions of which he termed "septic gastritis," "septic enteritis," and "septic colitis." These were familiar results of the commonest non-specific pathogenic infections in the intestinal tract, viz., those associated with septic conditions in that tract. The infection referred to was streptococcal infection, which in his experience, played the greatest part in the pathogeny of the infective troubles so common in the digestive tract.

The possible relations of these two sets of changes—those produced in the first instance by vitamin deficiency, and those due to subsequent streptococcal infection, were thus of great importance with regard to the intestinal changes.

Here, again, the question presented itself—how much of this effect was attributable to the vitamin deficiency itself, and how much to the infective changes thereby favoured.

Similarly, with regard to the atrophy of lymphoid tissue and lymphopenia, his own studies showed that a definite and sometimes extreme atrophy of lymphoid tissue, and lymphopenia, were among the commonest effects produced by streptococcal infection. This effect could be illustrated by direct figures of the degree of lymphopenia, viz., in some of his cases as low as 400 lymphocytes per c.mm., instead of the normal 2,000 per c.mm.—a fall to 20 per cent.

But it was the bone-marrow changes that appealed to him with an altogether special interest. They were in character identical with the aplastic bone-marrow changes so well known to him as being commonly associated with the type of anæmia generally termed aplastic anæmia. The degree of aplasia possible in severe cases of anæmia he could express in actual figures of the numbers of the various elements of the blood he found, as in one of the last severe cases he had seen—viz., red cells as low as 500,000 per c.mm. instead of the normal 5,000,000 (a reduction to 10 per cent.); polymorphonuclear leucocytes as low as 450 instead of the normal of 5,000 (or only 9 per cent.); and lymphocytes only 400, or 20 per cent. of their normal number of 2,000.

But the further interest he attached to these marrow changes arose from another circumstance connected with his own studies regarding the ætiological nature of the type of anæmia presenting these aplastic changes in the bone-marrow. This interest consisted in the fact that the ætiological factor underlying the aplastic anæmia was in all cases streptococcal sepsis, chronic, latent and obscure, but always discoverable when looked for. The relation between the two was so close in his observation, that it was this form of anæmia that he had differentiated and distinguished by the name of *septic anæmia* (1903).

Here, then, in relation to the lymphoid and marrow changes, as well as in relation to the character of the gastro-intestinal changes described by Colonel McCarrison, he was struck with the close resemblances between the changes found in vitamin deficiency and those found in chronic intestinal streptococcal sepsis. The resemblances were so great as to suggest that if the vitamin deficiency could initiate the changes described, the ultimate result of these changes would be to favour

infection with streptococcal sepsis, and thereby invest the initial predisposing factor of the vitamin deficiency with an entirely aggravated severity.

The marrow change produced by vitamin deficiency might suggest that the type of anæmia associated with streptococcal sepsis, to which he had given the name of septic anæmia, might itself be attributed to vitamin deficiency. He was quite certain, however, that this septic anæmia and its aplastic marrow changes could be produced by streptococcal sepsis quite apart from vitamin deficiency.

The severest cases he had met with had been seen in patients in the best possible social circumstances, and the best conditions of life, with everything conceivable as regards quality or quantity of food, and the capacity for enjoying it.

It was certain that two of the chief manifestations of vitamin deficiency in the class of subject in whom it played the greatest havoc, viz., children improperly fed, and insufficiently fed, were the prevalence of the digestive and intestinal disturbances and catarrhal conditions described by Colonel McCarrison, and the pallor which was so marked an accompaniment of the inanition and marasmus presented by them. Any prominent anæmia in children had always, in his opinion, the character of the septic anæmia he had described.

The part played by vitamin deficiency in favouring gastro-intestinal infection which sooner or later invariably accompanied it, might well be explained by the observations of Colonel McCarrison. These observations revealed the action of the vitamin deficiency in creating catarrhal conditions always marked by excess of mucus secretion. This mucus was deficient in the protective and vitamin qualities normally possessed by it of resisting bacterial invasion (vitamin qualities, such as those shown by the tears in xerophthalmia); and this deficiency in its turn favoured the invasion of the intestinal mucosa by the most pathogenic organism that happened to be present, e.g., streptococci, or *Bacillus coli*, etc.

That organism might in tropical countries be some specific organism such as that possibly underlying beri-beri, or amœbic dysentery, or some other intestinal infection—sprue, for instance. But in this country, as indeed in all countries, the prevalence of streptococcal sepsis and its potentiality for causing manifold infective disturbances, far exceeded anything connected with any one class of intestinal infections such as those referred to. He (Dr. Hunter) therefore fully agreed with the conclusion arrived at by Colonel McCarrison that vitamin deficiency must be regarded as an important factor in the causation of common gastro-intestinal disorders of mankind. This it effected by creating pathological conditions of the mucosa of the gastro-intestinal tract, which thereby seriously hampered nutrition and assimilation.

The catarrh thus set up diminished the resistance of the intestinal mucosa to pathogenic organisms in that tract—amongst which the streptococcus was by far the most pathogenic and important, and the sepsis that ensued might well be, in children especially, the combined result of vitamin deficiency and this infection.

Dr. Robert Hutchison.

I agree with Dr. Cramer that we must limit the use of the term vitamin for the purposes of this discussion to the three accessory food factors A, B and C. If we once extend it, as Dr. Leonard Williams would have us do, to mineral salts, catalysts, enzymes, and chlorophyll, not to say violet rays and electricity, we shall land ourselves in hopeless confusion. Now we are asked to believe that a deficient supply of these specific vitamins in the diet is a cause of widespread disorders of health, and the evidence which has been brought forward by the openers of the discussion in support of this proposition is mainly experimental. As to this I should like to point out that it is very difficult to be sure that disturbances of health in animals fed on avitaminous diets are due solely to the absence of vitamins. Such diets are usually very abnormal in other directions as well. Some of them are

"synthetic" diets; others—as one of the speakers has admitted—are deficient in proteins of high biological value or in a proper balance of the inorganic salts. Colonel McCarrison, for instance, tells us that his monkeys were taken from the Madras jungle and fed on an autoclaved diet and that in these circumstances they developed various gastro-intestinal disturbances. But let us suppose the position reversed and that Colonel McCarrison is turned loose in the Madras jungle and fed on a diet of raw fruits and vegetables which is rich in vitamins. Is it not possible that in these circumstances he also might develop "a severe intestinal flux"?

Putting aside this objection, however, and granting for the sake of argument that the disorders mentioned are usually due to deficiency of vitamins and nothing else, is there any reason to suppose that comparable disorders are being produced to any extent from a similar cause in the ordinary community? I doubt it. I entirely agree with Dr. Cramer that one can draw no hard-and-fast line between specific and non-specific disturbances of health due to vitamin deficiency, but that the former are only the final links in a pathological chain of which ill-defined disturbances are the beginning. But it seems to me incredible, if, as the protagonists of the theory would have us believe, large numbers of persons are suffering from the early and non-specific disturbances, that none of them ever reaches the final link of the chain and exhibits the specific disturbance in its full-blown form. That this is true of this country at least cannot be denied. I have myself, for instance, in twenty-five years' experience at the London Hospital, never seen a case of adult scurvy and never one of beri-beri unless it had been imported from the East. Yet we are asked to believe that a deficiency of the antineuritic and antiscorbutic vitamins in the diet is an important cause of vague ill-health in the community! We may as well be told that in a community in which poverty is widespread no one ever goes bankrupt.

As regards vitamin A the case is a little different. Deficiency of this factor does not so much culminate in the production of a specific disease like beri-beri or scurvy (unless we regard rickets as such, and I regard the case for rickets being a deficiency disease as still *sub judice*), but in interference with growth and development and with the power of resistance to certain infections. That impaired growth and low resistance to infections may sometimes be due to deficiency of vitamin A it is difficult to disprove, but I think there is more evidence that when such disorders occur in any class of the community they are due rather to a deficient total caloric value in the diet during the years of most rapid growth than to deficiency of a vitamin which, after all, is so widespread in foodstuffs, and so resistant to the effects of cooking, &c., as vitamin A. The disease xerophthalmia which we know to result from a deficiency of this vitamin in the diet, is, after all, a rare one in this country, and I cannot help thinking it would be commoner if our diet was really lacking to any extent in the fat-soluble vitamin. This is the true inference from the Denmark experience referred to by Dr. Cramer.

In conclusion, I agree with the late Professor Emmett Holt when he says:—

"The physician can fall into no greater error than to assume that vitamin deficiency is the chief cause of all chronic nutritive disorders; just as not very long ago these were all referred to auto-intoxication. The use of vitamins without definite indications will be popular for a period, but like other fads it will pass. With time and further investigation their true place will be determined."[1]

Dr. M. J. Rowlands

remarked that Dr. Hutchison indicated that he had no use for experimental work, but he (the speaker) did not know that without it any advance would be made at all. He had himself experimented largely with an animal which was akin to man in some respects—the pig. If what Dr. Hutchison said was true, he, as a farmer, was at a loss. One of the results of deficient vitamins which he regretted

[1] "Food, Health and Growth," p. 204.

had not been brought into this discussion was infection by the tubercle bacillus. Anyone was at liberty to come to his farm and verify his statements. He had been able to eliminate tuberculosis from herds on his farm which had hitherto been full of that disease, as shown by many post-mortem examinations. His only method had been by feeding the animals on foods known to be rich in vitamins A, B and C. A confirmation had been furnished only that morning. He had a farm which he had rendered quite free from tuberculosis, as a result of carrying out thirty to forty post-mortem examinations weekly—and he did not think that number was equalled, even at the London Hospital. Every animal was watched carefully. He sold this tubercle-free farm, and within two years it was again badly infected with tubercle. That morning he heard that the birth-rate on that farm had now dropped below five, whereas when he had it the rate was over twelve. He agreed that temperature was an important matter, and on his farm he was not troubled with the usual epidemic pneumonia.

With regard to cows, unless cows were fed on vitamin-rich diet, bovine tuberculosis would increase among the children. He had been able to eliminate tuberculosis in cattle by feeding them on foods known to be rich in vitamin A and vitamin B, still keeping the animals, both pigs and cattle, in the natural state out of doors. But the latter without the appropriate food did not prevent the disease. He had now a thousand animals and he did not believe one was tuberculous. The relation of bovine tuberculosis to bone and gland disease in man was a very important one; and he had no doubt about the intestinal trouble which had already been demonstrated. He had recently demonstrated, by examination of stools, that animals fed on vitamin-rich foods absorbed more of their foods than did animals which were not so fed.

Dr. H. Scurfield

said he considered there existed plenty of evidence of derangements of health of a widespread extent, which might be due to deficiency of vitamins in the diet. Two of the openers had pointed out that intestinal stasis was one result of vitamin B deficiency, and practically the whole nation suffered from constipation. It made a great deal of difference to the teeth according as to whether the right or the wrong kind of fat was eaten when the dentine and enamel were being laid down; and most of us suffered from rotten teeth. Rickets, too, was probably mainly due to defective diet. It was a common thing during the war to hear comparisons made between the well-developed colonials and our stunted town-dwellers. The figures for 1923 showed that there were 80,000 recruits examined for the Army, and 50,000 of them rejected. We had nothing to boast about in the matter of physical efficiency.

We suffered from all kinds of infections, and there was abundant evidence that some infections might be warded off by taking a better dietary. Col. McCarrison had shown that native races who lived on dairy products, fresh fruit and vegetables, were remarkably free not only from intestinal diseases but also from cancer.

Dr. F. Parkes Weber

said that Dr. Hutchison remarked that he had not seen the end-results of avitaminosis arrive among the out-patient class in East London. As physician to the German Hospital, he (the speaker) could, he believed, say that the terminal stages did arrive, and often in the form of tuberculosis, and in death from various acute infections. Ordinary bad hospital cases could be divided, roughly, into two classes. In the first were patients with digestive disturbances, associated often with visceroptosis, &c.—miserable people, some of them with "muco-membranous colitis." They all suffered from chronic constipation. The other class included muscular, corpulent, plethoric patients, who died from such causes as cerebral hæmorrhage and degenerative diseases of the circulatory system. These took excess of food, and whether rich or poor in vitamins this large intake was often their undoing.

He asked Dr. Findlay what would be likely to occur in his animal experiments, in which he had induced the gelatinous type of degeneration of bone-marrow, if suppuration in some part of the body likewise occurred. He asked because, when a man (or animal) developed an abscess, or a suppurative catarrh of mucous membrane, he got rid of an enormous number of white blood-corpuscles of bone-marrow origin. Would a kind of aplastic anæmia occur under such circumstances? A gelatinous bone-marrow would not be able to make good the enormous quantity of leucocytes lost in the process of suppuration.

In cases of myeloid leukæmia, in which the myeloid type of leucocyte might consist in enormous excess in the blood, an acute infection sometimes occurred, followed by the rapid reduction of the number of white cells in the blood, heralding the approach of death. He wondered whether in this respect vitamin-deficiency would have a similar effect to that of an infection.

Had the influence of avitaminosis been tried in fowls experimentally infected with so-called "fowl-leukæmia" (Ellermann and Bang)?

Mr. Aslett Baldwin

said the discussion had been very gratifying to him, because a year ago, when he was President of the Sub-Section of Proctology and there was a meeting of Presidents of Sections to decide on subjects for discussion, he suggested this present subject be debated in conjunction with the Section of Medicine, but the suggestion did not find favour with the President of that Section, and the President of the Society considered it too large a subject to be tackled. But he (the speaker) thought and still thought that it was one of the most fruitful debates the Society could engage upon.

Dr. Hutchison had just referred to Colonel McCarrison's experiments with wild monkeys, but he only stated a half truth, viz., that monkeys fed by Colonel McCarrison on autoclaved foods developed intestinal flux, suggesting that Colonel McCarrison should try his own luck from living in the forests. But Dr. Hutchison did not mention that monkeys which were fed under the same conditions, except that their food had not been autoclaved, remained well. The intestinal flux was genuine amœbic dysentery—blood, mucus, and the *Amœba histolytica* were passed. The healthy monkeys had the same amœbæ in their fæces; thus proving they were protected against the disease by the vitamins in their food. Dr. Hutchison also said he had never seen scurvy or beri-beri in an adult in the out-patient rooms of the London Hospital. Was not that because it was difficult to find a person in London who was absolutely deprived of vitamin-containing foods?

He had been keeping a goldfish for eight months, and it had been fed on the crumbs from whole-meal bread. It remained well. He had friends who attempted to keep goldfish, but the goldfish had soon died, and he ascertained that they had been fed on the crumbs from white bread.

Sir StClair Thomson (President)

said that this had been a most interesting discussion, because all were agreed that it was better to discuss a subject without arriving at a conclusion than to arrive at a conclusion without having discussed it.

The remark of the last speaker reminded him of a little vitamin demonstration which he had seen only the previous day, and which interested him very much He was working at the sanatorium at Midhurst, where, of course, the windows were always wide open, and as soon as the patients left the room after having their meal, the sparrows flocked in and over the table, helping themselves to the remains. During the war there was great difficulty in getting patients to eat margarine instead of butter. He spoke to Dr. Bulloch, the pathologist, who said that, chemically, margarine was as nourishing as butter. He remembered that the Governors of

the Bank of England had margarine at lunch, but it was introduced stealthily. He therefore thought that as the Governors of the Bank of England had margarine, and it was said to be as good as butter, it was a case for a firm hand, and the Midhurst patients were induced to eat it. Now the sparrows which flocked in to the dining-room would not touch the margarine, but they quickly ate any butter available, so that it was necessary to cover the butter up! He left the incident for the Fellows' consideration.

Lieut.-Colonel R. McCarrison (in reply)

said he thought the reference by Dr. Hutchison to his experiments on monkeys had been sufficiently dealt with by Mr. Aslett Baldwin.

He wished particularly to refer to a point which was mentioned by Dr. Scurfield, who said he (the speaker) had not noticed cancer in native races living on a lacto-vegetarian diet. That was an important statement concerning which there was a good deal of confusion. What he had seen and what he had said was that cancer and chronic gastro-intestinal diseases were very uncommon among certain races in the extreme north of India whose diet was largely lacto-vegetarian and rich in vitamins. He had not generalized in regard to this, because his experience did not go beyond those particular races. It seemed to him it was not a question of civilization versus want of civilization, nor of native races versus western races. For instance, cancer was, he understood, as common in the people of Bengal, whose food was very faulty, as it was in this country. What was of importance was that the food should be well-balanced and rich in vitamins and other essential substances. And he also thought that in the present state of our knowledge—or of our ignorance—it would be as unwise to disregard the possible influence which faulty food might have as a predisposing cause of gastro-intestinal cancer as it would be unwise to assert, at present, that faulty food was the cause of gastro-intestinal cancer. The wisdom of a very wise man, whom all loved, had been mentioned, and perhaps it might be permitted him to refer to some other wise statements. One was in a recent report of the Medical Research Council, wherein it was stated that without vitamins normal metabolism was impossible. The other was from the Book of Ecclesiasticus: "The Lord hath created medicines out of the earth; he that is wise will not abhor them." He (the speaker) knew of no medicine created out of the earth on which normal metabolism was more dependent than on vitamins, and the wise man would not abhor them.

Dr. W. Cramer (in reply)

said he had found a distinct difference in the matter of susceptibility to tuberculosis in the animals kept on a vitamin-rich diet in comparison with those not so kept, and he thought there should be more experiments on animals normally susceptible to that disease.

Dr. Hutchison, in his speech, had questioned the whole basis of the vitamin conception, as he said the effects observed might be due not only to vitamin deficiency but to foods of low biological values, deficiency in salts, &c. But it must be remembered that the whole basis of the experimental evidence was that one might give everything necessary—proteins of full biological value such as casein, fats, carbohydrates and inorganic salts in their proper proportions—except the hypothetical substances under discussion, and the animal died and showed specific lesions. If animals in that condition were given a small amount of butter-fat or yeast, a dramatic change was seen to come over the whole animal in two or three days.

Dr. Hutchison had said he could not accept the idea that the people suffered from this vitamin deficiency because he did not see the terminal stages. But if a child was kept on white bread, margarine, jam, and boiled whiting, and developed pneu-

monia, it was called pneumonia; if it developed otitis media, it was given that name, and the condition was not appreciated as being the result of vitamin deficiency.

The reason xerophthalmia had been called a specific vitamin deficiency condition was that the rat could not be examined as carefully as the human subject, and it was the most obvious thing to see. The first experiments on animals were made by men who were not clinicians; they found this lesion and took it as a criterion. But not every rat developed xerophthalmia; many of the rats died without having shown it.

Dr. Hutchison had referred to the condition of Denmark. That was an agricultural country, and its main industry was the production of vitamin A. In such a country there could be produced a deterioration of the general health of the people because of the exportation of too much vitamin A stuffs. Were not the same economic factors operating in this country? It was not a question of better housing, or more air, or more light, because when by rationing butter the whole population was supplied with vitamin A, this particular ill-health disappeared. Xerophthalmia and beri-beri did not appear because people received just sufficient vitamin A and vitamin B to prevent them getting into the terminal stages, and only a very little would ward off the terminal stages.

The very object of the experiments adduced in opening the discussion had been to show that lesions occurred on diets which never led to the terminal stages, and that therefore the non-appearance of beri-beri and xerophthalmia could not be taken as a proof that the people were receiving enough vitamins.

Dr. G. M. Findlay (in reply)

said that though Dr. Hutchison, in his long experience, had not seen an adult case of scurvy in this country, he (the speaker) had been more fortunate, for though he was not a clinician he had seen two cases of adult scurvy in Scotland.

A few years ago he had the opportunity of investigating an outbreak of pneumonia and conjunctivitis in an industrial school near Edinburgh. In that school, from 1902 onwards, there had been outbreaks of acute atypical pneumonia among the boys. Before the war an outbreak was investigated by Dr. Macneil and Dr. MacGowan, who thought there might be overcrowding. Fresh dormitories were therefore built, but this did not stop the outbreaks. The diet for the school had been drawn up by Professor Haldane. The conjunctivitis had not been stopped by bathing the eyes with antiseptic lotion. He (the speaker) was called in, and he found that, in order to save expense, the whole of the food was placed in huge cauldrons which were screwed down, the contents being stewed for three to four hours. On his recommendation, cod-liver oil and raw swede-juice were supplied. There had been no outbreaks since, nor any isolated cases of the kind.

With regard to the question of tuberculosis, which Dr. Rowlands had raised, a few years ago he (the speaker) carried out experiments on guinea-pigs and rats fed on a diet deficient in vitamins A, B, and C, and inoculated with tubercle. With regard to the amount of tubercle present he could not come to any very definite conclusion, but what he found was, that the tuberculous animals which had been fed on a diet deficient in vitamin, particularly vitamin A, died much more quickly than other animals from intercurrent infections. A condition of tubercle plus a diet deficient as regards vitamins, resulted in a much more rapid death.

In answer to Dr. Parkes Weber, as to the extent of suppuration present in animals with gelatinous degeneration, as a rule these animals appeared clinically to be healthy, but they were neither so well grown as others, nor so active. There were, however, no gross signs of inflammation to be seen at death.

The Royal Society of Medicine.
President—Sir StClair Thomson, M.D., F.R.C.S.

SPECIAL DISCUSSION ON ENDOCRINE THERAPY.

Dr. Langdon Brown.

The subject of our discussion this afternoon is endocrine therapy and not endocrinology. The latter, though hampered by uncritical and extravagant hypotheses, has been making advances by the purely clinical method defined by Christian as the construction of a symptom-complex which corresponds to pathological changes observed in one of these structures, and by proving the constant association of this symptom-complex with such changes. I may instance the recognition of acromegaly, Fröhlich's syndrome, Addison's disease, and virilism or sexual precocity from disease of the adrenal cortex, as products of this method which have been obtained without any aid from laboratory experiment. But endocrine therapy, with the brilliant exception of insulin, has not kept pace with the increase of our knowledge of the endocrine system. In one sense this has been as well; for the way in which extracts were often ignorantly given would inevitably have done harm had they been potent. The damage that an injection of secretin can do to a fasting animal illustrates this.

Endocrine therapy may have as its object the replacement of a deficient internal secretion. With thyroid extract and insulin we are here on sure ground; with parathyroid and pituitary extract on less certain ground; with the remainder on very doubtful ground. But it may also have as its object the employment of some extract simply for its pharmacological action, such as adrenalin for its local vaso-constrictor effect or for other effects involving its stimulating action on the sympathetic, as in asthma. Pituitrin may similarly be used for its stimulating effect on plain muscle as in paralytic distention of the bowel.

I should also include under endocrine therapy methods that have as their object the use of other drugs which, by their influence on the endocrine glands, enable those glands to carry out their normal function more satisfactorily. This measure may have a future of considerable promise, for substitution therapy can never exactly imitate the steady drip of a hormone into the circulation, but at present the only definite example is the use of iodine medication in diseases of the thyroid; though it seems probable that vitamins may be exploited to that end.

Next we may consider methods of administration:—

(1) *Oral Administration.*—Organo-therapy won its first great triumph with thyroid extract, and there was considerable disappointment because other glandular extracts failed to give commensurate results. It was not realized that thyroid secretion originally entered the alimentary tract by the thyroglossal duct, which may account for the ease with which it is absorbed from that tract. This probably applies to the parathyroids also. Then the anterior lobe of the pituitary is an outgrowth of the alimentary tract, though before the respiratory tract had been separated from it. This might lead us to expect that oral administration of pituitary extracts, at any rate if prepared from the anterior lobe, might be effective, though here the results are much less constant. Hamill has, however, observed uterine contractions produced in the cat by administration of pituitrin through the stomach tube after ligature of the pylorus. He has also seen post-partum oozing of blood from the uterus checked by oral administration of pituitrin, to return when the drug was discontinued and to cease when it was resumed. In a case of chronic intestinal atony I first injected pituitrin with good result, and later on gave it by the mouth,

with the same result. I omit cases where the element of suggestion could have come in, but it would appear that oral administration is occasionally effective, though it is difficult to explain the inconstancy of the result. The effect of the oral administration of other extracts is, to say the least of it, very uncertain, apart from the local effect of adrenalin on the gastric mucosa, as in the control of hæmatemesis and of vomiting. Blum, however, has reported reduction of the blood-sugar from lingual administration of the hydrochlorate of insulin, though two to three times as large a dose was required. For the rest, we do not know whether the active principle can escape the action of the digestive ferments.

(2) *Nasal Administration.*—It was suggested by Blumgart that as the pituitary is more strictly an outgrowth of the nasal mucosa, its extracts might be more easily absorbed when sprayed into the nose or from plugs of cotton wool soaked in a solution of the extract inserted high up into the nasal cavity. This method has met with a fair degree of success in the treatment of diabetes insipidus by pituitrin. It is also believed by some observers that adrenalin may be absorbed by this route, for they have seen effects on the blood-pressure after free application of this drug to the nasal mucosa as a preliminary to operations there for its local vaso-constrictor action.

(3) *Rectal Administration.*—Very little is known of this method, but Stewart and Rogoff report reduction of blood-sugar by large doses of insulin given with blood, serum or water *per rectum.*

(4) *Subcutaneous or Intravenous Injection.*—It is clear that adrenalin, pituitrin and insulin can readily produce powerful effects when given in this way.

(5) *Grafting and Transplantation Methods.*—These are still on their trial. It would appear that such grafts are treated by the organism as foreign bodies and become absorbed, so that their effect is only temporary. It may be that a graft is more likely to take root if it is inserted into an homologous tissue, such as the testicular grafts of which so much is heard to-day. Hurst was able to get an adrenal graft to take and to remain effective in the testis for over two years; and it seems to me that this might be associated with the fact that the interstitial cells of the testes and the cortex of the adrenal have a similar origin.

It would add very much to the accuracy of our observations on endocrine therapy if biochemical tests could be devised to demonstrate the effects of an extract. At present we have two such—the effect on the basal metabolic rate and on the blood sugar. These have already yielded valuable results, particularly in diseases of the thyroid and in diabetes respectively, but are probably capable of wider application. Kenneth Walker, for instance, has used them in studying the effects of grafting operations. I look forward to the introduction of biochemical tests on the lines of those employed for investigating hepatic efficiency. Loewi's adrenalin eye-test, Goetsch's intradermal test, the diastase test, and tolerance to quinine hydrobromide as employed by Bram, are probably capable of modification so as to yield objective evidence of the efficacy of an endocrine preparation. Vines has assayed the activity of parathyroid extracts by their power of destroying guanidine, and their therapeutic effect by estimating the calcium content of the blood.

Of late there has been some conflict between the physiologist and the clinician on endocrine therapy, which is due to the laboratory worker not realizing the urgency of the problem a sick man presents, and to uncritical enthusiasm, and, I may add, undue suggestibility on the part of the observer at the bed-side. The physiologist can select his problem and can make use of rigid controls. The clinician has his problem thrust upon him and has to attempt a solution without the aid of proper controls. Moreover, in the artificial world of the laboratory the selected problem is deliberately reduced to its simplest terms, as, for instance, by excluding a large part of the body from the circulation. Thus, a conception of blood-pressure

emanated from the laboratory which did not take into account the extraordinarily complicated adjustments which occur in disease. The physiologist is rather too apt to apply the results of laboratory experiments to man, although the striking difference between the effect of morphine on the cat and on man is before him, and although clinical evidence clearly shows that digitalis does not raise human blood-pressure. He told us that pituitrin was a diuretic, although under clinical conditions its anti-diuretic effect can be constantly demonstrated. But if the laboratory worker is not guiltless we must admit that we are the greater sinners when we use glandular preparations which cannot be shown to contain the active principle, or which may even come from a gland which contains no active principle at all, and then attribute the patient's improvement to the medicine. I am not condemning the use of extracts that have not, as yet, received laboratory sanction, for some of our methods must remain empirical, and, as Timme reminds us, "the abysmally ignorant South American savages who gave us quinine didn't even know its formula." But I am pleading for a more critical attitude towards our results. Indiscriminate polyglandular therapy has led some laboratory workers to challenge the very existence of inter-actions and antagonisms of the different endocrine glands in association with the vegetative nervous system, yet others of them have recently obtained results which support this conception. Thus Mackenzie Wallis showed that the blood-sugar curve in the glycosuria of pregnancy was the same as that of pituitary glycosuria; Dixon showed that the injection of ovarian extract caused an immediate secretion of pituitrin into the cerebro-spinal fluid; and J. H. Burn showed that the injection of pituitrin inhibited the fall of blood-sugar otherwise produced by insulin. The deduction seems justifiable that an ovarian hormone stimulates the pituitary to antagonize the pancreas, and Allen and Doisy (*Journ. Amer. Med. Assoc.*, 1923, lxxxi, p. 819), have prepared an ovarian extract which has produced the œstrual phenomena in oöphorectomized animals. Dixon and Marshall have more recently shown that the presence of a corpus luteum whether of œstrus or pregnancy inhibits the activity of an ovarian extract, but that this inhibitory effect terminates just before parturition begins. There appears to be at that time a sudden rush of pituitrin into the cerebro-spinal fluid, which, entering the circulation, helps to empty the uterus and to start the flow of milk.

The antagonism between adrenalin or thyroxin on the one hand, and insulin on the other, manifests itself differently. It is not the direct antagonism of pituitrin and insulin, but an indirect one through the liver, adrenalin and thyroxin tending to empty the glycogen reservoirs and to flood the blood with sugar, while insulin checks the conversion of protein and fat into glycogen and at the same time increases the assimilation of sugar by the tissues. Such observations as these have led Calvert to apply insulin to the treatment of hyperthyroidism, while others have used it in the treatment of non-diabetic acetonæmia. Its effect on fat metabolism certainly suggests that in combination with dextrose, insulin might have a field of usefulness for hepatic toxæmias such as post-anæsthetic vomiting. Cramer has also shown that insulin can prevent death in the experimental hyperpyrexia produced by tetra-hydro-naphthylamine through stimulation of the thyroid, adrenals and probably the anterior lobe of the pituitary. This is an observation worthy of clinical application. The antagonism between the adrenals or thyroid and the pancreas is also suggested by Loewi's test. On the other hand, the co-operation between pancreas and parathyroids is shown by the liability to glycosuria after parathyroidectomy, and by the calcium drainage resulting from defect of either of these glands.

We may fairly claim, therefore, that the theory of co-operation between the adrenals, thyroid and pituitary on the one hand and between the pancreas and parathyroids on the other, and of antagonism between these two groups, has a

considerable body of experimental support, as also for the view that the gonads and the sympathetic nervous system interact with the former group. But if polyglandular therapy has a rational basis one must admit that it is generally irrationally applied without reference to potency, dosage, or method of administration. It has been pointed out that most polyglandular preparations contain thyroid extract, and that the actually observed results may generally be attributed to its presence alone.

Agnosticism has been carried to the point of doubting whether the observed pharmacological actions of adrenalin and pituitrin have any relation to the functions of the structures from which these extracts are prepared. But I can hardly believe that the body is so ill-designed as that. It may be true, as has been urged, that diabetes insipidus is due to a disturbance of the hypothalamus, but it surely must be more than a coincidence that the posterior lobe of the pituitary, situated just beneath this nervous structure, yields an antidiuretic substance which is effective even on the denervated kidney. Surely it is more reasonable to conclude that the hypothalamus acts through the pituitary. The medulla of the adrenals is formed from sympathetic ganglion cells, and has on any structure the same effect as that obtained by stimulating the post-ganglionic fibres to it. Yet we are asked to believe in any connexion between these facts. Adrenalin relieves the asthmatic paroxysm because it dilates the bronchial muscles; so does the sympathetic, thus redressing the disturbance produced by an over-acting vagus. Does this not point to co-operation between adrenalin and the sympathetic? To attribute such facts to the long arm of coincidence is surely to display credulity similar to that with which we are taxed. It is scientific nihilism, in face of which I prefer to believe in the definite plan of the endocrine and vegetative nervous systems previously referred to. Otherwise the body of experimental and clinical observations that has been collected is meaningless. Accepting it we have a rational basis for endocrine therapy.

I would summarize my conclusions as follows: Thyroid medication has the widest application of all the endocrines, and is the easiest in practice. Thanks to Leonard Williams we know that the doses formerly prescribed were too large. A total daily dose of 6 gr. of the extract of fresh gland seldom needs to be exceeded, and it is wise to start with not more than ½ gr. three times a day. It is important to remember that the extract of the desiccated gland is five times as strong as that of fresh gland. It is worth while in children who fail to grow, who suffer persistently from nocturnal enuresis, who have night terrors or who suffer from relaxation of the articular ligaments, causing knock-knee, painful heel, flat-foot or lordosis, to look for stigmata of hypothyroidism, and, if they are present, to give thyroid extract. Whereas in children the thyroid is the great stimulant to growth, when this is complete its great function is as a katabolic agent. When it is in defect there is an accumulation of the products of incomplete combustion, which consequently infiltrate many tissues, including those of the central nervous system, the nasal annexes, the heart, the intestinal musculature and the ligaments, besides the well-recognized instance of the subcutaneous tissues. Parathyroid extract can, I believe, be absorbed from the alimentary tract, since I have seen its use followed by a rise in the calcium content of the blood. It has been advocated for the various results of chronic sepsis. I have found it of great service in tetany and gastric ulcer, but I should like to point out that even in chronic gastric ulcer the blood calcium is not always reduced.

Adrenalin is of little, if any, use in substitution therapy. It is of great use for its local effect on blood-vessels, as in local anæsthesia, epistaxis and gastrostaxis, and for its effect on plain muscle, as in œsophageal spasm and asthma. It is sometimes useful in vomiting by stimulating the inhibitory nerves to the stomach.

Pituitary extract may be absorbed by the stomach, but this is not constant. It is of doubtful value in substitution therapy, though it may occasionally alleviate Fröhlich's syndrome and adiposis dolorosa. Injections of it are very useful for paralytic distention of the bowel, for diabetes insipidus, as an oxytocic, and in hypoglycæmic collapse after an overdose of insulin. It is useful in asthma and as a temporary cardiac stimulant.

Insulin is of proved value in diabetes, and I should like to protest strongly against the statement that if it is once given it must be continued for the rest of life; especially as the public take this to mean that if it is discontinued the last state of the patient is worse that the first, which is emphatically untrue. Moreover, I have a number of patients who came under early treatment with it and who are keeping free from glycosuria with a normal blood-sugar, although insulin has been discontinued for periods varying from three months to over a year. Its use in certain other conditions is worth consideration.

The existence of an ovarian hormone I regard as proved, and it should not be beyond the wit of man to prepare a reliable extract. It is doubtful whether the present commercial preparations are active, but I have seen thymic asthma, a case of epileptiform seizures accompanied by temporary swelling of the thyroid, and transient glycosuria following double ovariotomy in a girl of twenty, greatly benefited. In neither case did the patient know what she was taking. It is important that the ovary from which the extract is made should not contain a corpus luteum.

I doubt whether any orchitic extract is effective; and though apparently striking benefits have followed grafting operations it is too soon to say how long they last.

Professor Swale Vincent.

(Middlesex Hospital Medical School.)

I imagine that there is now less difference between Dr. Langdon Brown's views and my own than there was two years ago; though I fear I shall still fall under his categories of agnostic and nihilist in regard to two or three subjects. My task is rather a thankless one, and I was reluctant to undertake it at the present time. It was, however, pointed out that a great deal will have to be said on the side of sanity in order to check the uncritical enthusiasm of the "clinical endocrinologist."

The present boom of endocrine therapy is an evil of the same general nature as the indiscriminate sale of patent medicines. But, whereas in the latter case it is the members of the general public who are so continually duped by the vendor of the drug, in the former we have the distressing spectacle of hundreds, nay, thousands of medical men in all parts of the world being cajoled into prescribing preparations which are nearly always of doubtful value and are frequently quite inactive in health or disease. The methods employed by the manufacturing druggists are based on the recognized value of widespread and lavish advertisement, accompanied by letter-press of a pseudo-scientific character advocating the use of the drugs. Sometimes the drug firm will publish a journal, looking at first sight rather like a genuine medical or scientific journal, to which by hook or by crook, medical men are occasionally persuaded to contribute. From time to time a member of the firm happens to be a medical man. Then, of course, he can the more easily become familiar with the jargon of the "clinical endocrinologist" and can write and distribute books to advertise his wares. The evil, I am told, is greater in America than in England.

Endless seems the wrangle as to the differences in method and outlook between the laboratory worker and the clinician. I am strongly of the opinion that there are, or should be, no essential differences either in general method, in mental attitude, or in the desire for truth. In the case of the clinician there is in the majority of instances perhaps even a more complicated problem to solve. This is all the greater reason why he should gird his loins and summon all his scientific acumen to deal

with it. In a recent paper Timme,[1] pointing out the action of adrenin on the heart, inquires whether clinicians ought to wait till they know *how* it acts before they administer it with the object of augmenting the heart-beat. He charges physiologists with saying "Yes, you should wait." Now this charge is unjustifiable, but any physiologist worth his salt would urge his clinical friend to wait, at any rate, until there was some reason to believe that adrenin does actually augment the heart-beat when given in the ordinary way. But since the fight is on, we must admit that there is sometimes a marked difference between the scientific training and insight of the practising physician and that of the most humble laboratory worker. It would indeed be hard to find anyone in the latter group who could perform such bad experiments and be so shameless in the publishing of them as some of the writers on therapeutic subjects (especially endocrinological) whose papers appear by scores in our medical journals.

We meet with advertisements of extracts of blood (hæmoglobin), bone-marrow, muscle, spinal cord, brain, lymphatic gland, spleen, prostate, ovary, testis, mammary gland, duodenum, pancreas, liver (bile), placenta, gastric mucous membrane and kidney, to say nothing of the adrenal bodies (both cortex and medulla, and the whole gland); thyroids, parathyroids, pituitary body (both anterior and posterior lobes and the whole organ), the pineal body, and the corpus luteum. Various mixtures of these extracts are also put upon the market and are presumably sold, as, e.g., ovary and mammary gland extracts, mixtures of extracts of lymph gland, testis, spinal cord, and brain; combinations of thyroid and pituitary substances, and of thyroid and adrenal extracts; such combinations as "polyglandin," a mixture of thyroid, parathyroid, ovary, testis and pituitary extracts. Several of these combinations are sold with or without the addition of other drugs, under various fanciful names, as "virilogen," "mammagen," "ferovarin," &c.

Now in the case of the great majority (in fact, in all except one) of these preparations there is no evidence that any effects whatever are produced when they are given in the ordinary way by the mouth, unless they are given in such quantities as to serve as foods. I have on a previous occasion expressed the opinion that "it would be difficult to mention a drug which is clearly and beyond doubt of value in the treatment of disease and yet which produces no known pharmaco-dynamical effects."[2] I imagine that few will be prepared to question this in the case of the great majority of ordinary drugs. If preparations of iron or the vitamins are given to replace these substances which are lacking in the diet, I suppose it might be that little or no effect would be produced in the normal individual. The same argument might be applied in the case of the active principles of the internally secreting glands, where these are administered in the course of a true "substitution therapy." In animals or in human patients suffering from an insufficiency of the active secretion good results might accrue, while in a normal individual little or no effect might be observed after the administration of the material in question. It has been stated that parathyroid grafts do not "take," unless a parathyroid deficiency has been brought about either by disease or operation. Further, it is believed by some observers that parathyroid feeding has an effect in diminishing an excess of guanidine in the blood and in promoting an increase of calcium salts in certain morbid conditions. However this may be in the case of the parathyroid, it must be pointed out that insulin, the active principle found in the pancreas and elsewhere, has a powerful effect on the normal animal. The same applies to the active substance of the thyroid. Another instance may be cited in the powerful principle of the chromaphil tissues, if, indeed, this may be looked upon as a true internal secretion. On the whole, then, it would seem probable that we are justified in regarding with

[1] *Endocrinology*, 1924, viii, p. 724.
[2] *Proc. Roy. Soc. Med.*, 1923, xvi (Sect. Therap. Pharm.), pp. 9-14.

great suspicion any report that a pharmacologically inactive substance is of therapeutic value.

Thyroid Medication.—Thyroid preparations are generally recognized as being among the most valuable drugs at our disposal. They bring about an increase of the total metabolism of the body, and their effect is stated to be more marked in cases in which there is a deficiency of the thyroid secretion. But it must be borne in mind that they produce prompt and easily-recognized effects when administered to healthy animals and human beings. It is by no means proved that Kendall's "thyroxin" represents the active principle, or at any rate, all the active principles. When tested physiologically by certain methods thyroxin is found to be less powerful than crude thyroid extract. Moreover, there is considerable doubt about the formula given for this substance. Some chemists are of the opinion that it would be impossible to synthesize a substance possessing such a constitution.

There is no need to emphasize the value of thyroid preparations in myxœdema and cretinism. In lesser degrees of hypothyroidism and in rheumatism, rickets, mental diseases and other conditions, the preparations may also be useful on the general principle that a drug which stimulates the chemical activities of the whole body may be applicable in a variety of morbid conditions.

Parathyroid Medication.—The results of parathyroid medication are still very uncertain. It is difficult to say exactly in what conditions one would expect it to be of use. Tetany, the result of disease or damage to the glandules, is of course, such a condition, but it is by no means clear that any form of tetany is connected with the parathyroids, except that which sometimes occurs after operation on the thyroid. Other forms of tetany are due to a great variety of causes, not connected, as far as we know, with the parathyroid bodies. Moreover, at the present time, it is exceedingly doubtful whether any commercial parathyroid preparation when given by the mouth will even relieve the symptoms due to injury to or extirpation of the parathyroid bodies.

The commercial preparations of ovary and parathyroid at present on the market are, according to the majority of competent observers, physiologically inactive, and in all probability useless as therapeutic agents. In the case of both these organs (as also of the pancreas) some special method of extraction is necessary in order to obtain a preparation containing the active principle. If we adopt the ordinary view about such active substances, the most obvious supposition would be that all of them must be soluble in, and extractable by, water. But some of them appear to be found in combination with lipoid substances, and it is necessary to break up this combination in order to get the substance into watery solution.

Collip,[1] of Alberta, has recently made what appears to be an extremely important preliminary communication on an active substance which he has isolated from the parathyroid gland. He claims to have extracted a principle which, on injection, would prevent tetany for thirty hours in parathyroidectomized animals. He has been able to keep one such animal alive for ten weeks by the use of the extract. He was also able to produce such a rise in blood-calcium that vomiting occurred, then weakness, atony, coma and death. The blood became so thick that it would not pour. He reports that dogs without either thyroid or parathyroid are more sensitive to the parathyroid substance. This he considers to be the first definite evidence of an antagonism between the thyroid and the parathyroids.

If there are any cases of tetany in the human subject due to deficiency of the parathyroids, it is reasonable to hope that the substance of Collip may be a useful remedy. We must, however, bear in mind that tetany in animals, due to extirpation of parathyroids, frequently disappears without active treatment, especially if the animals are kept on a milk diet. This I have observed in the case of the monkey on several occasions. Nor must we lose sight of the fact that tetany is probably

[1] *Science*, 1925, lxi, 156.

due to many different causes and that many different modes of treatment have been found to be beneficial.

Pituitary Medication.—It is still doubtful whether we ought not to ascribe the symptoms generally supposed to be due to lesions of the organ to injury to the hypothalamus. It has never been demonstrated in a satisfactory manner that treatment by pituitary extracts has any effect in remedying the symptoms thought to be due to pituitary insufficiency. Posterior lobe preparations are useful as drugs in the treatment of diabetes insipidus and in aiding the contraction of the uterus. They are also valuable in meteorism. But these uses have no relation to any known functions of the organ. They are not instances of a "substitution-therapy."

Adrenal Medication.—Adrenin, prepared and sold under various names, is a very useful drug. But it would be rash to affirm that this usefulness is directly related to anything we know about the functions of the adrenal body or the chromaphil tissues. Here, as in the case of pituitary extracts just referred to, there is no question of a "substitution-therapy." Our knowledge of the effects of extracts made from the chromaphil tissues, and our study of their chemical nature, have shown us the value of certain amines as drugs. We have now no further need of the adrenal body as a pharmacological agent. When we know the chemical nature of posterior-lobe substance the same may be said of the pituitary. Adrenal extracts and the various forms of adrenin and amines having similar chemical constitution, produce no effect of any kind in health or disease when given by the mouth. A possible exception may be instituted owing to the local action of adrenin on the lining membrane of the stomach.[1]

Testicular and Ovarian Medication.—Extracts of ovary and testis produce no noticeable effects in health or disease when given by the mouth. When administered subcutaneously the commercial preparations appear to be for the most part inactive, and, at the best, only produce a slight irritant effect, just as do extracts from all kinds of organs and tissues.

Allen, Doisy and others[2] claim that they have extracted the principal ovarian hormone by treatment with alcohol and ether. The residue from the ether extract is treated with acetone, which throws down an inactive precipitate. The filtrate contains the active substance, and after being once more taken up in alcohol and evaporated to dryness, the minute yield of oily residue is dissolved in corn oil for injection.

These observers have tested their extracts by careful observation of the effects produced by them on spayed animals. In 1917 Stockard and Papanicolaou[3] described a method for following the œstrual changes in the living animal. During the anabolic phase of the cycle in the guinea-pig, the rat and the mouse, the vaginal epithelium grows and a cornified layer appears. During the katabolic phase the outer layers of their epithelium degenerate and are removed by leucocytic action. These changes show a regular succession of cell types in the vagina, each characteristic of a certain phase of the cycle. Thus the microscopic examination of vaginal smears becomes an indication of the œstrual condition of the animal.

According to Allen and his co-workers, from one to three injections of the extract into spayed animals produce the typical hyperæmia of œstrus, hypersecretion in the genital tract, and the growth of the mammary glands. There is thickening and cornification of the vaginal walls and subsequent degeneration. Moreover the animals will admit the male during the artificial œstrus. It is stated further, that by means of injections of the extract, the young animals may be brought to sexual maturity from twenty to forty days before the usual time.

[1] Leyton states that adrenal extract given by the mouth will raise the blood-pressure in cases of Addison's disease, and Hitchcock has recently affirmed that adrenin, given by the mouth, raises the metabolic rate.

[2] *Amer. Journ. Anat.*, 1924, xxxiv, pp. 133-168.

[3] *Amer. Journ. Anat.*, 1917, xxii, p. 225.

No positive results were obtained with commercial extracts.

An active principle can also be obtained from the placenta.

The results obtained by Allen and Doisy have been fully confirmed by Wright, Dodds and Dickens working in the Physiological and Biochemical Laboratories of the Middlesex Hospital. They find that no active substance can be extracted from any of the commercial preparations which they have examined. The yield of active material is very small, so that it would necessarily be very expensive if put on the market. No effects are produced when the material is given by the mouth.

Pancreatic Medication.—Although it has long been recognized that the pancreas is responsible for an internal secretion, the failure of which produces diabetes in animals (and probably also in human beings), it is only within the last two years that a preparation was made from the pancreas by Banting and Best which lowers the level of the blood-sugar in normal and de-pancreated animals and in human beings suffering from diabetes. The literature of insulin is already enormous. It will only be necessary for our present purpose to deal briefly with one or two points.

There are now several different ways of preparing insulin, and it (or substances having a similar pharmaco-dynamical action) may be obtained from many organs and tissues, animal and vegetable, though in greatest amounts from the islets of Langerhans, in those species in which islet tissue may be obtained separately from the rest of the pancreas.[1] The substance, whatever yet may be discovered as to its chemical nature, appears (according to our present knowledge) to originate in the body chiefly from the modified pancreatic tissue forming the islets.

However great may be the theoretical difficulties, insulin ranks with thyroid substance as the means of instituting a very valuable substitution-therapy. The original preparation and most of the recognized modifications or variations of it must be given by subcutaneous injection. Several observers, however, claim that their special substances are effective when given by the mouth. Quite recently Murlin[2] claims to have demonstrated various ways in which insulin may be successfully administered by the alimentary canal.

Perhaps the most inexplicable series of experiments is that of Best, Smith and Scott,[3] and Baker, Dickens and Dodds.[4] The former workers showed that insulin could be obtained from the tissues of a dog which had been de-pancreated some weeks previously. Baker, Dickens and Dodds, in their study of the insulin distributed in the human body, reported an abundant amount of insulin in the tissues and pancreas of a patient who had died in diabetic coma. The independent observations confirm each other and clearly demonstrate that there is yet much to be explained in regard to the functions of the pancreas and the pathology of diabetes.

Summary.

If we are called upon to enumerate the cases in which it is possible to employ a true substitution-therapy—artificially to replace the internal secretion of a gland—we have as yet only two clear instances, those of the thyroid and the pancreas, and of these only one, the thyroid, is known to produce any effects when given by the mouth. Some observers claim that certain preparations of insulin may be efficacious when given in this way.

It may be that we shall before long be able to include the parathyroid and the reproductive organs in the same category, though, according to the best evidence, the preparations of these organs which are commonly employed are no use when administered by the mouth.

[1] Vincent, Dodds and Dickens, *Lancet*, 1924, ii, p. 115.
[2] *Science*, 1925, lxxi, p. 156.
[3] Best, Smith and Scott, *Amer. Journ. Physiol.*, 1924, lxviii, p. 161.
[4] Baker, Dickens and Dodds, *Brit. Journ. Exp. Path.*, 1924, v, p. 327.

There are, however, two striking instances in which substances, obtained from organs usually supposed to furnish internal secretions, are valuable as drugs, apart altogether from the question of internal secretion and substitution-therapy. These are adrenin and preparations made from the posterior lobe of the pituitary body. And neither of these has any effects when administered by the mouth.

Thus, out of the twenty-four preparations of organs and tissues in the list given above as sold by the manufacturing druggists and prescribed by the "clinical endocrinologists" to be administered in the ordinary way by the mouth, only one, the thyroid, is of proved value.

When a "clinical endocrinologist" allows his imagination to run riot and break away from the fetters of scientific discrimination, he may wander far from the path of truth, and may, if he has a plausible way with him, tempt many of his weaker brethren to follow in his footsteps. Physicians who continue to prescribe preparations for the use of which there is no scientific basis and no sound clinical evidence, are guilty of conduct which is unworthy of modern medicine.

Mr. Leslie Pugh.

The use of endocrine preparations in veterinary practice does not differ in any important particular from that which is customary in medical circles. Pituitary extract is employed largely in obstetric work, and thyroid extract in a variety of conditions, while there is a certain amount of literature dealing with the employment of other hormones either alone or in the form of mixed preparations. But it would be difficult to contend that veterinary practice can throw any important new light at the present moment on these therapeutic aspects of endocrinology. I therefore propose to limit my remarks to a consideration of certain naturally occurring disorders in which endocrine preparations have special use.

In some of these cases symptoms are apparent which human beings would not exhibit owing to moral and social restraints or for other reasons. Moreover, a diseased animal is usually slaughtered in a comparatively early stage of its disease, and opportunities are thus afforded which would be of very great value if it were possible to take more advantage of them. The difficulties which stand in the way can only be realized by those who have attended at the autopsy of a very large animal conducted by a comparatively ignorant man whose financial interests are strongly opposed to any process which might seem likely to lead to the detection of disease. Those whose practice lies mostly amongst milk cattle have naturally to pay great attention to the function of milk production. Several disorders of this function are known, and in connexion with some of them pituitary extract has been employed.

The general effect of pituitrin is that which is well recognized, namely, that the udder is emptied more quickly but that the total milk yield is neither increased nor diminished. Pituitrin is, therefore, no longer administered in the hope of getting any true galactogogue effect. But there remains, however, a curious condition in which it has at times proved useful. It sometimes happens that a cow which will readily yield an abundant supply of milk to the calf refuses to do so when milked by hand. In such cases pituitrin may be administered intramuscularly a short time before milking. It may need to be repeated, but the final result will be that a full supply is obtained by the usual hand milking. This condition, in which cows refuse to "let down" their milk, can only be described as a psychic disorder. It may equally be apparent when an unaccustomed milker makes his appearance. The act of milking, therefore, is not a purely mechanical process. It evidently requires some sort of unconscious co-operation on the part of the cow. This co-operation can apparently be made to be forthcoming by the use of pituitrin. I cannot pretend to explain these facts but they exemplify a use of this agent which may be of interest beyond the bounds of veterinary work.

Extracts of corpus luteum have not found any great use in animal diseases, but one is constantly brought into contact with this glandular body when treating cattle for sterility. A cow not infrequently suffers from uterine infection during the puerperium. In many cases she recovers spontaneously; in others, however, she may appear healthy but the normal ovarian cycle does not commence. Examination on several occasions reveals that the corpus luteum, instead of atrophying after a period of activity, persists, and this persistence prevents those uterine contractions that occur at the œstrus and which prove so helpful in eliminating retained discharges or infective material. The mechanical extrusion of the corpus luteum by rectal manipulation determines the onset of œstrus four or five days later, and if proper antiseptic measures are carried out at that time complete recovery occurs. If, however, the case is neglected, the chronic irritation, combined with the cessation of the ovarian cycle, leads in quite a number of cases to a well-defined endocrine upset, conforming in many respects to what the French would term "disfunctioning of the thyroid." This condition is associated with a cystic degeneration of one or both ovaries and the presence of the cysts determines the sexual abnormality of nymphomania.

There are profound psychic and physical changes in the affected animal. Ultimately, very definite masculine characters are developed and features somewhat suggestive of acromegaly can at times be observed. The administration of thyroid extract in the early stages of the disease has yielded beneficial results in combination with judicious treatment of the local changes. The most important part of local treatment consists in the repeated rupturing of the cystic Graafian follicles as soon as their presence is evidenced by the onset of nymphomania. There is the possibility that the administration of endocrine preparations might determine the normal rupturing of the follicles, but so far I have been unable to obtain this result other than by employing the mechanical interference already indicated.

I have attempted to bring about the expulsion of the mummified fœtus from the uterus by the use of pituitrin and other glandular preparations, but in every case I have been compelled to dislodge the corpus luteum of pregnancy before obtaining this result. It is of interest to recall that the changes which occur as the result of cutting out the action of the corpus luteum are similar to those that occur before normal parturition.

In attempting to treat a condition of cows associated with hyperglycæmia and ketosis by the use of insulin, I have on several occasions observed the symptoms portrayed by these animals when in a condition of hypoglycæmia. There is no time to elaborate in detail, but the following points may be of interest to those who have had to deal with hypoglycæmic shock in smaller animals and in man: (1) Pronounced thirst, hunger, frequent yawning, continuous licking at coat, legs and nostrils. (2) Later, continuous chewing movements of jaw with elimination of very frothy saliva. (3) Subnormal temperature. (4) Inco-ordination of movement and subsequent inability to stand. (5) No signs of convulsions nor any evidence of excitability. (6) Rapid spontaneous recovery in all cases, once the recumbent position has been assumed.

I fear that I have not kept strictly to the therapeutic side of the question, but I was anxious to show that although we have no very outstanding therapeutic results to record, other than those which are already well known or are of an empirical nature, yet we have some conditions involving the endocrine glands, the study of which would inevitably lead to valuable knowledge both in the pathology of these diseases and in the therapeutics that could be invoked to prevent or treat them when met with in human beings.

Dr. H. Gardiner-Hill.

(*Medical Unit St. Thomas's Hospital.*)

THE EFFECT OF THYROID AND PITUITARY EXTRACTS ON OBESITY AND GROWTH.

As my contribution to the discussion I should like to give you the results of some investigations we have been making in the Medical Unit at St. Thomas's Hospital on the treatment of obesity and defective growth by thyroid and pituitary extracts. We have tried the effect, both of thyroid alone and of pituitary alone, and the results leave no doubt in our minds that pituitary extracts have some influence; but we have found that a combination of these two gives the most satisfactory results. By this method of treatment it seems possible to improve the carbohydrate metabolism, which in nearly all these individuals is defective. This improvement is accompanied by a loss of weight in the patients with obesity, and an acceleration of growth in the children with deficient growth.

Dr. Forest Smith and Dr. Isaac Jones have investigated the sugar-tolerance, both before and during treatment, in all the cases I am about to discuss. We have used this test as an indication of the efficiency of the carbohydrate metabolism.

I will show some lantern-slides illustrating the effect of thyroid and pituitary extracts on the sugar-tolerance curves of our patients, but before I do this, I must give you some idea of the type of case we have been dealing with and a more detailed account of our methods.

(1) THE TREATMENT OF OBESITY IN ADOLESCENTS AND ADULTS BY A COMBINATION OF THYROID AND PITUITARY EXTRACTS.

The present series of cases number 150—75 adolescents and 75 adults.

The Type of Case suitable for Treatment.

The adolescents, in addition to obesity, all showed at some period disturbances of growth and bone development, disordered genital function and abnormal carbohydrate metabolism. They represent the type of case usually associated with pituitary disturbance. In the present series few presented any evidence of pituitary tumour.

The adult cases occurred chiefly in women after childbirth or at the menopause; or the obesity, first noticed at puberty, became exaggerated after childbirth or at the menopause. In many of these individuals a history of abnormal growth and genital function in adolescence could be elicited, and when they came under observation they all showed a disturbance of carbohydrate metabolism.

Treatment by Thyroid and Pituitary Extracts.

We have treated these cases by a combination of thyroid and whole-gland pituitary extracts given by the mouth in gradually increasing doses. Desiccated extracts were employed. The diet was not changed. It was found that thyroid extract alone, when given by the mouth, produced good results in some cases, but others showed thyroid intolerance and a tendency to hyperglycæmia. This difficulty seemed to be overcome by giving, in addition to the thyroid, whole-gland pituitary extract. The latter appeared to increase the patients' tolerance to thyroid. A considerable reduction in weight, with improvement in metabolism, was obtained. Whole-gland pituitary extract alone, when given by the mouth, did not produce any reduction in weight, though it seemed to exercise a definite influence on carbohydrate metabolism, as judged by improvement in the sugar-tolerance curves.

Investigation of the Carbohydrate Tolerance.

Sugar-tolerance curves were used to follow up the effects of treatment. Estimations of the blood-sugar were made according to MacLean's method. The fasting blood-sugar was first determined, 50 grm. of glucose given and subsequent blood-sugar examinations carried out at half-hourly intervals for two hours. In the normal person the blood-sugar rises from a fasting level of about 0·1 per cent. to

0·175 per cent. or thereabouts at the end of half an hour; after this it falls to the normal level again at the end of an hour and a half.

Our cases gave very different results from this. The adolescent cases before treatment showed two main types of curve. When the obesity was of comparatively recent origin, the curve was high and delayed. The carbohydrate tolerance was decreased. The patients were unable to deal with sugar in the normal way. In the cases of longer duration the curve was low and the blood-sugar did not rise to the usual level. The carbohydrate tolerance was increased. The patients dealt with sugar too quickly.

The adult cases before treatment on the whole showed a marked carbohydrate intolerance; an occasional individual with increased tolerance was found. But in the majority the curve was high and delayed, and the blood-sugar after two hours remained at a level considerably above that found in normal individuals.

Effect of Treatment on Carbohydrate Tolerance.

In adolescents, as a result of treatment with thyroid and whole-gland pituitary extracts, we found that both types of curve tended to become more normal. The results, however, in individuals with increased tolerance were consistently better than in those with decreased tolerance. In the former a reduction in weight could be more readily effected.

In adults we found also that the carbohydrate tolerance is improved by treatment with the combined extracts. Whole-gland pituitary extract given alone has the same effect, but the patients tend to gain rather than lose weight.

(2) The Treatment of Defective Growth by Thyroid and Pituitary Extracts.

The cases of this series fall into two main groups: (*a*) where there is a definite endocrine factor, thyroid or pituitary, (*b*) where this has not been recognized, though growth is considerably below the average and the infantile characteristics persist. These children seem often to have been the subjects of a chronic infection, such as tuberculosis, or of malnutrition.

In the first group, that of the endocrine cases, three instances are presented to illustrate the effect on the rate of growth of thyroid extract in juvenile myxœdema, anterior lobe pituitary extract in pituitary infantilism, and a combination of thyroid and whole-gland pituitary extracts in obesity with skeletal undergrowth. These children, when they came under observation, were considerably undergrown for their age. In each case treatment has improved the rate of growth and the carbohydrate tolerance.

In the second group there are six cases to show the results of treatment with various combinations of thyroid and pituitary extracts. The rate of growth of these cases during treatment has been compared both with the rate of growth without treatment and with the average rate of growth for age. Our figures have been compared with the growth curves for British children given by Holt in his "Standards of Nutrition."

Conclusions.

The conclusions we have drawn from these investigations are, that in certain cases of obesity a beneficial effect both on the obesity and on the carbohydrate metabolism can be expected from the administration of thyroid and whole-gland pituitary extracts; and it is suggested that a combination of these two extracts given together will produce better results than when thyroid extract is given alone. But the improvement in the condition is not maintained for any length of time after treatment is discontinued. Sooner or later the metabolism reverts to its former state, and again the patients gain weight. It does, however, seem possible, when once the weight has been reduced, to maintain the new level of metabolism on

decreased doses, and by alternating periods on and off treatment, to help these patients considerably.

The sugar-tolerance curves after treatment also seem to show that certain preparations of pituitary extract are active when given by the mouth and have a definite influence on carbohydrate metabolism, but these extracts, when given in this way, are not effective in reducing obesity.

As regards the investigations on growth, the effect of thyroid extract is of course well known. There is no doubt as to its value in cases of true thyroid deficiency, and it seems possible that it may be useful also in certain cases of undergrown children, where the thyroid defect is not so obvious.

I do not wish to lay too much stress on the increased growth we have found by feeding with anterior lobe pituitary extracts, though our results are in keeping with the recent feeding experiments carried out by Dott and Fraser in animals, and reported upon to the Medical Research Committee. Our cases, however, are not sufficiently numerous to enable us to pass a definite opinion upon them ; nor have we followed them up for a sufficiently long period of time ; but I feel in my own mind that the results on the sugar-tolerance curves are difficult to explain except on the assumption that the anterior lobe extracts are active.

Mr. Kenneth Walker.

Interest in the therapy of the testis was first aroused in 1889 by the communication made by Brown-Séquard to the Academy of Sciences on the results of certain experimental work carried out on himself. Brown-Séquard, then 72 years old, and in failing health, found that after a series of subcutaneous injections of the expressed juice of the testes of dogs and guinea-pigs, he experienced such an improvement in his general health that he was able to return to active life in the laboratory. He stated that he believed testicular extract to be dynamogenic in nature, and that it had a stimulating effect on the exhausted central nervous system. Since this time various investigators have worked on the effect of injections of testicular extract. Dixon showed that it produced a fall of blood-pressure after a long latent period, this fall being mainly the result of cardiac inhibition. Korenchevsky showed that injections stimulated metabolism and especially that of proteins. Others tried the effect of injections in correcting the changes following castration. The results were disappointing. Injections of extract of testicle had little or no effect on the growth of castrated animals, although Maisonnave and Monziols showed that in children with badly developed testes, who grew rapidly up to the age of puberty, injections of testicular extract checked skeletal growth.

Of recent years interest in the use of testicular extracts, which for a short time had waned, has been revived. Stanley suggested the use of testicular substance as having an advantage over an extract. By using a needle with large calibre he was able to inject subcutaneously a semi-solid substance obtained from the testes of rams and goats. His subjects were 656 inmates of the Californian State Prison, who submitted themselves to treatment suffering from a variety of conditions such as asthenia, rheumatism, neurasthenia and senility. Improvements in the general health of these patients followed the use of these injections. Three hundred and twenty-six asthenic subjects appeared to derive benefit and eighty-one reported increased sexual stimulation. In general, the injection of testicular substance seemed to exercise a beneficial effect in promoting bodily well-being and often in relieving pain of obscure origin. Actually we should regard Stanley's use of testicular substance as an instance of the employment of heterografts rather than of extracts. The semi-solid masses he used could be felt for many weeks as nodules lying under the skin, slowly becoming absorbed, and liberating during this process a certain quantity of hormone.

However, the use of injections of crude testicle or even of an extract has certain dangers and disadvantages, and it is natural that, as in the case of other endocrine

glands, an attempt should have been made to find a substitute in oral administration. Of the effect of testicular extract given by the mouth it is possible to speak emphatically. I am convinced that administered thus it is useless. Steinach fed rats, castrated at the age of four weeks, on fresh rats' testes. His experiments lasted for three months, but not the slightest favourable results were observed so far as the development of secondary sex characteristics was concerned: the animals remained, morphologically and functionally, typical castrates. Korenchevsky, at the Lister Institute, prepared in 1922 large quantities of extracts of bulls' testes, and at St. Mary Abbot Hospital careful records were kept of twenty or thirty inmates who were treated with this extract. Not only was the physical condition of these patients carefully watched, but they were submitted weekly to certain psychological tests elaborated by the National Institute of Industrial Psychology. Through the kindness of Professor Korenchevsky I had the opportunity of examining these patients, and I could find no indication of any of them having been in the least affected by their treatment. The active principle of the testes is either not absorbed by the alimentary canal, or, what is more probable, it is destroyed by its juices.

It is the failure to obtain results from oral administration that has led to the use of testicular grafts. Here experimenters have been up against two obstacles, the first of these being that of obtaining material, and the second the difficulty in ensuring that the transplants become vascularized and survive the operation. In order to overcome the first difficulty Voronoff and Thorek have made use of grafts obtained from the higher apes. The blood-relationship, as shown by Nuttall and others, and demonstrated by agglutination and precipitation tests, is very close between the higher anthropoids and man, and Voronoff considered that the chances of the survival of heterografts obtained from such a source would be far greater than if derived from the lower animals. He has now made some fifty or sixty transplants in the human subject and claims to have obtained valuable results. The grafts have been undertaken for various conditions: eunuchoidism, senility, neurasthenia and sexual neuroses. In summing up his results it would appear that the chief benefit has been in the direction of improvement in the general health, increase of mental and muscular vigour, and stimulation of the growth of hair. Less definite improvement has been noted in cases grafted for impotence or sexual neuroses. Other workers in this field have preferred to use human transplants and have derived their material, when opportunity occurred, from newly-executed criminals, patients dying of accidents, and from cases of "ectopia testis" in which the removal of the misplaced organ was justified. It is from this last source that I myself have obtained my grafts. The technique of transplantations is not difficult. Personally, I think that Voronoff's use of a serous cavity like that of the tunica vaginalis for implantation is the best. The placing of grafts within the tunica vaginalis invariably provokes a reaction in which there is a pouring out of an exudate rich in nutritive substances. This exudate supplies nutriment to the engrafted tissue until vascularization has taken place.

Do these grafts actually survive, or is any benefit that accrues solely due to the absorption of liberated hormone? Personally, I believe that the vascularization and survival of the graft is an actual fact. At the same time I am convinced that a steady atrophy of the engrafted material occurs and that the actual life of the transplant is considerably shorter than that claimed for it by Voronoff. Opportunities have occurred for the removal of a graft two or three months after operation and examining it histologically. Most observers, such as Ribbert and Herlitzka, have found that, although the grafts had become vascularized they showed signs of regression, the tubules being replaced by cords, and the cells of the tubules reverting to a non-differentiated embryonic condition. Voronoff, however, has claimed that on one occasion on which a graft was taken from the testis of a young ram not yet

arrived at the stage of spermatogenesis, the graft actually evolved in its host, and when removed subsequently showed the presence of spermatozoa. I find difficulty in accepting this statement. Other evidence of survival of the graft may be found, as Dr. Langdon Brown has suggested, in changes in metabolism as shown by the respiratory exchange and by glucose-tolerance curves. In a series of cases subjected to such tests I was able to show a distinct difference in the sugar-curves before and after operation. How long a graft survives must still remain a matter of doubt, but I should feel happy if I could be sure that complete atrophy has not occurred in eighteen months. But this does not necessarily mean that at the end of that period all benefit has been lost. In the first place certain observations have been made which suggest the possibility that a graft may stimulate the growth of existing testicular tissue, secondly the hormone supplied by a graft may tide a patient over a critical period until compensatory changes in other endocrine glands have succeeded in restoring the endocrine balance. It must be remembered that the relationship between the testis and such glands as the pituitary and the suprarenals is a very close one. Fichera first showed that a hypertrophy of the pituitary body occurred in castrated fowls. An equally close relationship between the testis and the suprarenals is demonstrated by the occurrence of sexual precocity or of virilism in certain cases of suprarenal tumour. There is a direct connexion between the sex glands and the thyroid, and an indication that both the thymus and the pineal act as a brake on the activity of the gonads. This intimate association of the testes with the other members of the endocrine circle, although it has the advantage of permitting compensatory changes when the activity of the testis is impaired, has corresponding disadvantages. In the majority of dystrophies that present themselves for treatment by means of testicular grafts we are not dealing with a lesion confined to the testis, and too much must not be expected of the operation. Even when a case has begun as one of pure testicular deficiency, a graft will only confer a certain degree of benefit on account of the reciprocal involvement of other glands and of other structures that has taken place during the lapse of time. The longer the history of the deficiency the less hopeful is the outlook. An old castrate will not experience so much benefit from a graft as does a subject who has recently suffered testicular loss. It is frequently advisable to combine testicular grafts with the use of extracts of other endocrine glands, notably of the pituitary, the suprarenals or the thyroid.

Finally it is necessary to say a few words about vaso-ligation as a means of stimulating the activity of the testes. It has been shown by many investigators that ligature of the vas causes degenerative changes in the spermatic epithelium, although it is interesting to note that these changes are not permanent, but are followed by a phase of regeneration. Ancel and Bouin in their classical experiments called attention to the fact that "tied" animals gave signs of marked sexual excitement, and correlated this with the increase in interstitial cells that takes place after ligature. This observation is the basis of Steinach's work in Vienna. Those who are familiar with Steinach's experiments on aged rats must certainly be impressed by the results of vaso-ligature. Aged animals in a state of apathy and neglect showed after the operation of ligature increased activity, improvement in fur, alertness, increase in bodily weight and a return of sexual vigour. It is true that after a period of regained health they relapsed again into senile apathy, but in spite of this the total length of life was greater than in the case of the untied rats. Macht, in America, has confirmed the statement that muscular co-ordination and vigour are increased in aged rats by vaso-ligature. In human beings the results of the operation have been less certain. In many cases no benefit has taken place. In some the general health and mental vigour of the patient have improved and in a few cases the results have been still more striking. Much criticism has been levelled at

Steinach's experiments. In the first place it is by no means certain that the interstitial cells are responsible for the internal secretion of the testes, and, personally, I am one of those who regard them as trophic cells rather than the source of testicular hormone. However, even if this be the case and if Steinach's arguments, based on the increase of interstitial cells that takes place after ligature, be valueless, the bottom has not been knocked out of his experiments. Up to the present time I have performed Steinach's operation in some fifteen cases, and although it is difficult to eliminate the element of suggestion, I believe that in at least half of these cases an improvement in general health and an increase in mental vigour have taken place. I am particularly inclined to recommend the operation in the treatment of that hopeless malady of later life, paralysis agitans, having seen a very marked improvement in two out of four cases which I have treated in this way.

Dr. H. Crichton Miller.

I do not propose to make any contribution to this discussion except an announcement. It occurred to me that evidence of two kinds was called for : first, in relation to the pharmacological action of endocrine preparations other than thyroid when taken by the mouth ; and, second, in regard to subjective effects apart from objective results. In order to obtain evidence on these two aspects of organo-therapy two things are necessary : first, competent observers, and, second, the elimination of the suggestive factor. To ensure the first the experiment is being limited to members of the profession, and to make certain of the second the content of the various preparations is unknown to the experimenters.

A commercial drug house of good standing has prepared two special series of tablets—one series for men and one for women. The bottles bear the firm's name so that the actual experimenters will know whose products they are dealing with. The bottles each contain twenty-five tablets and they are labelled A, B, C. Each bottle is issued with instructions as to dosage, and with a schedule, on which the doctor is expected to state such observations as he may make on himself while he is taking the tablets.

A few sets are still available, and I should be glad if doctors—both men and women—would volunteer to make the experiments. They may be had by applying to the Secretary, Tavistock Clinic, 51, Tavistock Square, W.C.1.

I suggest that in this way doctors may find out something for themselves more authentic than by reading a good deal of the literature at present in circulation.

Mr. J. E. R. McDonagh.

Introduction.

My contribution to the discussion on endocrinology deals only with the glandular extracts with which I have experimented on animals. These include adrenalin, thyroxin, insulin and "ageratin" (testicular extract). The animal experiments were undertaken to study the effects of the drugs upon the blood and organs and to form from these a basis for their "chemo-therapeutic" employment in man.

There are two points which, to my mind, stand out as the knowledge of the nature of the glandular extracts increases. One of these is that many of the extracts are derivatives of the amino-acids. The other is, that though the amino-acid group is the most active, the action of the compound really depends upon the way in which this group is checked. Adrenalin is a derivative of the amino-acid tyrosine, in which the action of the amino group is checked in the main by the paraphenolic hydroxyl group. Thyroxin is a derivative of the amino-acid tryptophane, in which the action of the amino group is checked in the main by the iodine groups in 4 and 6 positions. Insulin is probably a derivative of the amino acid histidine in which one or more of the amino groups are checked in the main by a sulphur group. In other words, a conductor

group suffices to check the condenser action of the simple dehydrator adrenalin. A simple dehydrator group is required to check the condenser action of the more powerful substance thyroxin. A powerful dehydrator group is required to check the action of the still more complex substance insulin. Comparing the action of the various groups which occasion condensation of the protein particles, it is found that $NH_2 > S > O > I > Br > Cl$.

Adrenalin.

Adrenalin causes the protein particles in the plasma to increase in number and then to go into solution. This dual action of conduction and dehydration leads to a constriction of the capillaries, which is followed in turn by a rise of blood-pressure. The action of adrenalin is not specific. All drugs exhibiting first a conductor and then a dehydrator action can be used as antidotes in conditions brought about by the protein particles increasing in size or becoming hydrated. Insulin causes hydration of the protein particles and adrenalin proves an "antidote" because it causes dispersion of the hydrated particles. When protein particles increase in size they are apt to be precipitated in certain capillaries. If the cerebral capillaries are involved, fits, or convulsions ending in coma, form the clinical picture presented. Precipitation in the capillaries of the lungs leads to the pulmonary form of shock and, in the capillaries of the liver, to the splanchnic form of shock. In these conditions adrenalin is not sufficiently powerful to be of any value and its place is better taken by glucose injected intravenously and oxygen injected subcutaneously. Adrenalin is frequently employed in cases of asthma and hay fever. If the asthma is due to "gelation" of the protein particles in the pulmonary veins, the arterioles, capillaries and bronchioles are usually contracted. In these cases adrenalin is worse than useless, the drug of choice being "sup. 36" (the symmetrical urea of para-benzoyl-para-amino-benzoyl-1-amino-8-naphthol-3-6-sodium sulphonate). In hay fever adrenalin can do no more than produce a temporary relief, while "sup. 36" injected intravenously in two or three 0·01 grm. doses arrests the condition for the season. Adrenalin, though often advised as a styptic, is apt to fail, because its later action of dehydration is one causing a retardation of coagulation which, when exhibited first, increases the flow of blood.

Thyroxin.

Thyroxin causes a dehydration of the protein particles, but the process is exhibited more slowly than is the case with adrenalin. On this account thyroxin is of chief value in the chronic stage of disease, because its dehydrator action is one which causes dispersion of chronically hydrated particles. In the chronic stage of disease the protein particles increase in size and become too independent. In fact it is this independency affecting cells which causes them to become malignant. In my opinion malignancy is nothing more nor less than an expression of protein hydration. When the particles gradually increase in size, thyroxin is able to break them up. This increases their negative electric charge, their Brownian movement, their surface area and energy, to the well-being of the patient. The reaction of thyroxin is not specific, because it can be effected as well by certain organic compounds of sulphur. Indeed, in the chronic stage of disease the action of thyroxin can be enhanced by the simultaneous employment of colloid iodine taken internally or injected intravenously, and of contramine injected intramuscularly. Contramine is the carbon di-sulphide product of di-ethyl-amine. When thyroid extract is prescribed internally, its action may be enhanced by the simultaneous employment of thiol-amino-methyl-glyoxaline in 1 gr. doses.

Insulin.

Insulin causes hydration of the protein particles. That is to say, it brings back to the protein nucleus the other constituents, such as electricity, salts, sugar, amino-acids and fat, which are being detached therefrom at the bidding of a

dehydrator. The action of insulin is not specific, because this restoration of the protein "solar" system can be achieved with certain synthetic organic preparations of sulphur. Though ordinary hydrators such as thorium, uranium, &c., restore the lost carbohydrate to the protein particles, they cause such a development of fat as to make the last state worse than the first. Sugar is only one of the wandering "planets" which insulin brings back to the protein "suns." Consequently, insulin is an excellent antidote against any powerful dehydrator, be it of bacterial or of chemical origin. Though insulin may come from the islet cells of Langerhans in the pancreas it is equally valuable whether these cells be damaged or not, and even in diabetes mellitus, the condition which has led to the birth of insulin. It is more than probable that the injury sustained by the gland is the last act of the dehydrator and not the *fons et origo mali*. Insulin is invaluable in all cases in which the protein particles undergo severe dehydration. This physical process is exhibited in a varying degree in every case of pregnancy, after severe operations, as a result of a severe bacterial infection, and in certain cases of metallic and non-metallic intoxication. Severe dehydration in pregnancy causes pernicious vomiting and atrophy of the liver, because of the gelation the protein particles undergo in the veins of the portal system. If insulin is prescribed in time the patient's condition may be improved in a few hours. Insulin corrects the gelation by causing the particles to increase in size and to diminish in numbers. But gelation can be corrected also by conveying electricity to the slow-moving particles, and equally good results as are obtained with insulin may be obtained with sup. 36. When the protein particles undergo precipitation in the cerebral and renal capillaries, rather than a gelation in the portal or pulmonary veins, and give rise to the clinical pictures of eclampsia and uræmia (*sine hyperuræmia*), insulin should be preceded by 500 c.c. of a 10 per cent. solution of glucose intravenously, and 500 c.c. of oxygen subcutaneously, to split up the protein particles first. In this class of case insulin is better replaced by "sup. 468," because the latter has both a hydrator and a conductor action, while insulin has a hydrator action only. Sup. 468 is the symmetrical urea of para-benzoyl-para-amino-benzoyl-1-naphthylamine-4-6-8-sodium sulphonate. The drug is best injected intramuscularly in 0·001 grm. doses every day or every other day. After about the fifth injection 0·01 grm. sup. 36 should be prescribed. A severe degree of dehydration occurring after operations is the cause of pulmonary venous thrombosis, a condition which, like the pernicious vomiting of pregnancy, responds equally well to insulin as to sup. 36.

An acute bacterial infection may occasion such a profound degree of dehydration as to call for insulin, as being the drug best suited to bring back to the colloid state those protein particles which are passing into true solution. But I have not formed a definite opinion up to the present in exactly what class of case insulin, sup. 468 and sup. 36 are indicated respectively. I can say no more than that the action of insulin is enhanced by the subsequent employment of sup. 468, and this again by sup. 36. Both sup. 468 and sup. 36 exercise a conductor action. The conductor action of sup. 468 is much greater than that of sup. 36, but the latter has the advantage in that the conductor action is exhibited without delay. The acute dehydration occasioned by metals is met with following the use of arseno-benzene, and is represented clinically by hæmorrhagic encephalitis, dermatitis and acute yellow atrophy of the liver. Here, again, I cannot say for certain which is the superior—insulin or contramine.

The acute dehydration occasioned by non-metallic substances in animals is readily prevented and checked by the use of insulin. Even in chronic lesions, so long as the dehydration of the protein particles is still in progress, insulin is of therapeutic value. For instance, insulin lowers the blood-pressure in some cases of hyperpiesis, alleviates the pain in some cases of osteo-arthritis, and relieves the spasmodic constriction of the cerebral capillaries in cases of threatened hemiplegia.

While dehydration of the protein particles occasions constriction of the capillaries, hydration is followed by relaxation. Insulin is particularly valuable in those cases of uræmia characterized by a hyperuræmia, interstitial nephritis, and hyperpiesis. In certain cases of late syphilis, when the protein particles are enormously increased in number and the suspension stability of the red blood-corpuscles is markedly reduced, insulin is the drug of choice. Clinically, these cases present the lesions met with early in the generalization stage. Insulin can be employed with advantage in cases of anæmia associated with a great destruction of the red blood-corpuscles. When an overdose of insulin is given the excessive hydration produced causes a precipitation of the protein particles in the cerebral capillaries. The clinical and pathological conditions produced are the same as those met with in eclampsia, in the type of uræmia characterized by an absence of a rise in the percentage of the blood-urea, by a low instead of a high blood-pressure, and by the presence of a nephrosis. The best way to get precipitated particles into circulation again is to break them up, and this is achieved with glucose intravenously and oxygen subcutaneously. Oxygen injected subcutaneously with a Bayeux apparatus is quite as good a dehydrator as glucose; consequently, it is incorrect to say that glucose acts by restoring to the blood the sugar which insulin has driven away. Furthermore, there are many metallic drugs, acting as conductors, which neutralize in an instant the toxic effects of insulin. Whenever protein particles undergo true hydration hypoglycæmia results, merely because the carbohydrate "planet" of the protein "solar" system becomes more closely drawn towards the protein "sun" or nucleus.

Ageratin.

"Ageratin" is the name I have given to the active principle of the testes, from *ἀγήρατος*, not grow old. Unfortunately I know nothing of the chemistry of the compound, but its physical behaviour suggests it may be a phosphatide and related to cephalin. In experimental work ageratin is one of the most extraordinary substances I have encountered. It prevents a conductor from causing dispersion of the protein particles, histamine from producing shock, and insulin from bringing about a state of hypoglycæmia. Ageratin protects an animal against infections, it interferes with the antigenic action of the organic extract in the Wassermann reaction, and it reduces the precipitating effect of a positively reacting serum. In view of these results the drug should prove useful in protecting man against the invasion and the continued working, when once in the system, of both parasitic and chemical agents. Much work is required and a long time must elapse before adequate proof of the former can be obtained. The latter is capable of proof more readily, and this can be done by comparing the "chemotherapeutic" treatment of disease alone with that followed by injections of ageratin. For this purpose I have chosen conditions presenting one or more signs, which can be detected easily, such as a rise of blood-pressure, hyperglycæmia, glycosuria, &c. When these are treated with chemotherapeutic remedies alone a relapse of the clinical signs and symptoms is almost inevitable. On the other hand, when the chemotherapeutic drugs are followed by 0·001 grm. doses of ageratin injected intravenously or intramuscularly every other day, the clinical relapse can be delayed.

Dr. M. B. Ray.

Long before the interest of both physicians and laity was excited by the profound alterations in the development of the human organism following the removal of the sexual glands in early youth, primitive man was engaged in the practice of organo-therapy, as exemplified by the sacrificial customs associated with his religious beliefs.

A very cursory study of ancient religions will at once show the great prevalence of the idea of slaying the god in the shape of his human or animal representative. Sir James Frazer's explanation would appear the most feasible, viz., that it was to

guard against the enfeeblement of the divine spirit by encroaching age and ensure its transfer to a young and vigorous successor. The human or animal representative of the god was *eaten* in order that the believer might acquire the divine physical characters and moral qualities, and thus share in his attributes and powers. In the case of the vine god, the juice of the grape was his blood, and the rites associated with Dionysus were not acts of revelry but a solemn sacrifice. In the case of the corn god, bread was his body and it was consumed with precisely the same object. Apart from the sacrificial customs thus briefly indicated, primitive man has always believed that nature transfuses into men and animals the qualities of the food they eat. A feeder on venison is swifter of foot than he who subsists on the flesh of the bear. A Chinaman once bought a whole tiger and ate it with the hope of making himself fierce and brave. Tiger's flesh is forbidden to the womenkind of some races for fear it might make them too strong-minded. The liver of a valorous enemy was supposed when eaten by his conqueror to endue him with all the qualities of the vanquished. Powdered human heart has long been credited with marvellous potency in the creation of courage and intrepidity. An American-Indian thought brandy was a decoction of hearts and tongues, because he said, "After drinking it I fear nothing and I talk wonderfully."

Enough has been said to show that there is nothing new in the ideas underlying the practice of endocrine therapy, and its advance to its present position may be said to be mainly due to a more precise knowledge of the bodily functions.

Besides the well-known reciprocal actions of the ductless glands, their interaction with the nervous system and their influence on growth, they exert equally well-known powerful effects on bodily metabolism. I have only to instance the control of carbohydrate assimilation by the pancreatic apparatus and its mobilization by the chromaffin tissue; the influence of the thyroid on protein metabolism and its further influence on glycæmia by its regulating action on pancreatic and chromaffin hormones.

When we come to consider the train of symptoms associated with insufficiency of the functions of the parathyroids, such as the increased excitability of the motor, sensory and vegetative nerves, the powerful muscular spasms, the gastro-intestinal disturbances, the altered calcium metabolism, the trophic changes in skin and bones—the latter chiefly characterized by rarefaction of the trabeculæ and marked atrophy; we have an example of a series of metabolic and trophic changes directly due to the disfunction of minute glands only discovered forty-five years ago. Indeed, it was not until 1891 that Gley first discussed the physiological significance of the parathyroids.

Having regard, then, to the dependence of bodily metabolism on the harmonious action of the endocrine system, it necessarily follows that these glands are primarily responsible for what may be termed the make-up of the individual. No longer can the nervous system be credited with undisputed authority in this direction. We know that the vegetative and other nerves are profoundly affected by the hormones of the ductless glands, themselves vegetative in nature and origin.

When we enter the realms of psychopathology we find certain Freudian theories explicable by physical-chemical action. Memory and the material basis of the nervous system may be regarded as manifestations of metabolic activity in which ions and possibly electrons play a part. To put it shortly the individual may be described as the sum of his ductless glands.

It is a commonplace observation that in a group of individuals there will be the greatest variation in the response to the same stimulus. An abnormal stimulus in a certain number will be borne without harm. In others again the identical stimulus will evoke symptoms of disease. To some extent the variety of symptoms resulting from the abnormal stimulus will fall into roughly-defined classes. To these classes

we give the name diathesis, a term somewhat reminiscent of the old humoral pathology. Until a fuller understanding of the activities of the ductless glands was forthcoming this term remained incapable of intelligent explanation. A diathesis, therefore, is not a sharply circumscribed disease-picture, but a manifold disease-tendency dependent on an altered blood-admixture with consequent tissue changes.

This leads me to the consideration of a well-marked diathesis, namely, the arthritic diathesis, and I propose to discuss its connexion with the endocrine system.

I will briefly indicate some types of the arthritic diathesis:—

(1) The young female who suffers from nervousness, tachycardia, sweating, pigmentation and various trophic changes, accompanied by a general metabolic upset, as evidenced by decreased sugar tolerance and an altered basal metabolism. Without any discoverable focus of infection she tends to develop symmetrical joint lesions, usually fusiform in shape, beginning in the smaller joints, with marked atrophic changes in bones, muscles and skin.

(2) The woman at or near the menopause, usually stout and well-nourished, with a tendency to put on more flesh, and showing all the signs of a mild thyroid deficiency, who suddenly develops coarse synovial changes in the knees or hips, characterized by crepitations on movement, changes which may progress to osteo-arthritis, and, in the case of the hip, to bony ankylosis.

(3) The pale, anæmic, sweating child, with a dicrotic pulse, tachycardia, arrhythmia and a tendency to dilatation of the right ventricle. Attacks of pyrexia with pain, swelling and stiffness of the joints, the formation of fibrous nodules with a liability to endocarditis.

(4) The occurrence of fibrositis, characterized by pain and stiffness of muscles and the formation of fibrous inflammatory nodules in the connective tissue, can, in my opinion, be adequately explained from the metabolic standpoint. At a recent discussion at the Royal Society of Medicine opinions were sharply divided between its metabolic and infective origin. Time does not permit a recapitulation of the arguments for and against. Ranging myself on the side of the metabolists, it would appear that there is a distinct type of individual with a special tendency to attacks of fibrositis, and this condition is worthy of more consideration as to the part played by the ductless glands in its causation.

Attention has been repeatedly directed by various observers to the probable inter-relationship between alterations in the thyroid and arthritic changes. For example: (*a*) Kocher long ago stated that larval hypo-thyrosis might cause pain, stiffness and heaviness of the extremities, and that joint distension might develop. (*b*) Rothschild and Levi treated twenty cases of light and mild arthritis with thyroid extract; eighteen were cured. Of nineteen severe cases, seventeen showed decided improvement. (*c*) Pringle and Sinclair Miller corroborate the findings of Pemberton, in that a lowered glucose tolerance is present in a large proportion of cases of arthritis and fibrositis. The degree of tolerance is roughly proportional to activity of the arthritic process. Glucose tolerance tends to return to normal with convalescence and recovery.

Increasing attention is now being given to the probable connexion between gout and endocrine disorders.

Llewellyn, in the *Lancet* of March 7, 1925, discusses the relationship between goitre and rheumatism. Taking the Ministry of Health Report, that the incidence of rheumatism between 15 and 16 years is higher in girls than boys, he finds that there is the same preponderant incidence of goitre in respect of girls. He points out that the periods of increased incidence of rheumatism and goitre synchronize markedly with puberty, the child-bearing decades and the menopause. These periods are associated with oft-recurring hyperæmia of the thyroid. He draws attention to the fact that where goitre is endemic the incidence of rheumatic fever, cardiac rheumatism, and rheumatoid arthritis is abnormally high. He also makes the most interesting observation that if rheumatic fever develops in the child of a goitrous mother, it is

very rebellious to the action of salicylates. In such a case the addition of thyroid seems to activate the salicylate. Lastly he discusses the most suggestive findings as to geographical incidence. There would appear to be abundant evidence to justify further inquiries in this direction.

Dr. Higgins

asked whether Mr. McDonagh had injected oxygen into patients. It was now largely used in France. He (the speaker) had recently discussed the question of its injection with Professor Elvey, in Paris; and Professor Elvey had stated that it was without any action at all.

Mr. McDonagh

(in reply to Dr. Higgins) said he used Bayeux's apparatus for injecting oxygen subcutaneously. This apparatus enabled one to inject any desired dose; one knew not only how much oxygen was being given, but also at what pressure. He had done many experiments on animals, and on man, with oxygen injected subcutaneously. If an animal were given a very large dose of oxygen in this way, it was impossible to give to that animal a dose of insulin large enough to cause fits or convulsions.

Dr. Langdon Brown (in reply)

said all he intended to do was to direct attention to what seemed to him to be some outstanding points in the discussion. Most of those who had heard the discussion would agree that there was a smaller difference between the clinical and the laboratory points of view now than was the case a few years ago. And it did not represent a movement entirely on one side. Professor Swale Vincent would be among the first to admit that some of the addresses which had been given in this discussion—particularly, perhaps, Dr. Gardiner-Hill's—showed that more exact methods were now being employed to check clinical results. He thought that, on the other hand, Professor Swale Vincent might concede the clinician the point that there was laboratory work which was beginning to confirm what some had concluded from clinical experience. Was Professor Swale Vincent really prepared to maintain what he (the speaker) had designated the agnostic point of view? By this he meant the view that because one could squeeze a certain juice out of the posterior lobe of the pituitary gland, it told one nothing about the functions of the pituitary gland. That was Professor Vincent's position two years ago, and he (the speaker) would be glad to hear whether Professor Vincent still held that view. It seemed to him (Dr. Langdon Brown) a very extraordinary thing, if that were so, that there should be such facts as the following: (1) That when the pituitary gland was diseased, there was produced a certain type of blood-sugar curve, and that that blood-sugar curve was the same as the curve found in cases of glycosuria of pregnancy. (2) That Dixon had shown that ovarian extract had a marked effect in producing a flow of pituitrin; and that pituitrin also directly checked the action of insulin. Was that gland placed there and forming a secretion merely for a purpose analogous to that which someone maintained was the reason for which God placed fossils in the rocks; to test the faith of man? Was the posterior lobe of the pituitary placed there, with a secretion of its own, or from which one could squeeze a juice, with no possible relation with those extraordinary facts? He did not think such a position was tenable at the present time.

He regarded Mr. Leslie Pugh's remarks as extremely interesting. The field of comparative medicine was still largely an unworked one, and some of Mr. Pugh's results coincided so closely with what had been obtained in clinical work—particularly in regard to pituitrin and the internal secretion of the ovary—that his was a valuable contribution. Mr. Pugh's observations with regard to the influence of pituitrin on the milk production in cows fitted in very well with the clinical observation of association between the sympathetic nervous system and the ductless glands. The sequence of events Mr. Pugh described he (the speaker) would

interpret as follows: The cow disliked its milker, and that dislike acted through the sympathetic or emotional nervous system on the pituitary gland, resulting in an inhibition of the secretion of pituitrin and so of its normal stimulant effect on the secretion of milk; hence the supply of milk was checked. By injecting pituitrin, that personal equation was eliminated, i.e., the dislike of the cow for the milkman, and the stimulation to the production of milk went on as a purely chemical and mechanical process. That fitted in very well with the clinical view of the association of the emotional nervous system with the ductless glands. It was not easy to accept the view that it was pure chance that the posterior lobe of the pituitary would yield a juice which acted at this particular stage in the process of yielding milk, but had nothing to do with the function of the pituitary gland.

The same argument was applicable to the case of adrenalin. He did not now want to go into the question as to whether clinical observations were to be placed on a par with laboratory experiments; both Professor Swale Vincent and he had expressed their views on that point, and there was not much difference between them. At all meetings of this sort it was good to be pulled up abruptly sometimes and to be challenged as to facts and deductions, and for that reason the meeting would feel indebted to Professor Swale Vincent.

Professor SWALE VINCENT (in reply) said that the discussion, so far as Dr. Langdon Brown had just presented it, seemed to have narrowed itself to the pituitary, and the relationship between experimental and clinical observation. He could not now deal with the whole matter, but on the question just raised he had something to say.

In the first place, the fact that one could extract an active juice from the pituitary gland meant nothing more than that one could extract such a juice from it. In itself it meant nothing more than that. In regard to the action of pituitary generally, he almost felt that people ought to be content at present to admit that they did not know what was the relation between the physiological and the clinical findings. And the same applied to the adrenal bodies. Most investigators had adopted provisionally the theory that the animal could not do without adrenin in states of physiological emergency. It was a good working hypothesis, but all workers were not convinced of its truth. One was very tempted to assume that adrenin might have some function in the system, but it was the very plausibility of the idea that made one suspicious of it. The chain of evidence was not complete.

With regard to the observations of Mackenzie Wallis and Professor Dixon, he was by no means convinced of their value. He did not consider that Professor Dixon had shown the mechanism which he asserted, and his tests for pituitary were not satisfactory. The whole of the methods of testing these active substances in minute quantities were very delicate. The guinea-pig's uterus would react with doses so small that one felt they were not any indication of physiological significance. He did not believe that the stated effect of ovarian and pituitary secretion had been proved.

He agreed generally with what Dr. Langdon Brown had said in his opening address. It would, indeed, be untrue for him to say there had been no attempt at movement towards the clinician on the part of the experimental physiologist; he thought there had. But he still thought there was a good deal of scepticism needed by the practicing physician. The fundamental difficulty was one which was so obvious that he was almost ashamed to mention it. It was, that one simply never knew what was going to happen if the treatment were not adopted. Until one knew the normal curve and incidence of the disease or disorder, and knew them very thoroughly, it was too doubtful to try the effect of a remedy and then assume it had been efficient. Especially was it doubtful when one gave, as most physicians did, a mixture of substances and then arbitrarily attributed the good results to some one of them. Difficulties were great in all branches of therapeutics, but much greater in endocrine therapy than in any other department.

SECTION OF ANÆSTHETICS.

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Section of Anæsthetics.

President—Mr. H. Edmund G. Boyle, O.B.E.

The Selection and Administration of Local and General Anæsthetics for Animals.

By Frederick T. G. Hobday, C.M.G., F.R.C.V.S.

ABSTRACT.)

The veterinary anæsthetist has mainly three things to keep in view. The first is to summarize in his mind the apparent general condition of the patient and its freedom or otherwise from serious organic disease. Secondly, to make sure that it has been properly prepared: and this with us is just as important as it is with you, for to anæsthetize an animal on a full and distended stomach is to court disaster. The third thing is to see that nothing interferes with the free movement of the heart and lungs; and here again the ingenuity of man is brought into play to secure each individual species of patient in the most appropriate position possible; and fourthly, the anæsthetist must utilize his experience to judge of the amount of the anæsthetic which is requisite to give the required stage of anæsthetization with safety in order to enable the surgeon to operate upon his patient without the latter feeling the touch of the knife.

It may not be generally known, but it is an accomplished fact, that animal patients, when they have the misfortune to require surgical attention, are better protected by the laws of the land than human subjects; for, by the amended Animals Anæsthetics Act which was passed in 1919, a long list of operations, which are performed in veterinary surgery, was officially scheduled for compulsory anæsthetization. With some of these a general anæsthetic is compulsory, and with others the option is given to select either a general or a local anæsthetic.

In regard to castration—the horse, ass, and mule are, however, specially exempted—not on the ground that there is less pain when their testicles are removed, but because of the powerful and successful opposition raised at the time by certain people, backed up by influential agriculturists and owners of valuable horses.

Their chief reason was that, had the Act become law as it was drafted, the travelling gelder (who in many districts of England is the man who does most of the castrations) would have been unable to carry on his job—partly because of his ignorance of the proper method of administration of anæsthetics and the risks to be incurred, and partly because of the difficulty (for him) of obtaining anæsthetic agents and the added expense in time and money. Many thousands of horses are operated upon in the standing position every year, and, when done expertly, the whole operation from start to finish scarcely exceeds a minute and a quarter or two minutes at most, the colt showing so little evidence of after-pain or discomfort that he usually walks up to the manger and begins to feed at once.

Methods of Securing.

The method of securing an animal patient before the administration of an anæsthetic needs the most careful consideration, for the most persuasive phrases to "be quiet, and breathe slowly," are of no avail to a kicking horse or a biting dog which is being forced against its will to inhale a strange vapour, unless accompanied by some form of restraint, *and this restraint must be of such a nature that the respiratory apparatus and the work of the heart are left unimpeded as far as is possible.*

Choice of an Anæsthetic.

As might be expected, this is a matter of the utmost importance. The choice depends somewhat upon the variety of patient to be operated upon, its age, and, of course, upon any diseased conditions (especially of the internal organs), together with the presence or absence of expert help in the form of a colleague who has had experience in this branch of medicine. In regard to the latter, the veterinarian is much more handicapped than his *confrère* in human practice, as the question of pecuniary outlay plays such a large part when dealing with the animals of the farm.

One has, for *general* anæsthesia, the choice between chloroform, ether, and various mixtures (such as A.C.E. and A.E.C.). Personally, from an experience of over thirty years, during which I can claim to have done over 12,000 anæsthetizations, I prefer pure chloroform for the adult horse, cow, and dog, with A.C.E. mixture, or ether, for the monkey, puppy, and the cat. *If given rationally, in accordance with the dictates of experience, I am prepared to maintain that chloroform is an ideal and safe anæsthetic for the horse and dog.*

Young animals take less in proportion than adult animals, and we find, too, that one has to be a little more careful with the cart-horse variety than with the thoroughbred. I think that this is due largely to the fact that the muscles of the thoroughbred are more in condition than those of the cart-horse on account of the increased exercise which it takes. During the first four or five minutes after beginning to administer the chloroform, the horse neighs, struggles, and passes through a state of excitement, and then unconsciousness slowly supervenes; the muscles relax, the tail becomes limp, and eventually, if the application of the chloroform is continued, the corneal reflex disappears. Judging from experience I never like to reach this stage before about ten minutes has elapsed. For many operations, however, it is not necessary to push the anæsthetic quite to this stage, as the degree of insensibility is now sufficient for short operations, especially for those (such as castration) which are done in a few minutes and involve only incisions in the regions of the hind-quarters. Moreover it is commonly observed that when the inhaler is removed and air admitted to the nostrils, the horse often enters into a still deeper stage of anæsthesia.

Ether, ethyl chloride, nitrous oxide gas, and A.C.E. mixtures, are not suitable or convenient for the horse or the larger animals. The chief disadvantages are: first, that a very large quantity is necessary before anæsthetization can be produced (much more so than in the case of chloroform); secondly, that the stage of excitement is very prolonged, and this, with a powerful animal like the horse, is obviously a great drawback; and thirdly, that so much of the drug is necessary to produce anæsthetization that the cost, when a large number of animals have to be anæsthetized, is very considerable and represents an unnecessary increase as compared with the cost of chloroform.

With the cow, sheep and the goat we have an additional difficulty to deal with, especially when the operation is urgent, in that these animals have four stomachs, the first of which, the rumen, is merely a receptacle and is generally very full and apt to cause pressure on the diaphragm. With sheep, too, experience has taught us that we must always keep the head up whilst the animal is on its side, as otherwise regurgitation occurs and death may supervene from asphyxia or from mechanical pneumonia about two or three days later. In cases of dystocia or eversion of the womb—and we get some very difficult cases of this kind in cattle and mares—the use of chloroform is a perfect godsend and has become almost universal all over Great Britain. Cases, for example, of eversion of the uterus, which used to require (in the old days when chloroform was not generally used) the help of at least two strong men in addition to the manipulative efforts of the veterinary surgeon himself, become enormously simplified, and the eversion can almost invariably be replaced with success. The chief secret of safe anæsthetization in these animals is to give the

anæsthetic slowly and in small quantities at a time. For the ox, the Cox's mask or one of its modifications (which I am showing here to-night) is the one generally used ; and you as medical men, who are able to work at your parturition cases in a warm room and generally with the help of a skilled *confrère*, will appreciate our difficulties in having to work in a draughty cowshed by the light of a lamp, generally with no better help than that of a farm labourer, or perhaps the farmer himself and his wife.

The Dog and Smaller Animals.

I am pleased to be able to state that anæsthetics are universally used for operations on the dog, and in refutation of the statements which I have heard made even recently and on various occasions in the past and of the impression which I believe still prevails in many physiological laboratories, I am prepared to make and to support the statement *that chloroform is the ideal anæsthetic for the adult dog if only it is given in vapour form and in a rational manner.* It can be given safely time after time in the hands of an expert by the open method, but the ideally safe plan is to give it with the aid of an inhaler. *The chief safeguard against danger during the chloroformization of a small animal is that of making sure that anæthetization takes place very slowly.* To hurry matters is to court disaster, and for a toy dog, and even one the size of a terrier, a minimum of four, or better still five, minutes should be allowed, whilst for a big dog from six to ten minutes is not too long. Once the stage of complete anæsthetization has been reached it requires the application of a very little chloroform now and again to keep the patient in an unconscious condition. Some practitioners prefer the use of a dose of morphia previous to chloroforming, and indeed morphia has such a deeply narcotizing effect on the canine species that many major operations can be done under its influence with the aid of some local anæsthetic for the skin. In any case, where morphia has been given first, the amount of chloroform necessary is very small. The use of the Junker inhaler (which, by the way, has also been applied to the horse by the aid of a foot bellows) has, in my experience, entailed a serious element of danger, as the vapour which comes through is much too strong. The pattern of which I have a model here, and which is now universally used by the veterinary profession, is much safer, as with it an exact percentage of from 1·9 to 2 per cent. can be administered all the time. This was worked out for me by Mr. Symes and the late Professor Waller, at London University. It acts equally well with chloroform, ether, or A.C.E. mixture ; and, as you will see, it is simple in construction, depending entirely on a small inexpensive motor to which a fan is attached. The air is driven by an electric fan over the surface and the vapour of the mixture is passed on to the mask on the patient's nose.

Chloroform is also used for birds, particularly for the operation of caponing, and the illustrations on the epidiascope show you a veterinary surgeon in South Africa administering chloroform to an ostrich.

My experience is that the respiration is the chief thing for the anæsthetist to watch ; for although the pulse is also a guide it is not so reliable as the respiration as a sign of warning that danger is at hand. Of course the anæsthetist should devote his whole attention to the business in hand and not be watching the operation or other things. Unfortunately in veterinary practice this ideal of a skilled veterinary *confrére* as anæsthetist becomes very difficult to attain on account of the smallness of our fees and the distance of practitioners from one another.

Antidotal Treatment.

I will briefly refer to the antidotal treatment in cases in which respiration has ceased as a consequence of the over-administration of the anæsthetic, because I believe that we veterinarians almost universally use as an antidote an agent which we assert is of great value and which I think is deserving of consideration in the case of human patients, namely, Scheele's hydrocyanic acid. This the

veterinary anæsthetist keeps at hand, and immediately upon the cessation of respiration, or its threatened failure, artificial respiration is applied, together with a full medicinal dose of Scheele's hydrocyanic acid either on the back of the tongue or hypodermically. The idea was suggested to me in the first instance by observing the increased respirations shown by dogs when poisoned by prussic acid, and it struck me that if I could work out the *full* medicinal dose of this drug consistent with safety I might be able to stimulate the respiratory centre to action for a sufficient length of time to enable the lungs to inspire fresh air and thus get rid of the overdose of chloroform. There is no quicker stimulant to the respiratory centre than this drug, and if once the breathing can be made to re-commence, it can be usually maintained by artificial effort. For the horse I advise the administration of from ½ to 1 drachm hypodermically, for the dog from 1 to 5 minims on the tongue, and for the cat 1 to 2 minims also on the tongue.

The performance of artificial respiration in the horse is somewhat of a task, as one can imagine, for the ribs will stand a good deal of pressure, and we sometimes find it of advantage to assist a man to stand upon them and jump up and down. In the dog and cat the best method is to compress the ribs by a series of short sharp movements of the hands and fingers. In all animals it is better to have the patient with the left side uppermost, as in this position the heart has the least pressure on it and is nearest to the stimulating effect of the fingers.

Of the other antidotes which may be used we speak particularly well of liquor ammon. fort. and nitrite of amyl; but strychnine, on account of the extraordinary idiosyncrasy of dogs and cats towards this drug, is an agent which we have almost entirely ceased to employ for them.

Local Anæsthetics.

The use of local anæsthetics for veterinary operations has increased very much of late years, and we have now such an excellent selection to choose from that the veterinary operator has no excuse for not doing an operation painlessly. The chief point to consider with the smaller animals is the relative toxicity of the drug employed.

Of the well-tried anæsthetics we have for selection cocaine, eucaine, novocaine, stovaine, procaine, holocaine, urea and quinine, eudrenin, codrenin, ether spray, ethyl chloride, and numerous others; and we have as adjuncts such agents as adrenalin, suprarenin, renastyptin and other products.

It is necessary to be aware of the idiosyncrasy to toxicity shown by the various animals, but if one knows that of cocaine one can work with absolute safety with all the other local anæsthetics as they possess much less poisonous properties.

With regard to the dosage of cocaine: The operator should never allow a maximum dose of more than 10 gr. to be absorbed at one time in a large animal of the size of a horse or an ox. Not that this amount will cause poisoning, but when exceeded to any extent it causes a hyperæsthesia and excitement, and this makes the animal twitch and interferes very materially with the performance of any delicate operation. I have given for the purpose of observation as much as a drachm hypodermically to a horse without causing death, but with this large amount the animal became absolutely frantic, and this affords a typical illustration of what happens on the racecourse when a racehorse has been overdoped by this drug as a stimulant to its running powers.

For the cat or the small dog I now put the maximum to be allowed to be absorbed at one time at ½ gr.; for the dog the size of a terrier 1 gr., and for one of the size of a great Dane or a mastiff it is not wise to exceed 2 gr. An overdose produces hypersensitiveness to sound, the animal becoming excited and restless, the limbs twitching, eyeballs rolling, respiration laboured, frothy saliva appearing around the lips; and eventually, if a toxic dose has been administered, death occurs from heart failure after violent convulsions.

Intraspinal Anæsthesia.

I have attempted intraspinal anæsthesia both for the horse and for the dog, but I must confess that it is not a method which I, personally, shall ever adopt nor has it become popular either in this country or on the Continent.

DISCUSSION.

Sir John Moore said he claimed that Mr. Richard Roberts, late of Kendal, was the real originator of the use of chloroform in the veterinary profession. In his country practice Mr. Roberts had a considerable number of castration cases, and he always operated under chloroform. He at first used an ordinary leather muzzle or nosebag, and his procedure was to take the colts into a field or paddock, attach a cart rope by the middle to the halter or headstall, and with men holding the ends of the rope at right angles to the animal, the colt was made to fall on the side desired when anæsthesia was in progress. He found that with the ordinary muzzle there was too much admixture of air, and the colts were too long in getting under the influence of the chloroform in consequence. He then proceeded to block out excessive amount of air by means of a towel round the top of the muzzle, with the result that the animals were quickly under, the operation performed, and animals on their legs again in about ten minutes altogether. Mr. Roberts was induced to read a paper on the subject at a Veterinary Medical Association meeting at Carlisle, and the outcome of the interest thus created was the Carlisle chloroform apparatus.

In his list of operations for which chloroform was used in veterinary practice, Mr. Hobday had omitted to mention reduction of dislocations. In the hilly Lake District dislocations of the hip-joint in cattle were quite common. It was impossible to reduce them without an anæsthetic, or at all events reduction was rendered very easy with its aid. The usual procedure was to cast the animal with the dislocated limb uppermost, pass a rope under it from flank to buttock and fix it to a convenient post, pillar or doorway at the back to act as a stay; then with a small pair of pulleys attached to the limb above the fetlock and fixed to some opposite point, traction was exerted and the limb extended; next, with the use of a stout pole, the head of the femur was lifted into the acetabulum either from behind or from the front according to whether the dislocation was in front of or behind the acetabulum, usually the latter. For general anæsthesia in large animals chloroform was ideal. There was no fear of accident attending its use. The voluminous respiratory organs presented a sufficient safeguard against a death usually attributed to a cessation of the respiratory act. Twice in the experience of his younger days he (Sir John Moore) had had to carry out destruction, by means of an anæsthetic, of favourite animals, an old hunter and an old pony, under the provisions of wills, and in each case after strong administration of chloroform for a considerable time he had had to complete the act with a revolver. He remembered also a similar experience in a mare with disease of the heart in which he had steadily administered chloroform for half-an-hour or forty minutes without result. During the war in France, where many thousands of horses were subjected to chloroform, he could not call to memory the death of a single animal from its use. He considered it practically impossible to kill a horse with it. In conclusion he (Sir John Moore) adversely criticized the law permitting castration to be performed without the administration of an anæsthetic. He had no use whatever for the standing method of that operation, and considered that the administration of chloroform should be made compulsory.

Mr. Henry Gray said the introduction of general anæsthesia in veterinary practice was coincidental with that in human surgery. Most of the methods of administration, including that in the standing position now so much in vogue, had their origin at the commencement of the anæsthesia era. Mr. John Roalfe Cox from that period for nearly half a century continuously and systematically practised chloroform anæsthesia on horses. If anybody should be considered to be the pioneer of that method in veterinary surgery that honour should be ascribed to Mr. Cox.

As to the administration of chloroform to the smaller animals no special apparatus was needed, for he had practised general anæsthesia on them for over forty years and had only used a glass jam-jar, or a tin or aluminium canister with great success and even without any assistance or special table. He did not fix his animals but gave the anæsthetic to them in the standing position or when held on the side. One could not exclude the personal element in the success or safety of general anæsthesia practised on whatever species or by whatever method or apparatus.

Not very much reliance should be placed on the corneal reflex in animals for it might be abolished and the animal could go on struggling or screaming or jerking the body or limbs. Certain breeds of dog had under normal conditions a very low degree of corneal reflex. The ciliary and palpebral reflexes in the dog, and the solar or pedal reflexes in the cat were much better indications.

To prevent excitement and struggling in the excitable breeds of dog he usually injected from ½ to 4 gr. of morphine half an hour before administering chloroform. Morphinized dogs breathed very slowly or infrequently and therefore they did not inhale very much of the drug in a given time. It was not, however, necessary for them to take in much before the surface and solar or pedal reflexes became abolished.

The avian species in general were very susceptible to general anæsthesia and if it were continued too long the mortality would mount up.

He did not see the value of HCN as a respiratory stimulant, since he found that if the majority of animals showing signs of respiratory arrest were left alone, or Laborde's method of rhythmical traction of the tongue were performed for a few minutes, breathing would recommence. He thought that recovery after the use of HCN was nothing more than a coincidence.

Excluding equines from the benefits of the Animals (Anæsthetics) Act, 1919, during castration was a disgrace to the Legislature. This very painful operation was solely performed for the benefit of the owner, not for the benefit of the animal. All operations for the benefit of the animal ought to be done under an anæsthetic.

Anæsthetics Committee.

(*Medical Research Council and Royal Society of Medicine.*)

As the result of correspondence between the Council of the Section of Anæsthetics and the Medical Research Council a joint "Anæsthetics Committee" has recently been instituted.

The objects of this Committee are to initiate and direct investigations into the mode of action of various anæsthetic agents and the impurities that may be associated with them, to assist in the search for new anæsthetics and secure their manufacture in this country and, in general, to interest itself in all matters concerning anæsthetics.

As representatives of the Section of Anæsthetics the Council has nominated: Dr. Blomfield, Dr. Shipway, Dr. Hadfield.

The Medical Research Council has secured the services of: Dr. H. H. Dale, F.R.S., Professor F. G. Donnan, F.R.S., Professor M. S. Pembrey, F.R.S.

The Committee has already commenced work, and appointed Dr. Joseph Blomfield as Chairman, and Dr. C. F. Hadfield as Hon. Secretary. Professor Storm van Leeuwen (Leyden) has been asked to serve as a Corresponding Member of the Committee.

Among the subjects which the Committee has already under consideration may be mentioned:—

(1) The desirability of further research into the condensation and anæsthetic properties of di-methyl ether.

(2) Various points connected with the anæsthetic value of pure ether.

(3) The possibility of obtaining in this country an adequate supply of ethylene and acetylene for anæsthetic purposes.

(4) Impurities in nitrous oxide. The discussion of this subject really suggested the formation of the Committee in the first instance. Various somewhat vague reports as to the poor quality of some cylinders of nitrous oxide are occasionally received. The fact that this gas is not included in the British Pharmacopœia and is thus not subject to any official tests for purity, renders further investigation desirable. The Committee has already secured the co-operation of several of the firms manufacturing nitrous oxide, but is specially wishful to obtain reports on and,

if possible, the samples of the nitrous oxide thought by anæsthetists to be either lacking in anæsthetic action or toxic in its effects.

Members of the Section of Anæsthetics who are able to assist the work of the Committee with regard to nitrous oxide in particular, or who have suggestions to make on other anæsthetic subjects are requested to communicate with the Hon. Secretary, at 47, Queen Anne Street, Cavendish Square, London, W.1.

CHARLES F. HADFIELD, M.D.,
Hon. Secretary.

Section of Anæsthetics.

President—Mr. H. Edmund G. Boyle, O.B.E.

DISCUSSION ON SPLANCHNIC ANALGESIA.

Mr. O. Stanley Hillman.

Splanchnic analgesia consists in placing a "block" across the path of impulses passing from the viscera by way of the cœliac ganglia and the great splanchnic nerves to the central nervous system. This is done by depositing a certain amount of 1 per cent. novocaine in the path of the splanchnic nerves.

Professor Charles A. Pannett[1] has shown, by animal experiments, that novocaine placed in relation to the mesenteries of the viscera definitely protects the animal from shock caused by handling those viscera. He also showed that the rigidity of the abdominal wall in operations on the abdomen was considerably reduced when the mesenteries of the viscera operated upon were infiltrated with novocaine. This proves that the rigidity of the abdominal wall is not entirely due to irritation of the parietal nerves, and therefore injection of the parietal nerves alone will not give the same relaxation as when the visceral nerves are blocked as well.

I am fortunate in being able to quote to you a clinical example of this. I was asked to give a splanchnic anæsthetic to a patient who had a perforated gastric ulcer of about six hours' standing. This man was very ill; he had had a recent hæmatemesis and was very anæmic, and when I saw him on the anæsthetic wagon he was groaning and writhing with pain; his abdominal wall was board-like. With very great difficulty I succeeded in administering the splanchnic block; I then had him turned over in order to block the abdominal wall. When he was lying on his back and I had uncovered his abdomen I was struck by the fact that he was now silent and quite still. I spoke to him, fearing that he was dying, but then found out that he was still because all his pain had vanished. That incident afforded me remarkable proof of the efficacy of the splanchnic block.

In the human subject the upper abdominal viscera are supplied by nerves through the great splanchnic nerve, which is derived from the fifth to the ninth or tenth dorsal nerves and also by the lesser and least splanchnic nerves, which run in close proximity. These nerves pass through the crura of the diaphragm and are distributed to the various viscera by way of the cœliac ganglia. For the induction of splanchnic analgesia, then, it is necessary to place the novocaine in the path of the great splanchnic nerve and this is done at the level of the first lumbar vertebra.

In order to complete the analgesia for an abdominal operation it is of course necessary to place a block across the paths of the nerves of the abdominal wall. This is done by performing an abdominal field block of the segment of the abdominal wall concerned, instead of by infiltration in the line of incision as described by Crile.

It is of course possible to operate under splanchnic analgesia and abdominal field block alone, and when I commenced, this procedure was carried out with considerable success. The method must mean a certain amount of psychic shock to the patient; it certainly entails a great deal of strain on the surgeon to perform a major operation on a conscious subject. For these reasons I changed the technique and commenced to make the injections under light ether anæsthesia, which is now given throughout the operation; thus the splanchnic block is now used, not as a substitute for general anæsthesia, but as a means of reducing shock and the amount of the anæsthetic drug administered.

[1] "Anoci-association," *Brit. Journ. Surg.*, 1914-15, ii, pp. 248-259.

The instruments required are a suitable syringe and needles. The syringe is of the Record type and is graduated to 10 c.c.; the barrel, which is of glass, will hold, however, 12 c.c., but the extra 2 c.c. is left ungraduated for aspiration purposes. The syringe is fitted with handles and a ring on the plunger to allow for easy manipulation. The nozzle carries a pin which fits the bayonet catch of the needle. The needle for splanchnic analgesia is 12 cm. long and 1 mm. in diameter; all the needles are fitted with bayonet catches to prevent flying off when injecting and are made of rustless steel. For the field block, needles 8 cm. long are usually needed, but the length of these needles depends on the fatness of the patient. If the procedure be carried out on a conscious patient then the smallest needle is required for raising the cuticular wheal.

The anæsthetic fluid used is 1 per cent. novocaine made up in sterile normal saline. Of this solution never more than 5 oz. is poured out at a time for each patient—this constituting the maximum safe dose of the drug.

After the patient has been anæsthetized he is turned on to his left side and a small pillow or sand-bag placed under the lumbar spine between the last rib and the iliac crest. This prevents any sagging of the spine and tends to open up the angle between the last rib and the spine. The knees are drawn up but not to so great an extent as is usual when a spinal anæsthetic is given. The first lumbar spine is then identified by counting upward from the fourth spine, which is found immediately above or on a line joining the two iliac crests. Using the smallest needle, a cuticular wheal is now raised over the first lumbar spine, which enables this spine to be found readily after turning the patient. A point 7 cm. external to the first lumbar spine is now found, either by measuring with a sterilized steel rule or by using the hand, the width of four fingers being about 7 cm. If the patient be conscious another cuticular wheal is raised at this spot, otherwise the splanchnic needle is entered at once. The needle is first advanced at an angle of about 45° to the median plane, forwards, inwards and slightly upwards until the body of the first lumbar vertebra is struck, the depth of this usually being 9 cm. or three-quarters of the needle. The needle is next withdrawn until its direction can be changed, when it is again advanced, but inclined slightly more forward and the body again struck. If the direction of the needle is now correct it will be felt to slip over the edge of the vertebra; if this is so the needle is then advanced 1 cm. further and there stopped. If the bony contact is not made when the needle has entered to a depth of 9 cm. it should not be pushed on, as it will not be in the right place, but it must be withdrawn and directed a little less forwards. It is then advanced again until contact with the bone is made at the right depth and the procedure finished as before.

The needle being in position, the syringe, previously filled with 10 c.c. of novocaine solution, is attached. Aspiration is now performed; a most vital point is to make certain that the needle is not in a blood-vessel. If no blood appears in the syringe its contents are very slowly discharged. If blood appears the needle must be withdrawn until it ceases to appear. The syringe is refilled and after aspirating again a further 10 c.c. is injected and then another 5 or 10 c.c. according to the kind of case. The patient is then turned on to the other side and the same injection made on the left side. The technique as described is not difficult, but it *must* be carried out in detail and with great care or the injection will be in the wrong place. Aspiration *must always be done*, for should such a quantity of novocaine be discharged into a blood-vessel the result would be a fatality. Before trying splanchnic block on the living it is essential to have practised the technique and to have become thoroughly conversant with the anatomical points in the post-mortem room.

One of the difficulties that may be encountered is that the angle between the last rib and the transverse process of the first lumbar vertebra is so small that it is very difficult to alter the direction of the needle without striking either the rib or the transverse process.

Gentleness in performance is essential. This is especially the case when making the injection on a conscious patient, though unconsciousness must be no excuse for roughness. If bone is struck roughly the point of the needle will be turned and will tear the tissues on withdrawal, unnecessary shock will be caused and pain will ensue in the case of a conscious patient.

When the injection is made on a conscious patient the feel of the parts to the needle is different as the muscles contract and grip it. This may cause difficulty the first time that it is met with and also it is necessary to withdraw the needle much nearer to the surface before its direction can be changed.

With regard to the technique of the abdominal field block. All the nerves except the seventh dorsal turn downwards on entering the rectus sheath. In the nerve block the novocaine has to be deposited along the outer border of the rectus sheath. The injection is made as follows: If the patient is conscious, cuticular wheals are raised just below the xiphisternum, at the costal margin where the rectus crosses it, half-way between this and the umbilicus and at the umbilicus. If the patient is anæsthetized the same points are entered, using the 8-cm. needle. The needle is made to enter at each point in turn, and at each, it is first of all directed vertically and pierces the anterior wall of the rectus sheath and is then pushed on through the rectus muscle for 0·5 to 1 cm. or until the posterior wall of the sheath is felt. Injection of 2 c.c. of novocaine is then made. The needle is withdrawn to the subcutaneous tissues and its direction changed upwards or downwards and entered to the right depth again and another 2 c.c. is injected. This is repeated so that one point of entry in the skin is used for several entries into the rectus sheath, and in this way the novocaine is distributed along the outer borders of the two recti and along the costal margin, cutting off all impulses passing from that area.

The abdomen is then painted with picric acid and covered with a sterile towel and the patient wheeled into the theatre, and by the time the towels, &c., have been arranged the anæsthesia will be complete. The whole procedure takes me fifteen minutes, though at first of course it took longer.

It now remains to discuss: (I) The advantages and (II) disadvantages to the surgeon. (III) The advantages and (IV) the disadvantages to the patient. (V) The effects on the pulse-rate and blood-pressure.

(I) *The advantages to the Surgeon.*—(1) The most striking phenomenon on watching a case under splanchnic analgesia is the smallness of the respiratory excursions as compared with a similar case under purely ether anæsthesia of as light a depth. This, of course, is of the greatest help to the operator.

(2) There is complete and perfect muscular relaxation of the abdominal wall with a light depth of anæsthesia.

(3) There is complete absence of any sudden straining on the part of the patient when some necessary traction is made. This is especially noticeable when working on the gall-bladder and common ducts. It is well recognized how difficult of management some patients can be when operations are carried out on the biliary tract, straining and deep respirations occurring on the least traction. All this is eliminated if the splanchnic nerves are successfully blocked.

(4) Lastly, the surgeon is relieved of all anxiety, as he knows that his patient is receiving no shock, though this again must not be an excuse for rough operating. The surgeon also is left with a free hand to do whatever he thinks best for his patient even though it involve a great deal of time and manipulation.

(II) *The Disadvantages to the Surgeon.*—These are perhaps two in number, though I think that they are easily outweighed by the benefits that the patients derive.

(1) The addition of fifteen to twenty minutes to the time required to anæsthetize the patient.

(2) The fact that if the operation is a very long one the effect of the field block

of the abdominal wall may wear off, so that there is some rigidity in the sewing up of the abdominal wall. This however can be overcome by the co-operation of the anæsthetist, who after the experience of a few cases will know whether deepening of the anæsthesia is needed or not.

(III) *Advantages to the Patient.*—(1) The patient receives less anæsthetic drug. (2) There is the minimum of shock. (3) There is no fatigue from excessive respiratory movements. (4) There is no "falling off" in the condition of the patient after return to the ward as is so often seen after ether anæsthesia. (5) The day following operation the patient is in better condition owing to the absence of shock. (6) Owing to the diminution of the amount of the anæsthetic drug there is less vomiting. (7) There is less liability to chest complications. (8) There appears to be less post-operative pain. (9) Some patients who would be considered beyond surgical aid can be given the chance of an operation by means of splanchnic analgesia.

(IV) *Disadvantages to the Patient.*—(1) In a few cases there has appeared to be a tendency to flatulent distension, but its incidence does not seem to be greatly increased. (2) In one case a patient complained of severe neuralgic pain in the back after a splanchnic injection. It was however never definitely ascertained that this was due to the injection though perhaps it is better to regard it as such. (3) In some cases patients have suffered from severe headaches of two or three days' duration.

(V) *Effects on the Pulse-rate and Blood-pressure.*—(*a*) In all cases the blood-pressure falls considerably during the first part of the operation and this takes place whether it be a severe operation or not, so that the fall cannot be attributed to shock. The duration of the fall works out on the average to be for twenty minutes, after which a steady rise in the patient's normal blood-pressure occurs. In nine cases the average fall worked out to be to 88 mm. Hg. (*b*) The pulse-rate does not rise with the fall of blood-pressure. This shows that shock is absent and that despite the fall in the blood-pressure the condition is not dangerous.

I have now performed the splanchnic block in forty-nine cases. Of these, thirty-eight were cases of upper abdominal operations, and in twenty-six of them a partial gastrectomy was performed. Of this series not one patient developed any chest complications and only two patients died immediately after the operation, and so could perhaps be said to have died of shock. The condition of these two patients however was so desperately bad that it was only the fact that splanchnic analgesia could be used which enabled them to be given the slender chance afforded them, so that the result of the method really speaks in its favour rather than otherwise. Thus out of forty-nine patients there were no chest complications and no deaths from shock, though the series include some very difficult upper abdominal cases, and at least one patient had a definite chronic bronchitis and one a tuberculous lesion of the lung.

Lastly, I would like to point out a comparison to be made between thirty-one cases of partial gastrectomy with and without splanchnic block. This shows that the average pulse-rate after operation has dropped from 111 to 85 and the deaths from shock to have been reduced from 12·9 per cent. to 3·2 per cent. (the one death being one of the desperate cases mentioned). It also shows that the number of blood-transfusions has fallen from fourteen to two and in both the latter the blood-transfusion was given for previous hæmatemesis and not for shock.

Splanchnic analgesia has a very definite value in abdominal surgery. I do not think that all upper abdominal cases need it, though we have found them to be all the better for it. In the case of gastric surgery, however, where the extent of the lesion is always doubtful until the abdomen is opened, I do suggest that the surgeon will be happier in what he does if he knows that the splanchnic nerves are blocked. Therefore for these cases I suggest that the splanchnic analgesia should be induced as the routine and be combined with as little general anæsthetic as possible. In this

way shock will be overcome and the chance of the patient developing a chest complication will be reduced to a minimum.

Mr. R. E. Apperly.

Few anæsthetists in this country use regional methods, and may still even leave spinal anæsthesia to be done by the surgeon if the case requires it. I must confess that had it not been for the constant help and encouragement of Mr. Gordon-Taylor and Mr. Hillman and my other surgical colleagues at the Middlesex Hospital, I should not have had the courage to attempt splanchnic block and other regional methods.

Personally I have no longer any doubt that combined regional and general anæsthesia will, in the future, be very largely used in all the more extensive surgical procedures. It is already used in abdomino-perineal excision of the rectum, Wertheim's hysterectomy, and often in prostatectomy, and recently we have used it with success in block dissections of the neck.

That the anæsthetist is the most desirable person to give the regional as well as the general anæsthetic, I have no doubt. I would therefore urge anæsthetists to take a greater interest in regional methods as an addition to, rather than a substitute for, their present armamentarium.

Perhaps the greatest drawback to regional anæsthesia is the time taken in its performance. This particularly applies to hospital work. I am, however, sure that in some cases the results obtained by combined regional and general anæsthesia are so strikingly good as to justify the extra time taken. I would also point out that as the anæsthetist becomes more experienced in the method the time taken is very greatly shortened.

Splanchnic anæsthesia presents no great difficulties from the anæsthetic point of view, provided the method has been practised sufficiently in the post-mortem room, and that the technique which Mr. Hillman has described is carried out in every detail.

When no general anæsthetic was used, morphia $\frac{1}{4}$ gr., scopolamine $\frac{1}{100}$, was given one and a half hours before operation, and morphia $\frac{1}{6}$ gr., scopolamine $\frac{1}{200}$, half an hour before. We soon found however that a light general anæsthesia throughout does no harm and has the following distinct advantages:—

(1) There is no need for the large preliminary dose of morphia and scopolamine which is almost a necessity if no general anæsthetic is given; (2) the splanchnic block is rendered much easier, takes less time, and is of course painless; (3) towards the end of a long operation when the field block is wearing off the anæsthesia can be quickly deepened and the closure of the abdominal wound facilitated; (4) psychic shock is abolished; (5) the strain on the surgeon, to say nothing of that on the anæsthetist, is lessened if they are dealing with an unconscious patient.

Therefore, excepting the first few cases, the anæsthetic technique has been conducted as follows: Morphia $\frac{1}{6}$ or $\frac{1}{4}$ gr., with atropine $\frac{1}{100}$ gr., is given three-quarters of an hour before operation. General anæsthesia is induced either with gas and ether or chloroform and ether, and the splanchnic and abdominal field block is then done in the anæsthetic room. This procedure generally occupies about twenty minutes to half an hour, and immediately afterwards the patient is taken into the theatre and general anæsthesia maintained with ether and oxygen, or a mixture of chloroform, ether and oxygen. The abdominal incision should not be made until five minutes after the abdominal field block is completed.

The blood-pressure begins to fall shortly after the first splanchnic injection and reaches its minimum at the end of about half an hour. From that time it begins to rise again until by the end of one hour it is, as a rule, actually back to what it was before operation. It may be well to mention that it is extremely important to

move the patient gently after the injections are made, and to avoid raising the shoulders too high, otherwise the blood-pressure may fall very rapidly.

In private practice, of course, the anæsthetist, after inducing anæsthesia, has to call to his aid the doctor in charge of the case or the surgeon's assistant to maintain a light anæsthesia while the regional blocks are carried out. It is extraordinary to see how light an anæsthesia can be maintained after the first injection. Patients who have been difficult to manage during induction become quite placid, this being presumably due to the fall of blood-pressure. I would emphasize the point that the performance of the regional blocks cannot be hurried, but to be successful must be done slowly and deliberately.

The anæsthetist will be aware of the success or otherwise of the splanchnic anæsthesia directly the surgeon handles the abdominal contents. If the splanchnic block is effective the breathing should alter neither in rhythm nor in rate even when considerable traction is put on any of the contents of the upper abdomen and the general anæsthesia is light.

In splanchnic anæsthesia, as in all other forms of regional or local anæsthesia, even when accompanied by light general anæsthesia, if the best results are to be obtained, there must be co-operation between the anæsthetist and surgeon. The anæsthetist must ascertain the exact position of the incision the surgeon proposes to make before doing his field block and even make his area of anæsthesia sufficiently wide to allow of a slight extension of the wound if necessary. On the other hand, the surgeon must not stray outside the anæsthetic area without warning the anæsthetist first, so that the nerve block may be extended, or the general anæsthesia deepened. In the same way heavy retractors used too forcibly, or muscle torn, instead of cut, may pull on tissues outside the blocked area, and render a deepening of the general anæsthesia necessary, and then part of the benefit of your nerve block may be lost. The two advantages of the method are the quiet respiration and absence of movement of the abdominal contents, but once they are disturbed it is often difficult to restore it again. Thus the sudden thrusting of a hand without warning into the pelvis—an unanæsthetized area—in the course of an operation on the upper abdomen may create difficulties for both surgeon and anæsthetist. It cannot be too strongly urged on the surgeon who wishes to get the best results from regional anæsthesia that gentleness in the treatment of tissues is absolutely essential.

In dealing with the advantages and disadvantages, as we think, of the method, I should like to take the disadvantages first.

Disadvantages of the Method.

(1) *The Fall of Blood-pressure.*—The blood-pressure falls, often very considerably and very rapidly, but in only one case has the fall caused any anxiety whatever. It was in a case of cholecystectomy, where the patient's shoulders were lifted very suddenly in order that an air-cushion should be placed under the back. The anxiety however was very short-lived as was the fall in blood-pressure. However this case taught us to exercise care in moving patients after the splanchnic injection has been made.

Bearing this disadvantage in mind we think that in cases where the patient has a very low blood-pressure or where the circulation has been recently depleted by a gastric or duodenal hæmorrhage, it is wise perhaps to reduce the amount of novocaine used. One can reduce the amount used in the splanchnic block, say to 20 c.c. on each side, and that in the abdominal field block by using ½ per cent. instead of the usual 1 per cent. solution.

(2) *Post-operative Headache and Backache.*—In six or seven of our cases there have been more or less severe headache and backache. We have been rather at a loss to explain this, as although our technique has been practically the

same throughout all the series, we have not had this complication arise in the last forty-odd cases. In those cases in which the fall of blood-pressure has been most marked there has been no headache. We think the real explanation of the disappearance of this disagreeable complication may lie in the fact that we have latterly used a smaller amount of novocaine in our splanchnic block.

One other possible explanation of the headache has occurred to me in that it resembles closely the headache which occurs after spinal puncture. In our earlier cases on a few occasions we tapped the spinal theca or a prolongation along the nerve, and cerebro-spinal fluid escaped; and this error in technique might furnish an explanation, especially as this complication has not arisen since we became more expert.

(3) *The Time taken in doing the Nerve Block.*—This is an undoubted disadvantage especially in hospital practice. But if the mortality after severe operations in the upper abdomen is lowered by the use of splanchnic anæsthesia, as we think it is, no one will grudge the extra half-hour spent in the anæsthetic room.

(4) *Regurgitation of Gastric Contents.*—One other slight complication has arisen in two of our cases which in fairness must be recorded. They were two gastric cases in which the stomach was not entirely empty at the time of operation. The cardiac sphincter being relaxed by the splanchnic block, when the stomach was handled by the surgeon gastric contents commenced to regurgitate. The passing of a stomach tube was however difficult as the patient was under very light general anæsthesia, and its introduction was at once resented. In neither case however did any harm result.

Advantages of the Method.

(1) *Absence of Post-operative Pulmonary Complications.*—In our combined cases —numbering ninety-one—we have only had one case in which there has been any post-operative lung complication. This is strikingly good, if we take into consideration the fact that a number of the gall-bladder cases, and some of those in which gastro-jejunostomy was performed, were done by this method, at the request of our surgical colleagues, owing to the existence of bronchitis or some other pulmonary complication. In none of these cases was the chest condition appreciably aggravated by the operation.

The one pulmonary complication which occurred was in the forty-first of my cases. The patient was a man of 59, on whom a partial gastrectomy was performed for a large gastric ulcer high up in the lesser curvature. It was a very difficult case, and the patient was in the theatre one hour and thirty-five minutes. He developed broncho-pneumonia on the second and died on the fourth day. He had extensive broncho-pneumonia of both lower lobes. He was medically a very fit and suitable subject for operation, which made the case all the more disappointing.

On the whole, therefore, we think that perhaps the most striking advantage of the splanchnic and abdominal field block from the anæsthetist's point of view is the absence of pulmonary complications in a class of case which has in the past been particularly prone to them.

(2) *Quiet respiration* and diminished abdominal movement with which it is associated. In elderly patients with emphysema and bronchitis, I am sure this is a most potent factor in preventing post-operative lung complications.

(3) *Perfect relaxation*, for which of course one relies entirely on the nerve blocks, until perhaps the closing of the abdominal wall in a long case.

(4) *Small amount of general anæsthetic used.*—The patient is very little upset after operation, and sickness is very greatly reduced.

Mr. C. Langton Hewer

said he had made a series of experimental injections of methylene blue in the post-mortem room. The results showed clearly that the solution travelled for a considerable distance in the retro-peritoneal tissues, and did actually surround

the sympathetic plexuses. He had then begun injections upon the living subject and had, up to date, anæsthetized eleven cases with the technique already described. The operations were: (*a*) Six partial gastrectomies; (*b*) one partial gastrectomy and cholecystectomy; (*c*) one excision of gastric ulcer; (*d*) two gastro-enterostomies; (*e*) one exploratory laparotomy and closure. All these patients made good recoveries with the exception of one partial gastrectomy case, in which the patient died twenty-four hours after operation. This death could not be attributed to the anæsthesia, as his condition at the end of operation was good, and he recovered in a few minutes from the extremely light general narcosis that had been given. He (Mr. Hewer) said he considered that shock in high abdominal operations was definitely diminished by splanchnic analgesia, and had found that in the series of cases given above the average pulse-rate at the end of operation was 91 compared with 120 in a similar series operated upon under a pure general anæsthesia.

Mr. Stanford Cade

said that his interest in splanchnic anæsthesia originated in seeing this method successfully applied to major abdominal surgery by Drs. Labat and Laborde in Professor Victor Pauchet's clinic in Paris. He had had personal experience of fifty cases of regional anæsthesia, which included twenty-seven cases of splanchnic anæsthesia for upper abdominal operations. Of these fifty cases he considered ten as failures, as the patient required a general anæsthetic in addition. The technique he employed was similar to that described by Mr. Hillman, with the difference that a larger amount of a weaker solution was used. He (Mr. Cade) used 160 c.c. of $\frac{1}{2}$ per cent. solution of novocaine for a bilateral splanchnic anæsthesia, believing that with a larger volume of solution a bigger area of retro-peritoneal tissue was infiltrated and a better anæsthesia obtained. The value of the method was shown by diminished mortality after gastrectomy, increased post-operative comfort, and absence of pulmonary complications. Experimental evidence of the beneficial effect of splanchnic anæsthesia was shown by comparing blood-pressure tracings of cats and rabbits on which various surgical procedures had been carried out with and without splanchnic anæsthesia. From these it was seen that afferent painful impulses from the viscera produced marked disturbances of the blood-pressure, pulse and respiration, and that these variations were eliminated by blocking the splanchnic nerves and semilunar ganglia. He pointed out that novocaine, even in dilute solutions, was not entirely devoid of danger. Some patients showed an idiosyncrasy to the drug manifested by tachycardia, cyanosis of the fingers and lips, and respiratory failure. Intravenous injection of novocaine was extremely dangerous as it produced apnœa; this had been repeatedly proved by experiments on animals. Combined with paravertebral anæsthesia, splanchnic analgesia was the method *par excellence* for nephrectomies and other renal operations, as it did not throw any additional strain on the opposite kidney. Secretion of urine was not affected by injections of novocaine.

Section of Anæsthetics.[1]

President—Mr. H. Edmund G. Boyle, O.B.E.

Chloroform Anæsthesia.

By H. P. Fairlie, M.D. (Glasgow).

I have chosen as my subject Chloroform Anæsthesia partly because I use it to a considerable extent, but principally because, at the moment, it is being subjected to much unfavourable criticism. With the arrival of new anæsthetics in more recent years, there are not wanting those who question the propriety of using it at all. I sometimes wonder, however, if there are anæsthetists who never use it under any circumstances. It certainly still claims a fair share of notice in the literature of anæsthesia. The time has not yet arrived for the omission of any reference to it from the standard text-books, which all, I think, devote a considerable amount of space to a more or less detailed description of chloroform anæsthesia. Medical journals reveal frequent reference to it, the *Proceedings* of this Section being no exception. I was interested at reading in an issue of the *Proceedings*, as recently as February of this year (1925), a paper by Mr. Hobday,[2] in which he states that he uses chloroform regularly in veterinary surgery and is prepared to maintain that it is the ideal anæsthetic for horses and dogs. Then we have had monographs on the subject, more particularly from the physiologist's point of view, e.g., the late Dr. A. D. Waller's "Chloroform in the Laboratory and in the Hospital," and Dr. A. Goodman Levy's "Chloroform Anæsthesia." Both of these works stimulate thought and, perhaps, criticism, but from neither book does one gather the impression that the author's opinions are in favour of discarding chloroform.

Before taking up the study of chloroform in greater detail, I will direct your attention to the more critical point of view. Criticism is, for the most part, directed to the depressant action of chloroform on the circulation, and to the high mortality which results during anæsthesia from this cause. It is on this ground that the claim for the use of other anæsthetics in preference to chloroform is advanced, and I will therefore briefly allude to the alternative anæsthetics at our command. One frequently hears expressed the desire for the "ideal anæsthetic," and this would rather indicate that those which are at present known do not quite satisfy all expectations. Whether the aspiration will ever be gratified is, I think, doubtful. The idea at the back of it implies the discovery of an anæsthetic which will have as powerful an action as any now known, but will pass out of the body without leaving any of the unpleasant sequelæ with which we are familiar. When we consider the requirements of modern anæsthesia, including, as they do, the abeyance of reflex action and muscle tone, in addition to the abolition of painful sensation, we realize that the demand for a drug which will achieve only these objects, without disturbing other functions, is not easily complied with. The anæsthetic which most nearly fills the place with respect to the latter condition is nitrous oxide gas. Gas and oxygen is a most valuable anæsthetic, but it falls short with respect to the requisite profundity of anæsthesia. As far as I have been able to gather from literature about them, the same objection applies to acetylene and ethylene, while additional complications, in the way of risks

[1] Provincial Meeting, held in the University Buildings, Manchester, June 27, 1925.

[2] *Proceedings*, 1924-25, xviii (Sect. Anæsthetics), pp. 1-6.

of impurities, explosiveness, and difficulty of handling, also enter. The various local anæsthetics, using the term in its widest meaning to include spinal and splanchnic anæsthesia, have a definite place, but one which does not rapidly encroach on the domain of general anæsthesia. Gas and oxygen in combination with local anæsthesia has had its enthusiastic supporters. But it is to ether that we turn as the most formidable rival of chloroform. It is not my intention at all to question the value of ether, as it occupies a very definite place. It is more the claim for its unrivalled supremacy over chloroform which I do wish to question.

To my mind, paradoxical as it may sound, the chief disadvantage of ether is its stimulating action. The accentuated type of respiration which it so frequently develops is not only an inconvenience to the surgeon—in some cases a serious one—but is also productive of fatigue, calling into play excessive muscular action. In addition to this the stimulant action on the circulation, particularly during a lengthy operation, however safe a type of anæsthesia it produces at the time, leaves the patient in a state of reaction on the withdrawal of the stimulus, and this just at the moment when the greatest call is made on the patient's reserves. It is not always easy for anæsthetists to follow up their cases and to make observations in the post-anæsthetic period, but those who do so cannot fail to be impressed with the change in the appearance of an ether patient in the hours immediately following anæsthesia. The fall in blood-pressure is considerable, and in my opinion a proportion of the post-operative mortality is due to this cause, a mortality, it should be noted, which is quite overlooked in the preparation of statistics of death from anæsthesia.

To cite one case in illustration, that of a woman of 35, thin, and decidedly below the average in physique. Operation: gastro-enterostomy for gastric ulcer. Anæsthetic: open ether. Anæsthesia uneventful, and of a good type, with satisfactory muscular relaxation. The respiration somewhat laboured, the pulse full, rapid and rather bounding, throughout. Anæsthesia lasted fifty-five minutes, and was quite devoid of any cause for anxiety. After return to bed the pulse was still rapid and increased to 140 per minute, becoming steadily weaker. The patient rapidly went down hill and died within twenty-four hours of the conclusion of operation.

In assessing the relative merits of ether and chloroform, cases such as the above ought not to be overlooked. I think that too much emphasis has been laid on the condition during operation, the aftermath having been ignored.

With regard to the period of anæsthesia itself there is, of course, no doubt that collapse and death are more frequent with chloroform than with ether. Statistics are provided in plenty to prove this. It is only fair, however, to remember that statistics ignore all personal factors. They include all cases and all administrators. I think if they could be sifted, the vast majority of fatalities would be found to have occurred in those cases in which the relatively inexperienced man was conducting the anæsthesia. It is too utopian to insist that only the specialist should administer anæsthetics, but I do think that, when the occasional administrator is called upon to give an anæsthetic, his choice should not be chloroform. The views which I express regarding chloroform presuppose that it is to be administered by one who has thoroughly studied the subject and appreciated the risks. I do not say that even under those conditions fatalities are impossible, but I believe that they are as infrequent as with any other anæsthetic which attains the same profound anæsthesia, due regard being paid to all aspects, the post-anæsthetic period included.

To turn now to the anæsthesia which chloroform produces, there is, I think, a fairly general admission, even on the part of its severest critics, that it is in certain respects superior to that obtained by any other general anæsthetic. It is more often possible to get a third stage, in which the breathing is quiet, the respiratory

excursion small, and the degree of muscular relaxation sufficient: these conditions are seldom attainable with ether. It is, perhaps, not a point of supreme importance in modern surgery, but there are still some operations in which the diminished hæmorrhage of a chloroform anæsthesia is useful. I do not pretend that in every laparotomy it is possible to produce those ideal conditions. But I do not think that on the average it is much more often possible to produce them with chloroform than with ether. Then, in the post-anæsthetic period, instead of a patient suffering from reaction after over-stimulation, we get one whose blood-pressure is tending upwards, and one who will respond to stimulation if it be necessary. In addition, there is less likelihood of chest complications supervening. I see it has been rather the fashion to belittle the irritant action of ether, and certainly ether is less irritating when given by an open method, and after the preliminary administration of atropine, than it was in the old days of closed inhalers, often badly used. Nevertheless, ether bronchitis is a factor to be reckoned with, at any rate in the West of Scotland.

As an offset to those favourable conditions there is the danger associated with the exhibition of chloroform, first, the danger from circulatory failure, and secondly, the danger of acidosis or delayed chloroform poisoning.

(1) *Chloroform Syncope.*—It has been the custom, of recent years, to describe two types of chloroform syncope, one coming on without warning and resulting in almost instantaneous death, the other of a more gradual type. I doubt whether there is any vital distinction. The cardiac failure which occurs in both types is of the same nature, and differs only in the rapidity of its onset. Even the rapidly arriving syncope does give some warning. It requires most careful observation to detect the premonitory symptoms, but, although they are rapid in their sequence, they can be detected, and are practically identical with those which occur in the cases of more gradual onset. The time available for remedial measures is more limited. Every second is valuable, but prompt response to the warning of the symptoms is sufficient to avert danger.

What are those symptoms? The two to which I assign most importance are pallor and dilatation of the pupil. They occur almost simultaneously and the very first moment at which they appear is the time to take measures. If, at that moment, the operating table be tilted, lowering the head and raising the feet, it is rarely necessary to do more to restore the circulation. The symptoms in question can be detected earlier than a definite weakening of the pulse or change in the respiration, although these quickly follow. It is notorious that this type of syncope is apt to occur early in the administration.

As regards the cause of the syncope, various explanations are offered. Levy, who has made a most valuable investigation into the whole subject, holds the view that it results from ventricular fibrillation, that it is especially liable to occur during light anæsthesia and that it is not due to overdosage. He advances the interesting theory that the suprarenal glands are, from varying causes which I need not detail, stimulated to secrete excessively during the early part of the administration of an anæsthetic, and that the resulting combination of adrenalin and chloroform, the danger of which is well known, is responsible for the ventricular fibrillation and resulting syncope.[1] With his conclusion that overdosage plays an insignificant part in the causation of syncope I cannot agree. Although cardiac failure usually occurs early in the administration, I am convinced, from clinical observation, that it results far more often from too much chloroform, than from too little. The Special Chloroform Committee of the British Medical Association (Report, p. 15) found that, in certain experiments, "the fatal dose was less than twice the amount required to produce unconsciousness to pain." Clinically, the following case may be cited

[1] A. Goodman Levy, "Chloroform Anæsthesia," p. 27.

in support of my contention, as it illustrates a sequence of events which occurs often enough to be worth attention.

A boy of 12 was undergoing an operation for right inguinal hernia; he was otherwise healthy. Chloroform was given by means of the Harcourt inhaler, which was gradually turned on to its maximum delivery, 2·5 per cent. The induction period was quiet, lasting six minutes. At eight minutes the incision was made. At nine minutes pallor and a slightly dilated pupil were noticed, the delivery of the Harcourt inhaler still being at 2·5 per cent. The chloroform was then withdrawn altogether, and none was given for two minutes. At the end of that time all cause for anxiety had disappeared, the colour had improved, and the pupil was now contracted and active. Further, and of interest as pointing to the danger of light anæsthesia, along with this general improvement there occurred reflex contractions of the flexor muscles of the right thigh in response to the painful stimulus from the wound. Those movements ceased on the resumption of chloroform administration, and the remainder of the anæsthesia, which lasted sixteen minutes, was uneventful.

As regards other theories of cardiac failure McWilliam has pointed out that in large doses chloroform acts directly on the heart muscle, the result being dilatation.

Then there is the theory advanced by Embley and others, that syncope is due to reflex vagal inhibition through action on the vaso-motor centres. In an interesting article,[1] Embley acknowledges the value of Levy's researches but alludes to his experiments as having been mostly on cats. He contrasts the behaviour of dogs and cats under chloroform, pointing out that while cats' hearts are very liable to fibrillation this feature is noticeably infrequent in experiments on dogs, in which syncope is of the vagal type. To quote from the paper:—

"The control of the cardiac rhythm in the dog is markedly vagal and approximates in this respect to that of the human. In the cat, on the other hand, marked vagal control is not characteristic, and such control is readily inhibited. As already shown, a heart under vagus control is not prone to ventricular fibrillation."

The question is of more than merely academic interest, as it bears directly upon the tactics to be adopted in using chloroform. The theory of reflex vagal inhibition appeals to me as more nearly fitting the clinical picture. I think that one of the chief dangers to be guarded against, particularly at the commencement of a chloroform anæsthesia, is overdosage. This is one of the points on which the Special Chloroform Committee placed great emphasis. In fixing the maximum concentration at 2 per cent. I think they were over-cautious. The Vernon-Harcourt inhaler, which was the outcome of their deliberations, sought to make possible the administration of chloroform within this 2 per cent. limit. I find it a most useful apparatus, one which I employ extensively, but not within the limits prescribed by its inventor. By heating the chloroform above the point he recommended, I obtain higher concentrations, up to 3 per cent. and 3·5 per cent., which in some cases are necessary. In addition to the control of vapour strength which the Harcourt inhaler secures it also maintains a fairly uniform concentration. Levy strongly stresses the importance of this, and on that point I am entirely in agreement with him. It adds to the danger of syncope if chloroform be given in rapidly varying concentrations.

Also, pointing in favour of the vagi playing a part in syncope, I think the preliminary administration of atropine is a safeguard in tending towards its prevention. I like patients to take $\frac{1}{100}$ gr. one hour before operation; children should be given a smaller dose.

There is one other important feature attaching to the action of chloroform, namely, the tendency to deficient oxygenation. Whether this be due to diminished

[1] *Lancet*, 1915, ii, p. 283.

power of the hæmoglobin to carry oxygen, as suggested by Buckmaster and Gardner, or to deficient ventilation, as maintained by Levy, the fact remains, that in deep chloroform anæsthesia there is a definite under-oxygenation. For this reason I think it is essential to have oxygen at hand, and in abdominal operations to use it almost as a routine.

The foregoing are the principal points in combating chloroform syncope.

(2) *Acidosis.*—The symptoms and progress of a typical case of so-called delayed chloroform poisoning are sufficiently alarming, but, fortunately, they are of rare occurrence. They are so uncommon that they need not weigh heavily in assessing the anæsthetic risk, except in one class of case, i.e., in the presence of sepsis, and especially of sepsis in the abdominal cavity. The danger arising from the combination of chloroform and, say, an appendix abscess, is so real as altogether to rule out the administration of chloroform in such cases.

In concluding my remarks I cannot do better than quote a passage from Waller's book,[1] already referred to, coming as it does from the pen of one who had very ample opportunity of weighing the danger of chloroform :—

"If I had to undergo anæsthesia unexpectedly at the hands of an unknown administrator, I should take ether. If I had to undergo anæsthesia after due warning I should take chloroform, but only at the hands of an administrator of known skill and experience."

Discussion.—Dr. J. BLOMFIELD said he differed from the views expressed in the paper, only as to the question of chloroform fatalities. These he believed to be of two distinct kinds, in one of which prevention was possible, and in the other not. The former were due to overdose, the latter occurred with light anæsthesia and without the intake of too much or too highly concentrated a vapour.

Dr. S. R. WILSON disputed Mr. Hobday's dictum regarding chloroform as the ideal anæsthetic for animals. Veterinary anæsthesia was in the same position to-day as that occupied by human anæsthesia twenty years ago. He agreed with Dr. Blomfield that there were two forms of chloroform syncope. If vagus control has any importance why give atropine as a routine measure?

Dr. W. J. MCCARDIE said that a chloroform-ether mixture varying in proportion with the type of case was the nearest approach to the ideal anæsthetic. When chloroform was given, sufficient was not done to ward off its dangers. The patient should undergo preliminary preparation with brandy, strychnine, atropine, digitalin and pituitrin. Ether was given for too long a time; half an hour's drop ether anæsthesia was enough for anyone.

Mr. G. R. PHILLIPS called attention to the construction of the Vernon-Harcourt inhaler in which special apparatus was provided to increase concentration up to 4½ per cent. without heat.

Dr. C. I. KIRKBY-THOMAS advocated routine lowering of the head as a precaution against heart-failure during chloroform inhalation, especially in gynæcological cases.

Dr. ALEXANDER WILSON said he had met with two patients who died on the table before any anæsthetic had been given. They died from circulatory failure similar to that which occurred after severe hæmorrhage. The symptoms were pallor and dilatation of the pupil, with convulsions. Artificial respiration and lowering of the head should be practised at once. Overdose of chloroform occurred in two conditions. In one there was violent struggling with deep inhalations and consequent overdose. In the other the overdosage was gradual. In the first, death was due to cardiac paralysis from over-exertion and not to chloroform. Nervous patients should never be given chloroform.

Dr. FEATHERSTONE said that he would not give chloroform without a clear reason being advanced for its choice. It took a long time to get chloroform out of the system, whereas in dangerous conditions with other anæsthetics recovery could quickly be brought about. Light anæsthesia at the beginning of operation was dangerous; and in the case of children there was also danger of collapse later, when they had been returned to bed. There was with

[1] "Physiology the Servant of Medicine" (Chloroform in the Laboratory and in the Hospital: Hitchcock Lectures, 1909), 1910, p. 97.

chloroform a fall in the systolic pressure, accompanied by a rise in the diastolic. The whole circulation was hampered not only by a failing output but also by increased peripheral resistance.

After the discussion of Dr. Fairlie's paper, Drs. S. R. WILSON and B. MCSWINEY demonstrated the Effects of Intravenous Injection of Adrenalin on Animals under Ether and under Chloroform. There was also an exhibition of Physiological Methods of Investigating Acidosis, and various New Anæsthetic Apparatus were shown.

SECTION OF BALNEOLOGY AND CLIMATOLOGY.

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Section of Balneology and Climatology.

President—Dr. WILFRID EDGECOMBE.

The Principles of Spa Treatment in Great Britain.

PRESIDENTIAL ADDRESS.

By WILFRID EDGECOMBE, M.D., M.R.C.P.Lond., F.R.C.S.Eng.

A SMALL handbook entitled "The Spas of Britain," issued by the British Spa Federation, gives information relating to the nine principal spas—Bath, Buxton, Cheltenham, Droitwich, Harrogate, Leamington, Llandrindod Wells, Strathpeffer and Woodhall Spa. The strictly medical particulars concerning each spa have been obtained from members of the medical profession, individually or collectively, at each place respectively; and therefore presumably represent their considered views. If we examine the book with the object of finding out the special diseases treated at these various spas, we are struck at once by the extraordinary unanimity of claims made by each of them. In order to present the facts concisely I have tabulated them, and show on the screen the list of diseases which each spa claims to treat with benefit. The names of the spas appear in alphabetical order.

BATH.

(1) Gastric disorders
(2) Gout
(3) Arthritis deformans, chronic rheumatism and fibrositis
(4) Gouty glycosuria
(5) Renal calculus
(6) Chronic infection of the urinary tract
(7) Anæmia

Cases for which Bath treatment is especially indicated.

(1) Articular and abarticular *Gout*
(2) The different forms of *Chronic Arthritis*
(3) *Fibrositis*, including sciatica and "rheumatic" neuritis
(4) Functional disease of the nervous system
(5) Certain organic diseases of the nervous system: Paraplegia and toxic neuritis
(6) Stiffness following injury to joints
(7) Functional and certain organic diseases of the heart
(8) Hypertension
(9) Colitis
(10) Certain pelvic disorders in women
(11) Various forms of chronic skin diseases
(12) Chronic forms of rhinitis, pharyngitis and laryngitis

BUXTON.

(1) Gout
(2) Gravel
(3) Rheumatism
(4) Fibrositis
(5) Sciatica and neuritis
(6) Rheumatoid arthritis
(7) Osteo-arthritis
(8) High arterial tension
(9) Arterio-sclerosis
(10) Cardiac affections
(11) Skin diseases
(12) Colitis

CHELTENHAM.

(1) High blood-pressure ; arterio-sclerosis
(2) Neurasthenia
(3) Gastro-intestinal intoxication
(4) Chronic gout
(5) Rheumatism ; fibrositis ; lumbago
(6) Obesity and plethora
(7) Chronic constipation
(8) Gastro-intestinal catarrh
(9) Torpid liver, biliary catarrh, gallstones
(10) Gravel
(11) Anæmia

DROITWICH.

(1) "Rheumatic" affections : lumbago, sciatica, neuralgia, neuritis, fibrositis
(2) Rheumatoid arthritis
(3) Gout
(4) Heart disease (muscular weakness)
(5) Nervous exhaustion
(6) Anæmia
(7) Some diseases of the spinal cord. Paralysis
(8) Sprain and injuries to tendons, muscles, and joints
(9) Convalescence fom acute illness

HARROGATE.

(1) Gout and other metabolic disorders (glycosuria, obesity)
(2) Functional liver derangement, and early cases of cirrhosis, cholelithiasis and cholecystitis
(3) Chronic skin diseases
(4) Synovitis, fibrositis, sciatica and other forms of neuritis
(5) Arthritis (rheumatoid, chronic rheumatic, and toxæmic varieties)
(6) Mucous colitis, chronic dysentery, constipation and intestinal toxæmias
(7) Anæmia in its various forms
(8) Nervous diseases (neurasthenia and hysteria)
(9) Hyperpiesia
(10) Sequelæ of tropical diseases, especially anæmia

LEAMINGTON.

(1) Disease due to faulty metabolism : gout, obesity
(2) Diseases of the alimentary system : dyspepsia, portal congestion and enlarged liver, mucous colitis
(3) Diseases of the circulatory system : arterio-sclerosis and hypertension. Some cases of valvular disease
(4) Diseases of joints : rheumatoid arthritis, sprain
(5) Diseases of muscles and connective tissues : lumbago, strains, fibrositis
(6) Diseases of the nervous system—
Organic : neuritis, tabes, hemiplegia, anterior poliomyelitis, the myopathies
Functional : neurasthenia, hysteria
(7) Pelvic congestion in females
(8) Skin diseases : gouty eczema, psoriasis, seborrhœic eczema, chronic urticaria, pruritus

LLANDRINDOD.

(1) Metabolic diseases : gout, glycosuria, obesity
(2) Rheumatism and fibrositis
(3) Injuries to joints and ligaments
(4) Chronic arthritis
(5) Dyspepsia and alimentary toxæmia
(6) Certain disorders peculiar to women
(7) Arterio-sclerosis in early stages
(8) Chronic skin diseases : eczema, psoriasis
(9) Neurasthenia and debility
(10) Anæmia
(11) The after-effects of tropical diseases

STRATHPEFFER.

(1) Chronic and sub-acute gout
(2) Rheumatoid arthritis
(3) Chronic rheumatism, lumbago, sciatica
(4) Chronic skin diseases : eczema, psoriasis, impetigo
(5) Chronic digestive disorders : intestinal cartarrh, biliary cirrhosis
(6) Chronic disorders of the bladder and urinary tract
(7) Certain chronic uterine complaints
(8) Anæmia, debility
(9) After-effects of tropical diseases, malaria, dysentery

WOODHALL.

(1) Diseases of the joints: rheumatism, gout, rheumatoid arthritis, osteo-arthritis, synovitis, injuries
(2) Diseases of the fibro-muscular tissues: muscular rheumatism, fibrositis, lumbago, stiff neck, pleurodynia, myalgia
(3) Diseases of the nerves: neuralgia, neuritis, sciatica, neurasthenia, hysteria, paralysis
(4) Diseases of the circulation: high blood-pressure, arterio-sclerosis, heart disease and functional weakness, phlebitis
(5) Diseases of women: congestion of pelvic organs, leucorrhœa, sterility, tumours
(6) Diseases of the skin: psoriasis, eczema, urticaria, acne, pruritus
(7) Diseases of the mucous membranes: nose, throat, intestine
(8) Diseases of the digestive organs: liver derangement, jaundice, gall-stones, obesity
(9) Enlarged glands: scrofula, rickets, goitre

	Bath	Buxton	Cheltenham	Droitwich	Harrogate	Leamington	Llandrindod	Strathpeffer	Woodhall
(1) *Metabolic Diseases:* Gout	+	+	+	+	+	+	+	+	+
(2) *Arthritis:* Infective and rheumatoid	+	+	—	+	+	+	+	+	+
(3) *Chronic "Rheumatism":* Fibrositis, lumbago, sciatica	+	+	+	+	+	+	+	+	+
(4) *Alimentary:* Catarrh; toxæmia; colitis, &c. Constipation	+	+	+	—	+	+	+	+	+
(5) *Liver:* Congestion; cholecystitis and lithiasis	—	—	+	—	+	+	—	+	+
(6) *Circulation:* Hypertension; arterio-sclerosis; heart	+	+	+	+	+	+	+	—	+
(7) *Nervous System:* Organic lesions	+	—	—	+	—	+	—	—	+
Functional	+	—	+	+	+	+	+	—	+
(8) *Chronic Skin Diseases*	+	+	—	—	+	—	+	+	—
(9) *Anæmia and Debility*	+	+	+	+	+	—	+	+	—
(10) *Bladder and Kidney:* Gravel	+	+	+	—	—	—	—	+	—
(11) *Post-tropical Conditions*	—	—	—	—	+	—	+	+	—
(12) *Diseases of Women*	+	—	—	—	—	+	+	+	+
(13) *Scrofula: Rickets*	—	—	—	—	—	—	—	—	+

The last table presents the facts in a different way. In the margin are the various diseases conveniently grouped: in the vertical columns a plus sign under each spa indicates a claim to treat that particular group of diseases; while a minus sign indicates that the disease in question is not mentioned among those claimed to be treated at that place.

You will observe that two groups of diseases, the metabolic disorders (gout, &c.), and the "chronic rheumatic" diseases are common to all spas. Three other groups—arthritis, alimentary diseases, and circulatory disorders—are common to all, with one exception in each case; arthritis, all claim to treat except Cheltenham; alimentary diseases, all except Droitwich; and circulatory disorders, all except Strathpeffer. Three other groups—skin diseases, anæmia, and functional nervous disorders—are common to all except two spas in each case; skin diseases are omitted by Cheltenham and Droitwich; anæmia, by Leamington and Woodhall; and functional nervous disorders, by Buxton and Strathpeffer. In the case of disorders of the liver there are three exceptions, Bath, Buxton, and Droitwich; diseases of women, four exceptions, Buxton, Cheltenham, Droitwich, and Harrogate. The claim to treat organic nerve lesions is made by four spas, bladder affections by four, post-tropical conditions by three, and scrofula and rickets by one only, viz., Woodhall Spa.

Now, there are two conclusions to be drawn from these facts. Either the claims made are extravagant and cannot entirely be substantiated—in which case they stand urgently in need of revision—or they are in the main true, in which case it must follow that there can be nothing specific in spa treatment in this country. (I

say nothing here of foreign spas, at some of which strong claims are made for a definite specific action of certain waters in certain specific diseases.) The point must be stressed that though these claims are put forward in the handbook by the *lay* management of the different spas, in each case the purely medical data on the nature and action of the waters, and their therapeutic indications, have been drawn up by representatives of the profession practising at the respective spas, and have been considered and passed by the medical societies in those places where such a body exists. They, therefore, must be taken to represent the considered opinions of the medical men, based on their accumulated clinical experience extending over a long period of time.

Assuming, then, that these claims are in the main justified, it must follow that there can be nothing truly specific in the treatment of any of these disorders at any one of the spas. It is absurd to suppose that such diverse waters as those of Bath and Harrogate, for example, can be equally specific for the average case of gout; or those of Cheltenham and Woodhall, or of Buxton or Droitwich. Yet, they all claim to treat gout with beneficial results. Similarly, with the group of "chronic rheumatic" diseases, which all the spas claim to treat, and so on through the list. If one attempts to formulate clear and precise indications as to which case of gout, or of arthritis, or of rheumatism, or of colitis, or of any other disease, should be sent to which spa, great difficulty is encountered. These indications can only be stated in a general, somewhat nebulous, way; but no definite rules can be laid down owing to the absence of specific data to serve as a basis, for the claims founded on clinical experience alone are apt to be too vague and too wide.

Experimental Work.

Are there any data obtainable founded on experimental work? I have communicated with leading medical men at each of the spas asking for information on and reference to any direct experimental work that may have been done on the action of their particular mineral waters and baths.

From *Bath*, Dr. Gordon writes: "I do not think there is any published work. Sir Wiiliam Ramsay made a report which was printed privately. Munro has done some work, but has not yet published it; and Raiment, of Oxford, did some work, but did not publish it, with relation to Bath waters, though his paper on sweat secretion was done during his work on the Bath waters." Sir William Ramsay's work was presumbly on the radium content of the Bath waters. So far as I am aware, no experimental work has been done to determine the biological action of the water with or without its radium emanation. In the *Lancet*, of 1922, Munro [1] published a paper, entitled "Studies from the Royal Mineral Water Hospital, Bath," dealing with subacute and chronic multi-articular arthritis, and with the blood picture in gout, but as no reference is made to treatment by baths or waters, the paper falls outside the scope of this address.

Recently, I understand, an investigation has been in progress on the diuretic action of the Bath waters, but so far I have seen no published results.

From *Buxton* I am indebted to Dr. C. W. Buckley for the following particulars: In 1911 an examination of the radio-activity of the Buxton waters was undertaken by Markower, who reported the presence of radium emanation in considerable amount. Up to 1923 no experimental work of moment on the waters and baths appears to have been done. In March of that year a bio-chemist, Mr. Joseph Race [2], was appointed to the Devonshire Hospital and since then active experimental work has been going on. The water of the St. Ann's Well has been proved experimentally to have a pronounced diuretic effect as compared with distilled water. Further investigation showed that this effect is largely due to the gases and the radium emanation present in the water. There is evidence also that the diuretic effect is enhanced by the simultaneous use of baths along with the waters internally.

A laborious piece of experimental work has been undertaken to determine the effect of the waters on nitrogenous metabolism. In a series of cases of gout it was shown that ingestion of the water was followed by a reduced output of uric acid in the urine. A preliminary rise occurred in the uric acid of the blood, followed after cessation of taking the water by a marked reduction in blood uric acid. These results were explained as follows: The initial rise in blood uric acid was thought to be due to mobilization into the blood-stream of uric acid from the tissues, and the subsequent fall as due either to increased destruction, or to diminished formation of uric acid in the tissues. Further investigations are in progress, I understand, in corroboration and elucidation of these results. Of the bacteriological work in cases of arthritis that is being actively carried on at the Devonshire Hospital, Buxton, it is unnecessary to speak here: my purpose is confined to experimental work on waters and baths and accessory treatments.

From *Cheltenham* Dr. Kirkland writes: "I cannot find any evidence of direct experimental work on the waters and baths of Cheltenham. Forms are sent out to each medical man in the town, and upon these the nature of the malady and clinical notes of cases are recorded. These are sent to the honorary secretary and summarized, but they are simply clinical observations."

Of *Droitwich* Dr. Wilkinson informs me that "no research work has been done on the Droitwich brine. A good many years ago Roche and I got Soddy to estimate the radio-activity, and he reported it to be marked."

From *Leamington* I have no reply, and have failed to find any record of experimental work done at this Spa.

From *Llandrindod* Dr. Ackerley writes: "So far I have not been able to get any report of scientific work done here."

From *Strathpeffer* Dr. Bearn writes: "I have searched the records, but am afraid there is no record of experimental work done here. I believe that Dr. Kaye did some and published it, but I can find no trace of it. The work was, I believe, on the effects of baths on blood-pressure."

From *Woodhall Spa* Dr. Leonard Boys writes: "I am afraid I can give you no details of any direct experimental work done on the Woodhall waters: nor do I believe that any such work has been attempted at present."

At *Harrogate* a good deal of experimental work has been done by various individuals during the last thirty years. The earlier work of Oliver was directed more to the physical and chemical side: the discovery of new sources, and in conjunction with Hayton Davis, the chemical analysis of existing springs. His work on the vascular system, on blood-pressure, and on tissue lymph was of a more general character and not specially in relation to mineral waters and baths.

The first bio-chemical investigation that I can find was made by Dr. Liddell [3], in 1893, on the effects of the waters on the excretion of urea and uric acid. To patients kept on a fixed diet he gave the strong sulphur water in the customary doses before breakfast, followed by the magnesia water during the day. The result was an increase in the excretion of urea, and a diminution, after a transitory rise, in the output of uric acid. These findings he explained as due to a stimulation of metabolism, accounting for the increased output of urea, and to lessened production of uric acid, accounting for the reduced excretion.

In 1896, Dr. Liddell and Dr. Edgecombe made a series of observations which were presented at the Annual Meeting of the British Medical Association, held in that year at Carlisle, on the effect of sulphur baths and artificial Nauheim baths in a number of heart cases. The results showed that the still saline-sulphur baths had an effect similar to that of the still Nauheim baths at the same temperature.

An important investigation was made in the year 1898 by Dr. Bain [4], and published in the *Journal of Anatomy and Physiology*, on the action of the old sulphur water on the secretion of bile. Experimenting on a human subject with a biliary

fistula he found that the administration of the old sulphur water caused a notable increase in the quantity and total solids of the bile secreted, an increase greater than that produced by any other mineral water or drug tested on the same subject. These included the Kissingen water, Carlsbad water, the strong Montpellier sulphur water, of Harrogate, and the chloride of iron water, and of drugs, euonymin, benzaote of soda, salicylate of soda, iridin, podophyllo-resin, and podophyllo-toxin. From these results it was inferred that the old sulphur water has a stimulating action on the liver functions generally.

An attempt was made by the same investigator [5] to ascertain the ingredient to which the old sulphur water owes its action as a cholagogue. Experimenting upon dogs, after establishing a biliary fistula, he injected sulphur water into the circulation. The result was a marked increase in the output of bile, thus confirming the results obtained in the human subject. The injection singly of various constituents of the sulphur water gave inconclusive results; the evidence pointed to barium chloride and sodium sulphide as being the constituents mainly concerned.

In 1899 Dr. Bain and Dr. Edgecombe [6] made an investigation into the action of the sulphur waters and baths on the blood and on the excretion of urea, sulphates, phosphates, uric acid and total acidity: the results were published in the *Journal of Physiology* for that year. They found the administration of the old sulphur water caused an increase in the excretion of urea and a diminution in that of uric acid, sulphates, and of P_2O_5. The diminution in the sulphates was held to be due to purgation, and the same explanation holds good for the decrease in uric acid, namely, that it is due to removal of its antecedents from the alimentary canal. On the blood the effect of the water was to produce a notable fall in the worth of the corpuscle, or loss of hæmoglobin, a result that was proved to be due to the H_2S in the water, for when this was removed and the experiment repeated, no such fall took place. A point of interest was the effect of the water upon the diurnal fall in the value of the corpuscles that normally takes place, the evening reading being almost invariably lower than the morning reading. While the water is being taken, this variation becomes reversed, the worth of the corpuscle being lower in the morning than at night, this suggesting that when the worth is rapidly lowered there is a tendency to conservation of the lessened amount of the hæmoglobin, so that the further loss that would normally take place during the day does not occur, or may even be replaced by a rise.

Another interesting fact that emerged during the course of the observations was an inverse ratio between the number of leucocytes present in the blood and the amount of uric acid excreted in the urine.

In the same year these two observers made a lengthy series of observations on the effects of baths, massage and exercise on the blood-pressure. The results were published in the *Lancet* for 1899 [7].

In 1899 a series of experiments were made by Dr. Bain on the solvent action of various mineral waters on biurate of soda. The results, published in the *British Medical Journal*, 1899 [8], showed that the solvent action on biurate of soda *in vitro* of the magnesia water of Harrogate more nearly approached that of distilled water than any of several other British and foreign mineral waters tested; and the same result occurred when the solvent power was tested in a medium of artificial blood serum—the magnesia water showed a greater solvent power on biurate of soda than any of the mineral waters examined.

In 1900 some experiments were made by Dr. Bain [9], on the effect of superheated air baths on the temperature of the blood. Using anæsthetized dogs, he found that the Greville bath of dry non-luminous heat at 300° F. applied for forty-five minutes to the hind limb produced a rise of 1·8° F. in general body temperature, and a rise of as much as 6·6° F. in the venous blood returning from the heated limb. With the luminous heat in the Dowsing apparatus the effect was more pronounced. An

exposure of the limb for forty-five minutes to a temperature of 250° F. caused a rise of 3·5° F. in general body temperature, and a rise of no less than 15° F. in the venous blood returning from the exposed limb.

In 1905 Dr. Bain made a further investigation into the action of the sulphur water on metabolism, with results which corroborated the earlier findings, viz., an increased output of urea, and diminished excretion of uric acid and sulphates. The results appeared in the *Scottish Medical and Surgical Journal* for 1905 [10].

In this year—1905—Dr. Bain, Dr. Edgecombe and Dr. Frankling made an investigation into the effect of certain baths and electrical procedures on the blood, blood-pressure, and metabolism, the work being published in the *Lancet* [11], and supplementing similar work done in 1899. They demonstrated the effects of immersion baths, hot and cold, of mineral water baths, of needle douches, and of massage douches on the blood pressure, and the effects of massage douches, Aix and Vichy, and of hot-air baths on metabolism. The Aix and Vichy douches caused an increase in the output of urea and uric acid, while the hot-air baths caused a reduction in the excretion of urea, due to free sweating, and a rise in the output of uric acid.

In view of the large amount of chlorides present in some of the Harrogate waters, Dr. Bain in 1909 made an inquiry into the excretion of chlorides in cases of gout. He found that gouty cases with normal kidneys—as evidenced by a urine of good specific gravity and free from albumin—were able to excrete chlorides given in excess to an extent equal to that of normal individuals. The results appeared in the *Lancet* for 1909 [12].

The next important piece of work was a prolonged, laborious and thorough investigation undertaken by Dr. David Brown into the pharmacological action on a normal subject, and on cases of gout, of the strong sulphur water. The results appeared in two papers published in the *British Medical Journal* for 1910 [13], and in the *Proceedings of the Royal Society of Medicine* for 1911. Briefly summarized, the action of the water was shown to be one of increased oxidation and tissue change. The profound effect on general metabolism may be inferred from the following findings :—

(1) An increase, up to 10 per cent., of the total nitrogenous output of the urine. (2) A 10 per cent. increase in the excretion of total phosphates. (3) An increase in the total solids of the urine. (4) An increase of 13 per cent. in the excretion of endogenous creatinin. (5) A large increase, up to 18 per cent., in the excretion of endogenous uric acid. (6) An increased excretion, to double the normal amount, of an exogenous purin body, hypoxanthin, administered in measured doses by the mouth. (7) A diuresis of 40 per cent. and upwards in physiological and in gouty cases.

In 1921 an experimental investigation was undertaken by Dr. Bain, Dr. Edgecombe, Dr. Kidd, and Dr. Sinclair Miller, into the action of certain electrical treatments on the blood, blood-pressure, and metabolism. The procedures studied were the D'Arsonval high-frequency current, the diathermic current, and the Bergonié treatment for obesity, and the findings appeared in the *Lancet* for 1921 [14]. Briefly summarized they were the following : The high frequency current caused a slight but definite reduction in systolic and diastolic blood-pressures ; a temporary trifling loss of hæmoglobin ; a more enduring fall in leucocytes ; an increase in the output of urea followed by an increase in the excretion of total nitrogen. The clinical claims made that this form of electrical treatment reduces the blood-pressure and stimulates metabolism are substantiated to a moderate extent by these results.

The diathermic current caused a slight fall in systolic pressure, an increase in leucocytes, and an increase in urea and ammonia nitrogen. These changes are very similar to those produced by the D'Arsonval current, which is to be expected, seeing that both procedures are forms of high frequency current. The only notable difference is in the behaviour of the leucocytes—diminished by the D'Arsonval,

increased by the diathermic currents. In view of the local heat-producing effect of the latter, this is an interesting fact which tends to substantiate its clinical use in promoting the absorption of local inflammatory products.

Summing up the effects of the Bergonié treatment, the current, without heat, causes a slight rise in hæmoglobin value; when heat is added a marked fall takes place. The blood-pressure is distinctly raised by the current alone, to an extent which suggests caution in the employment of this form of treatment in subjects of obesity with a high blood-pressure. The addition of heat neutralizes the rise and converts it into a slight fall, a fact which favours the employment of heat in addition to the current in these cases. Conversely, in cases requiring sweating baths in which a fall in blood-pressure is undesirable, this effect of heat may be prevented by the simultaneous use of the rhythmical current.

Judged by the urine analyses, the Bergonié current appears to have a definite effect in increasing metabolic changes, though not to the extent, at any rate in these experiments, that might *a priori* have been expected. As regards weight, the results were variable: no constant loss was observed.

In 1923 Dr. Edgecombe found that the hypodermic injection of minimal doses of strong sulphur water caused a definite polymorphonuclear leucocytosis. Control experiments with normal saline showed similar effects to a lesser degree.

Recently in 1923 and 1924 Mr. Woodmansey, analyst to the Harrogate Corporation, and Mr. Lissimore, technical assistant at the pathological laboratory, made an inquiry into the bactericidal properties of the old sulphur water, and of the Harlow Car alkaline sulphur water. The results were published in the *Archives of Medical Hydrology* [15]. They showed (1) that the old sulphur water is sterile; (2) that it exerts a marked bactericidal action on the *Bacillus coli*, the *Streptococcus fæcalis*, and the *Staphylococcus aureus*—the three organisms tested; and (3) when deprived of its sulphur content, the bactericidal property largely disappears. The Harlow Car water, chiefly used for bathing, was shown to have a similar bactericidal effect on the same organisms.

As they do not directly concern experimental work on waters and baths, no more than a passing mention need be made of the researches into the bacteriology of the arthritic diseases that is being carried on in the laboratory of the Royal Bath Hospital, Harrogate; also of the series of observations recently made by Dr. Geoffrey Holmes, and as yet unpublished, on the basal metabolism of a number of cases of rheumatoid arthritis.

Such is a brief outline of all the direct experimental work I have been able to collect on the action of mineral waters, baths, and accessory treatments, that has been done at the various spas. Valuable though the work may be as indicating certain definite actions of certain waters and procedures on certain organs of the body, it cannot be said that any fact has been established even remotely resembling a specific action in any one disease. We are therefore driven to the conclusion that spa treatment in this country, if it be beneficial in the variety of diseases for which its value is claimed, must be so by reason of certain broad general principles on which it is founded.

Chronic nutritional disease may be brought about by prolonged disturbance of one or all of three stages of the process of nutrition, which are sequentially dependent each on the other. The primary or alimentary stage is governed by the character of the food ingested, and the capability of the digestive organs. The intermediate stage of storage depends on the functional activity of the liver and its power of destroying toxins. The third or distributive stage depends on the proper interchange of fluid between the tissues and tissue-fluid, and between the latter and the blood; and this in turn depends largely on the soundness of the blood-vessels, and on their capability of contraction and dilatation. In other words, it depends largely on changes in blood-pressure. The first two stages are clinically investigable by various means.

The performance of the last stage cannot be directly observed, but may be inferred from the changes that take place in the capillary circulation as deduced from observation of the arterial and venous blood-pressure. In so-called Spa treatment, the main factors consist of the administration of waters, of baths, appropriate dieting, and exercise, active or passive. Three of these influence the body primarily by changes induced in the circulation: (1) Waters, mainly by their diluent effect on the blood, for the specific action of the medicinal constituents of the waters is in most cases—with a few notable exceptions—of secondary importance; (2) baths, entirely by the changes they excite in the circulation, for there is no proof that mineral constituents of waters used for bathing are absorbed by the skin; (3) exercise, by stimulating the circulation and promoting the flow of lymph. Diet, though it mainly concerns the primary or alimentary stage of nutrition, has a marked effect on the blood-pressure, and through the blood-pressure it influences the final or distributive stage.

I would state the broad principles as follows: The aim of spa treatment is to regulate faulty tissue interchange, rendered abnormal by excessive or deficient intake, by defective or excessive output, by lack of exercise and fresh air, or by the action of poisons, chemical or bacterial. Improvement of interchange between blood and body fluids leads to improved metabolism, increased resistance to disease, and increased power of recovery.

The modes of promoting these aims are:—

(1) The securing of increased elimination of waste products through the bowel, by means of waters having an aperient action, through the kidneys by waters having a proved diuretic effect, and through the skin by means of baths having a diaphoretic action.

(2) Improvement in the circulation and acceleration of the flow of tissue lymph, thereby enhancing the rate of dislodgment of waste products from the tissues and the supply of fresh nutriment from the blood. Such quickened metabolic changes are brought about by mineral water baths, massage douches, and other bathing procedures.

(3) The resulting changes in general metabolism lead to increased resistance to disease and to increased power of recovery. (Recent research tends to show that the bactericidal power of the blood may be raised by radiant light and heat baths, and by procedures such as the liver pack, that cause a powerful stimulation and reddening of the skin.)

(4) The regulation of the intake of food according to the needs of the particular case.

(5) The regulation of the amount of exercise or of rest.

(6) The securing of conditions having a psychological effect, the importance of which is not to be underestimated, such as change of scene and surroundings, absence from work and worry, and suitable congenial occupation or recreation.

(7) Lastly, the removal, where possible, of the cause of the disease; for example, the removal of any discoverable source of local infection.

On these principles, then, there are three broad types of treatment applicable to the various chronic diseases such as are sent to spas.

(1) *The anabolic type*, where the aim is to build up a deficient metabolism by increasing the intake, by promoting assimilation and by avoiding the production of excessive output. Examples of cases suitable for this type of treatment will readily suggest themselves: (*a*) convalescence from acute diseases; (*b*) anæmia and general debility; (*c*) post-tropical conditions; (*d*) functional nerve disorders; (*e*) chronic infective diseases such as rheumatoid arthritis.

(2) *The metabolic type* of treatment for cases in which metabolism is not necessarily deficient or excessive, but is defective and faulty and requires, in consequence, "alterative" treatment. Such as (*a*) the gouty, (*b*) the worn-out and jaded from over-

work and lack of exercise, (*c*) the hyperpietic, (*d*) the functional disorders of the liver, (*e*) and a large proportion of the chronic rheumatics and fibrositics.

(3) *The katabolic type* of treatment, where the aim is to promote over-stimulation of the metabolic processes, and allow tissue waste to overbalance tissue regeneration, as in the obese, the over-fed and the under-exercised.

Most of the diseases scheduled in the tables I have shown will be found fundamentally to fall into one or other of these three groups, and to be amenable to treatment on the principles I have laid down for each. These principles may be carried out equally well in most spas in possession of the necessary equipment. Naturally, spas will differ in their modes of application of the principles; one spa having waters more suitable for eliminant action by the kidneys; another, whose waters act on the bowel; another, where the waters have a proved action on the liver, and so on. Also the factors of climate, geographical situation, and time of year must be considered in the choice of a spa. But the principles remain the same and are acted upon in so far as the resources of a given spa permit.

To illustrate each of these three types of treatment the following cases may be briefly narrated:—

The Anabolic Type.—Miss W., aged 37, seen in 1921. Previous history good: no serious illnesses; had a mild attack of jaundice 1915, and again in 1917; subject to occasional mild "bilious attacks."

Her complaint was loss of appetite, loss of weight and strength, inability to walk far, whereas previously she had been a good walker. Her symptoms dated back for over a year and she had had all kinds of treatment without benefit. In appearance she was thin and ill-nourished: of sallow muddy complexion with a yellowish tinge. Her weight was 6 st. 2 lb.

On careful examination no disease was discovered. There was slight tenderness over the epigastrium: the liver was neither enlarged nor tender. The lungs and heart and kidneys were normal: blood-pressure low, 54/90. An X-ray examination after a bismuth meal showed slight delay in emptying of the stomach and a moderate degree of stasis in the colon.

She was put upon Kissingen water, liver packs, high frequency current and a careful diet. After six weeks' treatment she had improved to the extent of being able to walk eight miles without fatigue. She looked better, had gained 5 lb. in weight, and her appetite and digestion had greatly improved. Her blood-pressure, however, remained low: 54/92.

In 1922 she came again, looking better and having had a good year. Her weight was 6 st. 7 lb.: her blood-pressure still low—62/90: she had a similar course of treatment with the addition of massage douches.

In 1923 she reappeared, now weighing 7 st. 2 lb. She had had only one slight bilious attack during the year, and she felt fairly well. Her blood-pressure remained low—60/95. Another course of treatment resulted in further improvement.

This year, in 1924, she came again looking in perfect health, weighing 8 st. 2 lb., having gained two stones in all, with a blood-pressure of 75/110, able to do anything and to lead a normal life.

Here was a case of long-standing malnutrition from some unexplained cause, probably of gastro-intestinal origin, that had resisted home treatment. The stimulus given to her metabolism by spa treatment on "anabolic" lines initiated an improvement which was maintained until she gradually progressed to perfect health.

The Metabolic Type.—A man, aged 40, leading a sedentary life and living well, came complaining of breathlessness, fatigue and general malaise of some months' duration. His previous history was good: he had had no illnesses. In appearance he was pasty, with flabby and toneless muscles; he weighed 13 st.

On examination, his heart was slightly enlarged, no bruit, blood-pressure high 145/190, vessels soft, liver slightly enlarged and tender, kidneys sound.

Under a course of strong sulphur water, liver packs and massage douches he greatly improved, and after a month's treatment his blood-pressure was 130/170: and his symptoms greatly relieved. He was instructed to modify his diet, to take periodically a mercurial pill, and to lead a more active life. He came again each year for the following four years and had a repetition of his course. During this time his blood-pressure gradually fell until at the finish

it was 100/130; heart normal in size. He had no symptoms and was quite well. I saw him again six years later, i.e., ten years from his first appearance, and he had remained perfectly well, the blood-pressure maintaining its normal level of 100/130.

This was a case of hyperpiesia, caught in the early stage and cured by a re-adjustment of his metabolic processes, initiated by spa treatment and maintained by subsequent remodelling of his mode of life.

The Katabolic Type.—Mrs. M., aged 51, seen in December, 1923. She had always been an active healthy woman; had had no illnesses of importance. During the last two years she had put on 5 st. in weight—the normal weight varied between 12 and 14 st.; she now weighed 20 st. 2 lb.

On examination no disease was found; all the organs were normal; there were none of the classical signs of myxœdema. The heart was sound; blood-pressure 110/160; menstruation regular. She was put on to a rigid diet, a course of strong sulphur water, the Bergonié treatment, massage douches, and vigorous dry massage, and given thyroid extract by the mouth. In a month she lost 2 st. in weight. In three months her weight was down to 17 st., when the treatment was interrupted by an acute attack of appendicitis, for which she was operated upon. She made a good recovery, and afterwards resumed the reducing treatment, till in September, nine months from the commencement, she weighed 15 st., and was an entirely different woman, able to take exercise and enjoy her former active life. Her blood-pressure came down to 80/130.

Here was a case of obesity, in a middle-aged woman, of somewhat rapid development and unexplained cause, greatly improved by treatment on "katabolic" lines.

It is true that these broad principles are equally capable of application at home, by the use of drugs, and by physical modes of treatment. Home treatment, however, lacks certain manifest advantages possessed by treatment at a well-equipped spa. The necessary physical methods are rarely available at home. At a spa the concentration on the spot of a wide choice of physical procedures, by waters, baths, and accessory treatment, the change of scene and climate, the removal from business worries and anxieties, the opportunities provided for rest and exercise according to need—all these are factors to be considered in weighing home as against spa treatment.

My final conclusion is this: That in the present state of our knowledge of the precise action of our mineral waters and baths, we, as practising physicians, should lay no claims to the successful treatment of chronic disease by the specific action of these agents, but should rather stress the fact that we claim to treat these diseases on the broad general principles I have sketched. Briefly, that we act as general physicians, making use largely of physical methods, in addition to ordinary therapeutic treatment, rather than as purely spa physicians, putting our faith in the specific action — as yet unproved — of any particular water or bath.

It may be said that my argument shakes the foundation on which the edifice of spa treatment is supposed by many to rest. On the contrary, I am seeking to replace insecure foundations, constructed of unproved material, by others of a more durable character.

I am far from belittling the value of clinical experience as opposed to experimental findings, but I would assert that much of the clinical experience of the value of certain waters in certain diseases rests on somewhat nebulous data. Too little careful observation is made, and there is too little publication of results to enable them to be checked by independent observers. Nor am I blind, on the other hand, to the many fallacies underlying experimental work. Faulty methods, errors in technique, difficulty in controlling conditions, all conspire to vitiate results; and when results are obtained, there is the difficulty of their correct interpretation. Clinical experience, backed by experimental work, is immeasurably strengthened, and my plea is for more

research into the nature and action of the mineral waters of our spas. All too little has been done so far, and unless and until further work establishes specificity, or corroborates the full claims that many of us feel to be wholly extravagant, let us frankly discard these claims and concentrate on the broad unassailable principles.

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Section of Balneology and Climatology.

President—Dr. Wilfrid Edgecombe.

Clinical Types of so-called Multiple Infective Arthritis.

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No satisfactory classification of joint disease can be accomplished until the ætiology of many types is more fully established. It is necessary, however, to have a simple elastic grouping, liable to review from time to time, and to work out the less understood varieties of joint lesions in order to be able to pigeon-hole these correctly when more is known of them.

As a step towards this goal a mass of joint affections, habitually aggregated under the titles of rheumatoid arthritis or infective arthritis but embracing diseases of widely diverse origin, is discussed, so that the results of future research may be reported and rationalized in a sane manner. It is hoped to show the necessity for union between clinician and biochemist on the one hand, with bacteriologist and pathologist on the other, the present position being not one of divorce but of disengagement. It may be stated at the outset that nearly every point raised is controversial and that this is but an honest attempt to focus attention on a subject which needs much revision if any ultimate results are to be achieved.

The rubbish heap requiring particular attention at the moment consists of the following entities: (1) Periarticular fibrositis, (2) bursitis and synovitis, (3) atrophic arthritis, (4) multiple non-specific arthritis, (5) menopausal and villous arthritis. It must be stated at once that it is impossible to clear one corner of this dump completely, namely, multiple non-specific arthritis, but some sort of order can be attained elsewhere.

Periarticular Fibrositis.

This condition, originally described by Jaccoud, falls properly into the classification of fibrositis and therefore is not discussed here, but is merely referred to in order to avoid confusion.

Bursitis on the other hand is apt to be a little confusing in that this may either be a separate clinical entity, such as subacromial bursitis described by Buckley [1], or may complicate an arthritis. In the latter instance the primary arthritis is the important factor.

The same may be said of synovitis: possibly the only instance in which it is difficult to place synovitis occurs with regard to the type referred to by Poncet as "tubercular rheumatism," in which a recurrent synovitis is complicated by rheumatic nodules. It would appear that this type is simply the expression of a subacute rheumatic infection and is in fact the condition likely to obtain in a smouldering infection liable to exacerbations, such as occur in the commoner experience of rheumatic infection in childhood. The joint surfaces, as in rheumatic fever, are not damaged unless there is a secondary infection, or unless villous arthritis develops.

Atrophic Arthritis.

This name has been chosen in preference to others such as true rheumatoid arthritis, the Poncet type, &c., from the fact that atrophy is one of the most conspicuous features.

The salient points of this affection are the following:—

(1) It is a disease of obscure origin, usually insidious or subacute in onset. Almost exclusively if not entirely attacking young females.

(2) There are no obvious infective foci: that is to say that the tonsils are not septic and the teeth are usually excellent (for research purposes any case of this type

is rejected if there are gaps in the teeth). The urine is normal, and no signs of chronic appendicitis or of other gross abnormalities are found.

(3) Atrophy is conspicuous; there is both local and general wasting of muscles as well as skin, and there is marked rarefaction of bones in relation to joints, without osteophytic changes.

(4) In the well-established condition the joint lesions present the following characteristics: (*a*) Symmetry of distribution; (*b*) tendency to affection of the smaller joints initially; (*c*) fusiform enlargement; (*d*) associated glandular enlargement out of proportion to the swelling, heat and tenderness.

(5) A general metabolic upset comprising: (*a*) Nervous apprehension; (*b*) sweating; (*c*) tachycardia, (*d*) pigmentation; (*e*) trophic changes.

(6) Secondary anæmia [2], usually with an absence of polymorphonuclear leucocytosis but a relative or absolute lymphocytosis.

(7) A lowered resistance of mucous membranes to infection.

(8) Various biochemical anomalies, among which are (*a*) either an absence or deficiency of free HCl in the stomach content [2] [3] [4]; (*b*) decreased sugar tolerance [5]; (*c*) lowered basal metabolic rate.

It would appear, therefore, that the physical or perhaps more properly speaking the biochemical make-up deviates considerably from the normal; and further research along this line is likely to prove more fruitful than that conducted purely from the bacteriological point of view. At present, with the above data, it is a little difficult to dismiss lightly the thought that up to the present time the part played by disordered metabolism in the production of atrophic arthritis has not received the attention it warrants.

Multiple Non-specific Arthritis.

This simulates atrophic arthritis, but the following distinguishing features should be noted: (1) It is not always of obscure origin, though frequently insidious in onset. The sex distribution is much more even and the disease at the onset is not confined to young adults.

(2) In many instances there are obvious infective foci, removal of which greatly improves the prognosis [6].

(3) Atrophy is not *the* conspicuous feature, though local wasting of muscles is present. The X-rays do not show the same degree of rarefaction of bone, at any rate in the earlier stages, and bony changes are often apparent.

(4) The joint characteristics lack the symmetry of distribution. The local wasting is not usually so marked. The associated glandular enlargement, if present, is in direct proportion to the heat, redness, and swelling of the particular joint involved, which may be villous in type.

(5) The signs of a general metabolic upset are absent or but slight in comparison with those obtaining in atrophic arthritis.

(6) While secondary anæmia is common and lymphocytosis is frequent, polymorphonuclear leucocytosis is usually manifested in some stage of the disease.

(7) Mucous membranes generally are not so prone to subinfection.

(8) The biochemical anomalies are less constant; for example, there is not the same constancy of achlorhydria, and lowered sugar tolerance can be accounted for by the presence of septic foci, removal of which assists a return to the normal.

On general consideration, therefore, it cannot be said that the clinical and biochemical picture of multiple non-specific arthritis comprises the definite entity defined as atrophic arthritis, nor would one expect this to be so having regard to the fact that many types constitute this heterogeneous mass of joint diseases.

Menopausal Arthritis.

This is the entity styled hypoglandular arthritis by the French—a name not used in this country, unless arthritides associated with endocrine mal-function, other

than that to be described, be included under this heading. This clinical group of cases usually occurs in women from 45 to 55 years of age, but it may also affect men at a somewhat later age-period.

It is not suggested that ordinary multiple non-specific arthritis does not occur at the climacteric as at other times, but in the class under discussion it is difficult to state exactly the importance, if any, of infection, in the production of the initial state. Trauma and static factors influenced by the increase of weight common at this period certainly play an important part, as do also those vascular degenerations so frequently found in these subjects.

Patients suffering from menopausal arthritis present themselves showing signs of either coarse synovial crepitation, usually in the knees, often almost, if not absolutely, indistinguishable from osteo-arthritic grating, or of villous arthritis. From careful clinical and X-ray observation it seems to be almost without doubt that the condition of dry synovitis may proceed directly to osseous changes or that villous fringes may develop, eventually ending in osteo-arthritis. In some cases both types co-exist with the eventual onset of osteo-arthritis. It is these patients who benefit in such marked degree under thyroid treatment.

Villous Arthritis.

To some extent this ground has been covered by preceding remarks on multiple non-specific arthritis and menopausal arthritis. That is to say, that according to the view here put forward, villous arthritis is simply an expression of trauma, sepsis or endocrine disorder. In other words, the particular reaction to abnormal stimuli in certain cases of synovial involvement is of a villous nature. It follows that osteo-arthritis subsequent to infection or villous arthritis should be tabulated under these disorders, until it is proved or disproved that all osteo-arthritides of middle or late life pass through the stages commonly occurring in menopausal arthritis.

It would appear possible to correlate the pathological findings of Strangeways [7] with the above clinical grouping already in use before the publication of his contribution; if a certain elasticity be allowed, and it be remembered that he describes absolute findings any one of which may be but a stage in the course of different morbid processes.

(1) *The Atrophic Type.*—In the earlier stages of the disease inflammatory changes are stated to have occurred in the capsule. Accumulation of fat was present in the muscles and other tissues of the limbs. There was atrophy of the synovial membrane and articular cartilage. The bones were greatly rarefied, the cancellous tissue being largely replaced by pure fat so that the bone cut readily with a knife. This was specially marked in the hands, in which the carpi might be found fused together. This type was confined to the female sex, and the patients are described as being usually well-nourished and cheerful.

This is the pathological picture of what one would expect to find in the atrophic type as estimated by clinical and X-ray observations. The only point of difference is the fact that the patients were usually well-nourished. Since, however, the disease was of old standing and inactive, and death was presumably due to some intercurrent cause, the fact of the maintenance of general nutrition is not remarkable.

(2) *The Adhesive Type*—"This type began with inflammatory changes in the synovial membrane and capsule. These changes were associated with the formation of a vascular granulation tissue which spread over the cartilage and became adherent to it, thus causing vascularization of the perichondrium and of the deeper layers of the cartilage. This was followed by erosion and replacement of the cartilage by a vascular inflammatory connective tissue which later invaded parts of the calcified cartilage and the bone underlying it. As this inflammatory process spread, vascular adhesions were formed in the joints between adjacent articular surfaces.

. . . . . The inflamed capsule and synovial membrane, together with the newly formed connective tissue and adhesions, gradually became less vascular, and eventually were

replaced by firm fibrous scar-tissue in which development of new bone might occur, leading to bony ankylosis. This type appears to be confined to the female sex. The patients were thin and wasted and several joints were usually attacked."

The above description denotes a frankly infective process, such as would be likely to occur in an advanced stage of multiple infective arthritis. An X-ray plate of such a case in the earlier stages frequently demonstrates the presence of erosion. No explanation can be offered as to the circumstance of these pathological findings being almost entirely confined to women.

(3) *The Dry Type.*—The most characteristic change was the partial or complete absorption of synovial fluid. The atrophied fibrous capsule firmly gripped the bones of the articulation, so that the articular surfaces lay in close apposition. The synovial membrane was dry and parchment-like. The cartilage showed pressure-atrophy, but the bones were normal. True adhesions between the joint surfaces did not occur. This type prevailed more frequently among females. This is the pathological condition which presumably obtains in the early stage of menopausal arthritis, but clinical observation would show that bony changes may occur later.

(4) *The Capsular Type.*—The pathological changes were practically confined to the capsule, which showed some degree of thickening and fibrosis. The synovial membrane was not much altered and the articular cartilage as a rule appeared normal, but in the later stages of the disease it might exhibit atrophy over the area of pressure. The bones were normal. This type occurred in both sexes, but was more common among females. The joints were stiff, swollen and painful. A leather-like creaking was often present. The patient, who complained of loss of strength and feeling of slackness, sometimes made a complete recovery, but relapses at shorter or longer intervals usually led to gradual, permanent crippling. It is possible that this may be a stage of menopausal arthritis consequent upon persistent movement following the dry stage of menopausal arthritis. At the same time this description might well refer to periarticular fibrositis. The generalized swelling, accompanied by laxity of ligaments and weakness, may occur after rheumatic fever should there be a subsequent fibrositic involvement and occasional synovitis.

(5) *The Villous Type.*—The synovial membrane was in varying degree covered by velvety, pedunculated villi, usually pink in colour, but deeply congested where the pedicle was long. There was but little increase in the synovial fluid. Melon-seed bodies or "joint mice" were sometimes found lying free in the joint cavity. The cartilage was not much altered and the bones were normal. This cannot be other than the villous type of Max Schüller [8].

(6) *The Infective Type.*—Under the heading of infective arthritis a series of specific arthritides are implied, but no morbid findings are recorded. Neither the pathological nor the clinical data concern us here.

Summary.

(1) The necessity for proper investigation of clinical groups of arthritis has been stressed. (2) Certain affections of loose denomination have been allocated to definite pigeon-holes in a mobile classification of arthritis, in an attempt to clear up the contents of an abused rubbish-heap. (3) Correlation of clinical and pathological findings has been essayed in particular types comprising this dump.

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Discussion.—Dr. C. W. Buckley (Buxton) said that Dr. Coates had made a most useful contribution to a difficult problem. He felt that the only way of arriving at a satisfactory classification was by the definition of well-marked types, first, such as those Dr. Coates had

described. In his opinion, exactly similar forms of arthritis from the clinical aspect might result from different infective agents; gonococcal arthritis could frequently not be distinguished from forms due to streptococcal infection. The variations were due rather to differences in soil or constitution than to the infective agent and to the simultaneous action of trauma, often in the simple form of wear and tear which led to the joints which were most used being first or mainly affected. The type of polyarthritis which was regarded by some French authorities as tuberculous was, in his opinion, due to the same organism as that giving rise to the more common forms, viz, a streptococcus; but the subjects belonged to that constitutional type formerly termed the strumous or scrofulous diathesis. They had a low-resisting power to microbic infection and might escape tubercle to fall victims to this streptococcus, the form of arthritis resulting being characteristic by reason of the underlying constitutional characters. He would like to hear whether others had noted the comparative immunity of the Jewish race to the relatively acute, symmetrical forms of polyarthritis as met with in young subjects—an immunity to which attention was drawn by his colleagues at the Buxton Hospital where about 1,000 cases of arthritis were seen annually. This immunity did not apply to osteo-arthritis or to the arthritis of the menopause, and it might be due to some endocrine peculiarity.

Dr. H. A. ELLIS said that the classified list presented by Dr. Coates appeared to be constructed upon the correct lines and agreed with the facts of the metabolic changes as revealed by the urinary examination of affected persons.

Rheumatic cases examined in this way formed three defined groups, which were definitely characteristic of certain conditions. The first group was characterized by the signs of metabolic deficiency and gave evidence of errors of assimilation, produced either by inability to obtain, or inability to use the necessary sustenance. The next definite class comprised those cases in which urinary examination showed the signs associated with conditions of metabolic excess accompanying errors of elimination, which suggested that the organism was suffering metabolic retention on account of over-production or more commonly of a failure in the capacity to eliminate. The third class formed the large and indeterminate group existing between these two definite classes which up to the present had been impossible of accurate differentiation and in which the urinary evidence presented either a mixture of the other two classes, or one that at present could not be interpreted. It would be a work for the future to differentiate this class into its component varieties. The first classes, or those with assimilation deficiency, included the classes that Dr. Coates placed at the beginning of his list. The patients in those classes manifestly had, if not a complete absence of phosphoric acid in their urine, at least a great deficiency; and this deficiency of free phosphoric acid became the more apparent when its ratio was considered in relation to the amount of acid present combined with ammonia, as revealed by Sörensen's test. If the urine secreted during the night, and passed first thing in the morning, by an ordinary healthy person, was titrated with decinormal soda and the result recorded at neutralization as the amount of free phosphoric acid present, and to this neutralized urine was added neutralized formalin, a further amount of acid combined with ammonia in natural conditions was released; this was then estimated by titrating to neutrality with decinormal soda as before. Then when the amounts of these two acids were compared they would be found to have a definite ratio to one another. Now it was the variations of this ratio between the free phosphoric and the combined ammonia acid that revealed the indications that were being sought. These ratios would be found to divide roughly into three classes: (1) The amount of combined acid with ammonia would be three or four times the amount of free phosphoric acid present. In this class would naturally be placed those cases in which there was a complete absence of phosphoric acid. (2) The amount of acid combined with ammonia would be about twice the amount of free phosphoric acid present. (3) The amount of acid combined with ammonia would be approaching the amount of free acid present. Very rarely, if ever, was the amount of free phosphoric greater than the amount of ammonia-combined acid. In the first group would be found all those cases which could be classified under the head of assimilation errors and were affected with metabolic deficiency. These cases presented the characteristic of a deficiency or a complete absence of free phosphoric acid in all urines passed. This occurred even in old age when the tendency was for the two amounts normally to approximate. On account of this deficiency of free phosphoric acid these had been classified as the alkaline group. This group was very definite and could always be recognized by this deficiency. The other definite class (3) was not so large a one, but equally distinct, and contained those cases in which patients were suffering from metabolic excess and

had elimination difficulties. In these cases the free phosphoric acid approached the amount of ammonia-combined acid, and comprised those cases of rheumatics with cardio-renal disturbance accompanied by joint disease. This class, on account of the proportional excess of free phosphoric acid present, had been termed the acid class in contradistinction to the other, the alkaline class. This accorded with Dr. Coates' classification, as he placed the two kinds at opposite ends. In between these two definite groups came the large indeterminate class, as yet undifferentiated, often having characteristics of each class and on treatment found to have mixed conditions, as remedies that benefited one set of symptoms and gave certain relief made the other symptoms worse; hence their difficulty of treatment.

Dr. Coates' limitation of the term metabolic to one class appeared to him (Dr. Ellis) to be erroneous. All forms of rheumatism were due to metabolic errors. Sometimes the factor was deficiency of material, sometimes excess accumulation, sometimes septic toxins, sometimes glandular deficiency or excess, and at other times there were unknown factors in operation, but the main factor in constituting the disease was the metabolic error. He (Dr. Ellis) said that if Dr. Coates would analyse the urines of his earlier cases, as suggested, he would find that they all yielded alkaline characteristics. The urine must be that first passed on waking, and there should be a second specimen analysed of that passed some two hours after, to obtain the necessary contrast. Curiously enough those passing uric acid in the urine did not display characteristics of either group. The importance of these analyses consisted in the fact that where they gave a definite indication they controlled the treatment. It was manifestly absurd to give alkalies in alkaline cases or acid in acid ones. In rheumatic cases there was, in addition to errors of reaction, in his experience, another error of metabolism, namely, an excess excretion of magnesium. He had found it present in all types of active rheumatism. Whether it was due to a definite excess, or to balancing up some other salt, or to an incapacity of absorption, was not yet apparent. The interesting point was that it was always present in active rheumatism, and was not often present in other conditions.

Dr. G. L. KERR PRINGLE said that Dr. Vincent Coates was fortunate in the title of his paper, i.e., "So-called Infective Arthritis." He (Dr. Kerr Pringle) was convinced that infection was not the sole cause of these conditions and considered that the soil played an important part. He thought that tubercle had a definite relation or connexion with some of these cases and he referred to a case which he had had the opportunity of watching for a long period of years. The patient when aged three years suffered from a tuberculous knee. At from twenty-six to twenty-eight years of age she developed an enlarged gland in the supraclavicular region, which disappeared after administration of tonics and after taking a holiday in Switzerland. The gland became enlarged again some months later when tonics were again employed, then it finally disappeared. A few years later she developed a peri-articular swelling of the right wrist which subsided after change of air, malt in the diet, and the application of iodine. From time to time during the last ten years, there had been swelling and pain in joints, which had disappeared under the same form of treatment. No evidence of any focus of infection had been found at any time.

There were many cases of arthritis of the so-called infective type which he (Dr. Kerr Pringle) had followed up from year to year and he had found on looking over the notes of previous years that he was very apt to classify these cases under a different heading. He thought that instead of referring to types, we ought in the meantime rather to speak of stages in the disease.

Dr. GEOFFREY HOLMES commented upon the clearness of Dr. Coates' description of the type of case he named atrophic arthritis. He (Dr. Holmes) reminded the Members of the Sub-committee, formed to discuss nomenclature some two years ago, and that he had urged the use of the phrase "associated with infection" rather than the more "committal" term "infective arthritis." The preceding speakers had dealt with ætiology and to-day's discussion had been notable in that the infective theory had not been the only one considered. He thought this marked progress, for, although there was obvious sepsis associated with many cases of arthritis, in others no infective focus could be found and he was not satisfied that infection was the sole ætiological factor. At the Harrogate Royal Bath Hospital he (Dr. Holmes) had failed to find a lowered basal metabolic rate in arthritis, and this negative finding did not support the hypoglandular theory. As some of the urinary conditions described by Dr. Ellis possibly originated in the kidneys, he (Dr. Holmes) hoped he would extend his investigations to include the chemistry of the blood.

Section of Balneology and Climatology.

President—Dr. WILFRID EDGECOMBE.

The Climate of Brighton.[1]

By DUNCAN FORBES, M.B.E., M.D.

(*Medical Officer of Health, Brighton*)

BRIGHTON has been recognized as a health resort for some 200 years and owes part of its success to the good judgment of the Royal Family from George IV onwards. Another factor favouring its continued popularity is no doubt its easy approach from London, many trains each day doing the journey in one hour's time. But all these are only aids to success, and what we have to consider now is the climatic attractions of Brighton as a health resort and holiday resort, on which its present and future success is solidly based.

SUNSHINE.

One of the most important points is that Brighton shares with other health resorts on the south coast a high daily sunshine record. Each day in our newspapers we see the hours of sunshine at health resorts up to 6 p.m., but these records convey little compared with the records taken over a long series of years as shown in "The Book of Normals," published by the Meteorological Committee for a thirty-five year period (Printers: His Majesty's Stationery Office, 1919). From these it is at once apparent that the duration of bright sunshine, that is, a record burned by the sun's rays, is greatest on the south coast, where the daily average is about 4·8 to 5·0 hours as compared with 4·3 and 4·4 at Southport and Blackpool; 4·1 and 4·3 at Oxford and Cambridge and 3·9 at Harrogate.

Although Brighton is a large town and therefore has a larger number of domestic grates than its smaller neighbours, it maintains its place amongst towns with the highest records. In summer it suffers relatively from its position, its coast line running from south-east to north-west, and to the east high Downs ranging from 200 to 500 ft. are situated. In warm summer weather in the early mornings there is frequently a haze over these cold Downs, the result being that whilst this mist is slowly being rendered gaseous our record may begin as much as fifteen to twenty minutes after the records of neighbouring coast towns; but this happens between 5 a.m. and 6 a.m. when most of us are still in bed.

IMPORTANCE OF WINTER SUNSHINE.

It is absurd, however, to stress the loss of a few minutes' sunshine in the summer, when there is enough sunshine and to spare. It is very different in winter when we want all the sunshine we can get. In Brighton, in the winter months, the sun is well to the south and the disadvantage of high ground to the east disappears.

It is an interesting fact that we have no crippling bony deformities due to rickets in Brighton, whilst for instance in the rural districts of Salop rickets contributes 6 per cent. to the cripples admitted to the orthopædic hospital. As you are aware the onset of rickets commonly occurs in the sunless winter months and it is doubtless due partly to our relatively long hours of sunshine in winter that the Brighton slum children escape rickets. When we compare the daily average of winter sunshine in large industrial centres, for instance, Whitworth Park, Manchester, and Brighton, the contrast is marked.

[1] Read at the Annual Provincial Meeting, held at Brighton, May 23, 1925.

	Brighton	Manchester
October	3·74 hours	2·06 hours
November	2·50 ,,	0·67 ,,
December	1·61 ,,	0·42 ,,
January	1·87 ,,	0·58 ,,
February	2·87 ,,	1·42 ,,
March	4·26 ,,	2·35 ,,

It is no wonder, then, that if sunshine be a potent factor rickets is plentiful in industrial centres.

Rainfall.

The average rainfall of Brighton is 27·7 in., which is higher than that of sea-coast towns on the south-east coast, for instance Margate with 23 in., but is much lower than that of towns on the south-west, Torquay and Ilfracombe, for example, where the rainfalls mount to around 40 in. The low rainfall is due to the moist south-west winds passing over the town before they reach the colder Down high lands which condense their moisture. Eastbourne, for example, with high land to the west has 3 in. more rainfall annually than Brighton. In addition, practically all winds except the south-west pass over long stretches of hill and land before coming to Brighton.

It is surprising how quickly Brighton dries after rain; this is partly due to the steep gradients but also to the porous nature of its soil and subsoil, which are composed of fissured chalk with flints. In the valleys Coombe Rock is found, which is also porous. There is a deposit of surface clay from the West Pier to the Hove boundary between the sea front and the Western Road, but this deposit occupies only a small area in Brighton. Except in narrow strips at the bottom of the valleys no ground water is anywhere near the surface, so that in general even basements are quite dry.

In addition to the low rainfall, the steep gradients, and the great depth of the ground water, the Downs surrounding Brighton are largely treeless, so that, apart from dew ponds, there is little or no surface water. If, then, any advantage, for instance to rheumatic patients, is to be gained from a low rainfall and a dry subsoil, Brighton can claim that advantage.

Water Supply.

Our water supply is obtained from five wells in the Downs, and has a total hardness of some 15 degrees, only 3½ degrees of which is permanent hardness, so that prolonged boiling removes most of it. This degree of hardness is the same as that of much of London's supply and rather less than some of it—for instance, the New River, the East London and the Southwark and Vauxhall water supplies contain 16·1, 17·6 and 15·3 degrees respectively.

I get a great many inquiries with regard to the hardness of the water supply, many of the laity and a few doctors thinking it bad for gouty and rheumatic people. Some have argued with me that it must be so as there are "chalk deposits in gout," and have felt unconvinced even when I assured them that those so-called chalky deposits consisted not of calcium but of sodium urate. Others, for no very obvious reason, associate rheumatism with the drinking of hard waters. When one considers that you would have to drink a gallon of cold water before consuming fifteen grains of calcium, and, again, that most of us take water in the form of tea and coffee, which are made with boiling water that has lost at least a part of its hardness, I think it must be evident that the fear of a moderate degree of temporary hardness is groundless.

The Brighton water is highly palatable compared with soft waters because of its content of carbonic acid gas. Probably on account of its palatability and its power to hold gases, three well-known mineral water firms have large works here.

A common mistake is made as to the cause of the occasional milkiness of water drawn from the tap. Some people jump to the conclusion that this haziness is due to chalk in suspension; it is of course really caused by fine air-bubbles which upon

a little careful inspection could be seen to rise to the surface. The presence of these fine air-bubbles is partly due to the direct pumping of the water into the water mains, air being churned up with the water at the pumps. The chief cause, however, is that the cold well-water at about 52° F. can dissolve far more air than warmer water, so that when the thoroughly-aërated cold water from the wells mixes with warmer water from the reservoirs at about 56° F., or gets heated in the service pipes, as so frequently happens where hot and cold water-pipes are adjacent to each other, then a part of the dissolved gases is freed as minute air-bubbles, which produce a haze.

Temperature.

The temperature, as you are aware, is more equable at the seaside, the daily range being two degrees less in comparison with that at inland stations. But perhaps the greatest difference produced by the sea is a seasonal one. The temperature of the sea in October is still 55·7° F. as compared with 42·7° F. in March, which leads to a much higher average temperature for seaside places from September to December. Incidentally, I may mention that there is no time of the year in which the comparative advantages of Brighton are so marked as in the late autumn.

One record which appears in weather reports is that of cooling power, and this should always be considered along with the temperature. This record, which appears in *The Times*, is taken at the National Institute, the readings being recorded every hour for five minutes, the average highest and lowest cooling powers constituting the record. Professor Leonard Hill tells me that "the cooling powers are those of the air on the bulb of the dry Kata thermometer at approximately skin temperature in millicalories ($\frac{1}{1000}$ gramme calories) per square centimetre of surface per second." His standard for a fresh room is a cooling power of six. People in hot rooms, mines, submarines, often work in air where the cooling power is half this figure or less. The open-air workers are exposed to high cooling powers, as shown by *The Times'* readings, 27—12½ degrees, as measured above on May 21. I mention this subject of cooling power of air, as good ventilation is really not altogether a question of cubic content and air supplied: it is also a question of air movement. Surface moisture evaporates only slowly in still air, whilst live, moving air full of eddies leads to rapid evaporation, hence the benefit of fans, even when their revolutions simply agitate the air. What applies to a room applies to towns, and Brighton is one of the best ventilated towns. It faces south-west along two miles and more of sea-front, and the prevalent warm south-west winds blow freely along the whole front, the central valley and through the many streets at right-angles to the front. On the other hand, it is sheltered from the north-east winds of February to May inclusive by the Downs to the north and east. Particularly is the front sheltered by high buildings, and visitors are often surprised at enjoying warm, sunny weather there in spring, whilst east-coast resorts and others less favourably exposed are suffering from bitingly cold east winds. The same conditions, however, naturally do not hold good for the high parts of the town, which are districts familiar to the resident rather than to the visitor.

It is for this reason, namely on account of its free movement, that the air of Brighton both feels and actually is bracing, even in the warmest periods of the year, although Brighton itself has a more equable temperature than inland resorts and is warmer than many sea-coast holiday centres.

Death-rates and Infantile Mortality.

No conclusions can be drawn from the death-rates in Brighton, as we have so many invalids coming here to live—for instance over 20 per cent. of our cases of pulmonary tuberculosis are imported. But a sensitive index is our infantile mortality, which is around 60, compared with 75 for England and Wales, and 80 for the 105 great towns. I have prepared a table showing the weekly deaths from various diseases and their relation to the average temperature of the months of the year. You will note how markedly the deaths from respiratory diseases increase

	Normal average temperature 1881-1915	Death-rates 1902-1924 (inclusive)				
	Degrees Fahrenheit	Influenza	Bronchitis and other respiratory diseases	Phthisis	Other tuberculosis	Diarrhœa
January	40·0	1·97	9·73	3·39	0·98	0·23
February	40·8	2·86	11·78	3·09	0·92	0·28
March	42·8	2·29	9·13	3·47	0·94	0·32
April	47·6	1·19	6·22	2·89	1·25	0·15
May	53·7	0·45	4·16	2·80	0·81	0·23
June	58·9	0·25	2·75	2·78	1·03	0.31
July	62·2	0·18	2·14	2·48	1·03	0·40
August	62·2	0·78	2·17	2·46	0·85	2·28
September	58·9	0·16	2·00	2·52	1·03	3·48
October	52·2	0·35	3·68	2·90	0·93	1·64
November	46·1	1·87	5·65	2·88	0·98	0·50
December	42·0	0·92	6·19	2·80	0·95	0·31

in January, February and March. Influenza follows a similar course and so does phthisis to a lesser degree. Other tuberculous manifestations are not affected by temperature. Diarrhœa is late in reaching its maximum in Brighton, the heaviest death-rate occurring in September.

After a study of all these components of the Brighton climate is it possible to say what classes of patients can be confidently recommended to reside or recruit in Brighton?

In the first place I would recommend it all the year round for the patient suffering from brain fatigue, and for the patient temporarily run down. In Brighton he enjoys a warm, sunny, dry climate which is at the same time eminently bracing. Even on rainy days he can take long walks under cover, on our piers and in the covered walk on the Madeira Drive. Combined with this he has the advantages of all the amusements of a large town—first-class theatres, music halls, cinemas. If such a man is sent to a small town he is apt to suffer from ennui. At Brighton this is impossible, as he can play tennis, has a choice of five first-class golf courses, he can bowl, he can fish, he can boat, he can bathe and if he be lazy he can amuse himself by seeing others at play. He can enjoy wonderful, lonely country walks over the Downs or he can see the country-side in his car or from a crowded char-a-banc.

For all forms of tuberculosis the climate is excellent. Similarly, the patient suffering from bronchitis benefits. In each I make an exception of the advanced case, especially from January to April when east winds are frequent. For such patients the winds are too strong and the gradients too steep, and they would do better in warmer, more relaxing climates.

Just as the early cases of pulmonary tuberculosis and bronchitis benefit so does the patient recruiting after influenza or other acute illness. Weakly children, the rickety child, the child with tuberculous neck glands, and healthy children derive the greatest possible good from a stay in Brighton.

Another point to be remembered is that there is relatively little rheumatism in Brighton, probably because of the dry, sunny climate and dry soil.

In Brighton we have large hospitals serving a considerable part of Sussex and we have great numbers of people coming to the town to recruit their health. To meet the needs of these we have medical practitioners who have more opportunity than doctors in other towns of seeing and treating numbers of such persons. Consequently Brighton medical men reach a high degree of skill in the practice of their profession.

Although armed with meteorological statistics dating back to 1889 I have avoided in this paper giving many figures, as I feared that these might make my paper even drier than I am afraid it has actually been. Still, I have done my best to make our dry climate interesting in the brief account I have put before you.

SECTION FOR THE STUDY OF DISEASE IN CHILDREN.

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Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

Posture in the Treatment of Disease.

PRESIDENT'S ADDRESS.

By R. H. Anglin Whitelocke, F.R.C.S.

I propose this evening to direct your attention for a short time to the subject of posture, more especially as it may concern our dealings with sick children and their management. I shall begin by referring to the practical relationships of posture to ætiology and diagnosis, and later to treatment of various forms of disability in the adult as well as the child.

It not uncommonly happens that the individual, be he child or adult, assumes instinctively a posture by which his condition may be immediately recognized or which gives important indications as to his future management. The clinician also from time to time is accustomed to make systematic use of posture to assist him in diagnosis and to aid him in treatment.

Posture as a cause of disease is chiefly important in connexion with occupation, thus whilst many persons suffer, and in various ways, from long-continued standing, there are others who are placed equally at a disadvantage from following sedentary occupations. Some, who like colliers and miners, have to work in constrained and unnatural positions are also liable to suffer in consequence.

Posture is also of importance as predisposing to certain affections under peculiar and unusual circumstances and in modifying these effects. The recumbent position in low febrile states may lead to hypostatic congestion with its consequences, or to an accumulation of morbid products in the bronchi and lungs which may seriously endanger life.

Positions adopted spontaneously by patients often give useful information in diagnosis. In cardiac and pulmonary diseases the individual may be quite unable to lie down owing to the interference with the respiratory functions, so that he is perforce compelled to sit up, lean forward, or be propped up in bed. In one-sided lung affections, he may be only able to lie on one or other side, whilst in heart disease he may be quite unable even to lie on his left side. The position generally assumed by the subjects of acute generalized peritonitis is so familiar to us all, and so characteristic, that it needs only to be mentioned to supply us with a very striking example of the value of morbid decubitus as a diagnostic asset. In the various spasmodic attacks of pain frequently associated with this part of the body and known as colic, it is very common to see the patient bending forwards in a doubled-up position and pressing upon the abdomen.

In nervous affections also posture aids diagnosis. It may reveal paralysis in various parts. In cerebral meningitis the subject lies usually in a state of complete flexion with the limbs and body curled forwards and brought together closely, and in cataleptic states any position assumed is usually retained for some considerable though variable time.

The posture voluntarily assumed by a limb may give useful information as to local diseases likely to influence it, such as those of the joints, or draw attention to some injury.

Some of the facts already enumerated clearly demonstrate to us the value of posture as a therapeutic measure also, and it is to this aspect of the subject that I shall now turn. It will at once be evident to us that if a wrong posture is the cause of any disability or morbid condition, our first principle in treatment should be to set it right. It is often of great service to allow or even encourage a patient to adopt the position which makes him most comfortable at the particular time. Whenever general prostration and exhaustion are present the recumbent or semi-recumbent posture is clearly indicated as being the most restful, seeing that it involves a minimum of effort. In acute febrile conditions the first essential of treatment is to keep the subject recumbent in bed; and this is also desirable and advisable when excessive fatigue has been endured.

In respiratory affections attention to posture is often of much service, for symptoms associated with the breathing apparatus, such as pain, cough, and dyspnœa are in many cases peculiarly influenced by change of position. Coughing is often arrested and expectoration made easier by the assumption of a sitting or erect position, whilst the semi-prone or bending-forward position may help much in relieving any pressure upon the bronchial tubes.

All cardiac affections are more or less influenced by posture. The horizontal is invaluable in syncopal states since keeping the head at a lower level than the body allows the blood to reach the brain more readily. Whenever the heart is acting feebly from any cause, the act of suddenly raising the patient even into the sitting posture is injudicious and may even cause death.

On the other hand there are conditions of the heart, as when it is dilated, when the individual is quite unable to lie down with any comfort. In such a case it may be quite necessary to have him propped up constantly; the beneficial result being sometimes almost miraculous.

The influence of gravitation may be recognized in certain conditions such as dropsical accumulations and generalized anasarca. Even hæmorrhage may be rapidly diminished or even partially arrested, on occasions, by elevation of the bleeding limb or injured part.

In the treatment of such affections as sea-sickness, giddiness, neuralgia, headache, affections of the head and spine, change of position often brings relief.

The varied positions into which the limbs are placed involuntarily to relieve muscular spasm, tension or pain as the result of disease or injury, must not be lost sight of.

In the case of the feeble and those who have been long confined to bed, it is essential that their positions should be changed periodically to diminish the risks of hypostatic congestion of the lungs, and the development of bed-sores. The subjects of chronic cerebral and spinal diseases are also peculiarly liable to these postural disabilities.

Having endeavoured to point out the many and varied uses of posture as a prophylactic and therapeutic factor in general, I will now refer to some of

the forms as they are made use of both for prevention of disabilities and complications and in the after-care of surgical operations.

The operating surgeon of to-day must needs direct his best energies to his patient's requirements, not only before and at the time of his operation, but also from the time of operation until convalescence is established or complete.

Posture is a very important factor entering into post-operative care and treatment. Viewed from all points, it is certainly one of the most powerful means available both of avoiding complications and hastening recovery. The positions used at the present day are many and varied, and are adapted for preventive and active treatment. Among them are the following:—

(1) The Trendelenburg or position of raised extremities.

(2) The Sims, semi-prone or abdominal.

(3) The Fowler, sitting, or semi-erect.

These may be used separately, or in combination, to effect almost every conceivable requiremeut.

The Trendelenburg position has been extensively used by surgeons in the United States of America, mainly as a prophylactic agent in assisting to maintain the return of venous blood when for any reason it is hampered, either from a local or general cause.

The Sims and Fowler positions are chiefly concerned in establishing and maintaining the drainage of morbid secretions and effusions from wounds, or from the cavities and spaces within the body.

Venous thrombosis and its consequences have for many years past supplied a serious cause of anxiety and discomfiture to surgeons. Why the slight anatomical difference between the iliac veins should cause the left vein to be affected in this way at least twenty times more frequently than its fellow, and why the complication should occur usually in the second week after the operation and not before, and in post-operative cases rather than in those without operation, is difficult to understand, and is still to-day without satisfactory explanation.

W. Meyer, who twenty-three years ago published a very interesting paper[1] concerning this serious complication, wrote again recently[2] that in his opinion "treatment by posture will prevent venous thrombosis to a certainty" if carried out regularly and systematically as a post-operative method in the Trendelenburg position. It is gratifying to feel that so serious a complication may be overcome by such simple means as raising the lower end of the bed upon blocks so that the pelvis of the subject is higher than his thorax. Elevation of the thrombosed limb alone is not recommended. The whole true pelvis must be raised higher than the thorax and the posture adopted soon after the operation and maintained until all risks of thrombosis have passed, that is for at least ten days. During this time breathing exercises are encouraged as part of the treatment.

The typical Sims posture with both arms of the patient pointing forwards while he rests principally on his right shoulder is to my mind the most useful, efficient, and comfortable. If we put the subject of a perforated appendix in Sims' position immediately he is removed from the operating table to his bed, it will be seen that the true pelvis is now the highest point of the drainage system, and not the lowest.

Blood serum, wound secretions, bile, or any other intra-abdominal

[1] "Rare Complications after Operations for Appendicitis," *Annals of Surgery*, May, 1901.

[2] *Surg. Clinics of North America*, 1921, i, p. 305.

extravasated material will in this position run downwards by the shortest possible route, and find ready exit, providing an adequate drainage tube is used.

The continuance of this position for the first eighteen or twenty-four hours should be insisted upon—after this, a slight turning to the left is permitted. The usual dorsal decubitus is assumed on the third day, as by that time the discharge will have ceased or become greatly diminished.

The foot of the bed should not be raised for the first two or three days, lest by chance any toxic material should flow towards the subphrenic spaces where absorption is known to be most active.

Fowler's position, at one time so much extolled and almost universally used, is not now so much in favour. Instituted for the main purpose of establishing drainage and the removal of all toxic fluids towards the pelvis from the upper abdominal peritoneum by means of gravitation, it has been found in practice to present the following disability, which is not present in Sims, or the modified Sims position. The Fowler position is often very uncomfortable; the patient keeps sliding downwards into the bed and requires to be raised periodically, a manœuvre not without danger. Children will seldom remain in this position for any length of time, and I claim that it is by no means an established fact that it is essential to successful drainage. I consider that the gravitation theory is untenable, and for the following reasons:—

(1) Drainage of the pouch of Douglas is quite successfully established through a mid-line anterior abdominal incision with the drainage tube placed vertically and the flow against gravity.

(2) The thorough drainage of abscesses and other morbid effusions that have escaped into any part of the peritoneal cavity, be it pelvic, appendicular, hepatic, gastric or intestinal, can be effected with the patient lying on his back even continuously, provided certain precautions are taken as to the nature and size of the tube.

(3) Children seldom remain in the semi-erect Fowler posture for any length of time, yet who will say that the mortality of appendicitis is any higher in children than in adults?

(4) Some surgeons who have never allowed the Fowler position at any time in their practice have yet obtained excellent results.

That gravitation plays a very important part in abdominal, thoracic, or intracranial drainage, is doubtful. The drainage of morbid substances from the body cavities outwards or from one part to another is mainly a vital process. The to-and-fro respiratory movements of the diaphragm and chest wall are undoubtedly the main agents; and drainage is effected by muscular movements rather than by gravitation.

The peritoneal cavity is normally a closed space with all the contained viscera in close apposition. Every movement of the diaphragm and abdominal muscles in the respiratory efforts presses the viscera—which are elastic and largely compressible—closely together in a to-and-fro rhythmic way. Any fluid or semi-fluid material that is present in, or has found its way into, either the abdominal or thoracic cavities is also similarly compressed. It now only needs a suitable opening, prevented from closing by the insertion of a proper drainage tube, for automatic drainage to be established.

In a similar manner, in the case of joints, the encouragement of movements materially assists drainage.

In my opinion the noticeable reduction in the mortality of appendicitis

cases following the introduction of the Fowler and Murphy positions was probably not due to this postural form of treatment so much as to the contemporaneous use of saline injections.

I wish to emphasize that in the nursing of children, posture is especially important, and much, I think, may be gained for them in the future, if the nursing profession is taught as a routine the methods of its application. The child instinctively and naturally assumes that position in which he is most comfortable, yet how often is he roused and put into some set position regardless of his protestations, only to lapse again as soon as possible into his old place!

Let me say in conclusion, that though some of the opinions I have expressed may seem to be heterodox they are mainly the outcome of personal experience.

Case of Myelogenous Leukæmia in a Child of 13.

By STANLEY WYARD, M.D.

PATIENT first came under observation in March, 1920, at the age of 10 years, when she had signs of chronic bronchitis. As a result of further observation and X-ray examination a diagnosis of tuberculous mediastinal glands was made.

She was admitted under the care of Sir Humphry Rolleston, the diagnosis was confirmed and she was sent to a Convalescent Home at Broadstairs, for a month. Two months later the bronchitis became bad again, she lost weight and the tonsils were enlarged. She was again sent to Broadstairs for six weeks, her tonsils having first been enucleated. She returned much improved and continued to put on weight, so that treatment ceased in April, 1921.

In January, 1922, cough returned with some signs of bronchitis, but by March she was perfectly well again and was told to report in twelve months. At the end of this time (April, 1923) she was still very well and remained so till November, when examination revealed bronchitis as usual, but no abdominal abnormality. From then onwards she steadily got worse, losing weight and looking ill, but at no time was any complaint made of abdominal symptoms. Only in March, 1924, did she complain of some pain and discomfort in the right loin, and the splenic enlargement was discovered. There have been no hæmorrhages in the course of the illness, and the catamenia were established between January and April, 1923.

There is no point of importance in the family history.

On admission the girl, who was then just 14 years of age, was thin and pale, looking somewhat ill but not obviously anæmic. There were no abnormal signs in the chest; heart and lungs appeared normal. The abdomen was large and permeated with dilated and tortuous veins running over the anterior wall in the epigastric region. On palpation the spleen was found to be enormously enlarged and filling the left hypochondriac and lumbar regions. Its anterior margin, in which the notch was readily palpable, was rounded, regular and very hard, but not tender; it reached practically to the umbilicus. The lower pole reached the left iliac fossa, within 2 in. of Poupart's ligament. The whole organ was firmly fixed and little, if any, movement could be imparted to it. The liver, too, was greatly enlarged and the lower

edge—round, hard and regular—was felt 3½ in. to 4 in. below the costal margin. There was no other mass to be felt in the abdomen and no ascites.

There was no general glandular enlargement, but small, hard glands were palpable in the neck, axilla and groins.

The eyes showed the changes typical of leukæmia—redness, swelling of the discs, extreme congestion of the vessels, which are very tortuous, and a few retinal hæmorrhages.

The urine was normal, but gave a heavy deposit of urates on standing.

The blood gave a count of 1,820,000 red cells per cubic millimetre, with anisocytosis and poikilocytosis, but no polychromatophilia nor granular degeneration. There were a few erythroblasts. White cells, 740,000 per cubic millimetre, showing a typical picture of myelogenous leukæmia. Dr. Piney very kindly gave me the result of a differential count which he made, which is as follows: Polymorphs, 30 per cent.; eosinophils, 2·5 per cent.; basophils, 5 per cent.; lymphocytes, 0·5 per cent.; metamyelocytes, 6·5 per cent.; myelocytes, 3 per cent.; premyelocytes, 44·5 per cent.; eosinophil myelocytes, 4·5 per cent.; basophil myelocytes, 0·5 per cent.; myeloblasts, 3 per cent.

Dr. Piney considers that the condition is a comparatively acute myelosis. Judging by the characters of the blood cells the disease closely resembles acute myelogenous leukæmia, but the clinical symptoms are by no means rigidly in accordance with that diagnosis; there have been none of the signs generally associated with acute leukæmia, such as breathlessness, hæmorrhage, swelling of the extremities, and so on. This case thus seems to come somewhere between the acute and the chronic forms of the disease.

As to treatment, we have tried benzol, but she was unable to take it in the capsules; when she tried to do so they burst in her mouth and caused much distress. We therefore prepared a solution of benzol in paraffin, which she took well and has now been taking for five weeks. The result of that treatment, however, we find to be absolutely negative; neither the spleen nor the liver has diminished in size, and the blood-count has altered only in so far that she has now 640,000 white cells per cubic millimetre instead of 740,000. Such a difference is within the limits of experimental error, and there is probably little or no difference in the actual state. The characters of the cells now present do not vary from those of the cells originally found.

DISCUSSION.

Dr. DONALD PATERSON asked whether it was the intention of Dr. Wyard to use arsenic in this case and whether he had had experience of giving one of the salvarsan substitutes, such as novarsenobillon, or salvarsan itself. Or had he given arsenic by the mouth in large doses? Two or three cases certainly had been shown before this Section in the last four or five years which had done extraordinarily well on arsenic administered intravenously. In a case shown at the Section two years ago, the symptoms practically cleared up, and remained absent for at least a year. A similar case was shown at the meeting of the Section held at Oxford, and in that case also the symptoms cleared up after intravenous use of arsenic.

Dr. WYARD (in reply) said he had tried arsenic in some cases of the kind, but he was not able to say from that experience whether it did good or not, because the patients had been treated with X-rays also. For the present patient he proposed to try the effect of X-rays before using arsenic.

A Case of Xanthoma Tuberosum.

By G. A. Harrison, M.B.

(A) Clinical.

Dr. Donald Paterson's patient is a girl, aged 8 years and 10 months. There are xanthoma nodules on the right elbow and right knee. There was on the left elbow a larger nodule, which has been excised for examination.

The last-mentioned first appeared about three years ago. Those on the right elbow appeared last year, and those on the knee this year.

(B) Histological.

The sections which you have seen were prepared by Dr. A. M. H. Gray, who, unfortunately, is unable to demonstrate them himself. I would draw your attention to the large number of doubly refracting droplets which are cholesterol esters.

(C) Chemical.

As is usual in xanthoma, whether of the diabetic or non-diabetic variety, this case shows hypercholesterolæmia, and an excess of cholesterol in the deposit in the skin :—

Blood cholesterol (fasting plasma) = 641 mgm. per 100 c.c. (normal 190—310 : Bloor's method)
Cholesterol content of deposit in skin expressed as per cent. of ether extract = 29·4.
Cholesterol content of deposit in skin expressed as per cent. of dry weight = 9·2.
Cholesterol content of deposit in skin expressed as per cent. of wet weight = 2·8.

The two points that interest me are (1) whether the diabetic and non-diabetic varieties of xanthoma are different grades of the same condition, or are separate diseases ; (2) whether disorder of the liver or pancreas or both, or of some other organ or organs, is responsible for xanthoma.

This case is at present under investigation from these points of view. So far the following results are available.

The patient has a perfectly normal blood sugar curve after 50 grm. dextrose. There is no glycosuria, and nothing abnormal in the urine. Van den Bergh's test is normal, though the blood plasma is definitely more yellow than usual, probably due to an excess of circulating lipochromes. There is no naked-eye lipæmia, and the fæces are normal both macroscopically and microscopically.

This case, therefore, belongs to the non-diabetic group. It remains to be seen whether further investigation points to any disorder of liver or pancreas, or not ; and whether later in life evidence of diabetes accrues.

For further details of the pathogenesis of xanthomatosis I would refer those interested to a paper by R. H. Major in the Johns Hopkins Hospital *Bulletin*, January, 1924, vol. 35 (p. 27), and to a paper by Dr. Whitfield and myself in the *British Journal of Dermatology*, March, 1923, vol. 35, p. 81.

Treatment.

Insulin has been used successfully in the diabetic variety of xanthoma, when the usual fall in blood sugar has been accompanied by a fall in blood cholesterol, and a partial or complete disappearance of the skin lesions.

I dare not use insulin for this patient in her own home—as an out-patient —because her blood sugar is normal, but I hope to try it in hospital, where we

can make sure of an ample carbohydrate intake, and can make the necessary determination of blood sugar and blood-cholesterol.

Apart from insulin, I know of no medical treatment likely to cause the disappearance of the skin deposits. The latter are of course often removed surgically either because they have become tender owing to local trauma, or because they are regarded as unsightly.

Prognosis.

The deposits in the ordinary way do not absorb spontaneously. More deposits may occur at any time.

I hope to watch this case to see if any evidence of diabetes develops and I wish to thank Dr. Paterson for his kindness in referring it to me for investigation.

DISCUSSION.

Dr. F. Parkes Weber said he regarded this as a very interesting case, not because of the rarity of cutaneous xanthoma in children (though it was rare, a good many cases had been published), but because of the care with which this particular case had been investigated.

He took a rather different view of the case from that expressed by Dr. Harrison; he (the speaker) thought it should be looked upon primarily as one of very marked hypercholesterolæmia, and secondarily as one of cutaneous xanthoma (a symptom of hypercholesterolæmia). In fact he considered this patient as one having a peculiar metabolic abnormality which might be termed a "cholesterol diathesis."[1] If Dr. Harrison could manage it, it would be valuable to have the cholesterol-content of the blood of the patient's relatives determined: of mother, father, brothers and sisters. Only a few of all the subjects of hypercholesterolæmia ever developed cutaneous xanthoma, but every subject of cutaneous xanthoma had or had had hypercholesterolæmia.

The cause of this "cholesterol diathesis," whether it was congenital, whether familial, or whether due to some acquired disturbance of the viscera concerned in cholesterol and fatty metabolism, he would not discuss at present. Diabetes mellitus should be regarded as only one cause of hypercholesterolæmia. This patient might never and probably would never have diabetes mellitus. "Xanthoma diabeticorum" was due to the hypercholesterolæmia that accompanied and was caused by the diabetes mellitus, and so it might disappear when the glycosuria (the hyperglycæmia) was got rid of under insulin treatment. Cutaneous xanthoma might be due to hypercholesterolæmia of other causation, as in the present case.

He thought the great point in the present case was to see that the hypercholesterolæmia should do the patient as little harm as possible, that is, that the patient's life should be regulated as far as possible so as to avoid the special dangers arising from the "cholesterol diathesis." An effort should be made to reduce the hypercholesterolæmia by diet, avoiding excess of meat-fats, eggs, &c.; excess of fat appeared to favour the deposition of cholesterol (anisotropic cholesterol esters) in the tissues and the formation of cholesterol gall-stones. If these measures brought down the excess of cholesterol in the blood, the patient should be regarded as lucky to have developed cutaneous xanthoma early in life, because the xanthoma of the skin, which everybody could see, indicated a special tendency to aortic atheroma and other kinds of internal "xanthomatosis," using the latter term, according to Chalatow's nomenclature, to mean pathological deposits of cholesterol in the interstitial tissues of various parts. Obviously this patient was threatened with atheroma ("xanthomatosis") of the aorta. If in this patient the hypercholesterolæmia could be reduced, the danger of severe atheromatosis ("xanthomatosis") of the aorta and cardiac valves would be diminished. Otherwise this patient was almost certain to get severe atheromatosis of the large blood-vessels. Even if the hypercholesterolæmia could not be reduced,

[1] Cf. the writings of Anitschkow, Chalatow, &c., especially Chalatow, *Die anisotrope Verfettung im Lichte der Pathologie des Stoffwechsels* (*Die Cholesterindiathese*), Jena, 1922.

something could probably be done to lessen the danger of developing severe aortic atheromatosis, by avoiding excessive strain and other causative factors of arterial hyperpiesia. Indeed, all the various known exciting causes of atheromatosis of the aorta and cardiac valves should be thought of. Since any kind of infection might favour aortic atheroma, the patient should be shielded as far as possible from febrile catarrhs and so-called influenzal attacks.

Dr. HARRISON (in reply) said he was much interested in Dr. Parkes Weber's suggestion, and, if possible, he would investigate the cholesterol content of the blood of the relatives. He would like to know from Dr. Weber whether there was evidence of any harm having happened to patients because of their hypercholesterolæmia. He had seen only one other case of the kind, and that had been under the observation of Dr. Whitfield for more than twelve years. He did not know there had been reported any case in which there was hypercholesterolæmia antecedent to the atheroma; he thought the cholesterol changes in atheroma were regarded as purely secondary changes, not that the hypercholesterolæmia was primary. He would endeavour to ascertain, in this case, whether there was any change in fat metabolism, or any increase of fat in the blood. In the diabetic variety it was not only the metabolism of cholesterol which was affected, but also the metabolism of true fat and of carbohydrate.

Dr. PARKES WEBER (in reply to Dr. Harrison) said he thought that evidence existed that both before cutaneous xanthoma and before arterial atheroma occurred there was a hypercholesterolæmia; atheromatosis was a form of xanthomatosis, in Chalatow's use of the latter term.

The presence of definite cutaneous xanthoma showed either that the patient had at present hypercholesterolæmia, or had had it at some time; and he thought one could say the same thing in regard to the causation of atheroma of the aorta and cardiac valves. But it should be remembered that local inflammations predisposed to local cholesterol deposits. Thus, hypercholesterolæmia, as a cause of arterial atheromatosis, was aided by the occurrence of local inflammations of the inner coat of the arteries.

Case for Diagnosis.

Shown for C. P. G. WAKELEY, F.R.C.S., by R. C. LIGHTWOOD, M.B.

PATIENT, an infant, aged 11 months, showed the following clinical conditions:—

(1) MULTIPLE BONE DEFORMITIES.

These consist of a peculiar type of skull, similar in appearance to oxycephaly, but not identical with it, and multiple deformities of the skeletal bones, the most striking of which are the rudimentary ulnæ and radii on both sides.

(2) SKIN LESION.

The skin condition is very peculiar, and consists of a hitherto undescribed rash widely distributed on face, limbs, and trunk. This rash commenced as an erythema at the age of 3 months, and developed into a condition of capillary dilatation with pigmentation and desquamation. Two other members of the same family are afflicted with the same skin lesions.

DISCUSSION.

Mr. H. A. T. FAIRBANK said he saw this child in hospital recently, and the arm condition interested him very much. He did not remember having seen a case with a head of this shape, associated with deficiency in the radius. Absence, or great

deficiency, of thumbs was usual in these cases; the feature in which this case was so unusual was that the ulna was very much curved and the radius seemed to be holding it like a bowstring. In the cases he had previously seen the ulna usually seemed to be very fairly developed. Not long ago he saw a case with the ulna absolutely straight, and it was also of very fair length. In that case both arms were affected. On one side there was no radius, and on the other side there was a small piece at the upper end, and this was fused to the ulna.

This present case was a very difficult one to deal with, as far as the forearms were concerned. In the other cases he had seen, one could gradually get the hand more or less into line with the forearm; but in the present case one could not hope to do much without dividing or removing the radius, and even if that were done, the soft tissues would be very tight. All that could be done, he thought, was to try to elongate the radius by daily manipulations, and, when the child was older, to endeavour to get a splint on to hold it. He wished to call attention also to the lower part of the spine; he thought there was a deficiency in the neural arches of the spine in the lumbar region, though there was no protrusion of the membrane.

Dr. H. C. CAMERON thought the case was one of oxycephaly. He reminded members that some years ago he showed two cases which came under his care at the same time, which had a combination of "tower skull" and polysyndactylism. Shortly after showing these cases an exhaustive article in the *American Journal of Diseases in Children* called attention to the frequency of the combination of gross deformities of the fingers and toes with the condition of tower skull. In that paper, though many cases were recorded from the literature, there was, he thought, no example of this absence of radius and hallux. Recently he saw a child whose radius was absent, though the ulna was well formed. The mother told him that the child's father had the same deformity. But though the father's hand had been untreated, it was a clever hand, even in the absence of the thumbs. He was a shoemaker, and did his work quite well. He asked whether, in general, hands of this kind suffered from any serious incapacity.

Dr. DONALD PATERSON reminded members of two cases which Dr. Jewesbury[1] showed at the Section three years ago, of oxycephaly associated with syndactyly, showing that this form of skull was associated with multiple congenital malformations. Also, the children were mentally defective; certainly one of Dr. Jewesbury's patients was definitely backward mentally: it was sufficiently old to show it distinctly.

Dr. F. PARKES WEBER expressed the hope that the case would appear with "oxycephaly" or "turriform head" as part of the title. Among the conditions sometimes associated with tower-shaped head a curious one had been described on the Continent, namely, familial acholuric or hæmolytic jaundice. This association had been observed by M. Gänsslen[2] amongst certain families in the neighbourhood of the university town of Tübingen. Hæmolytic jaundice and "turriform head" both tended to run in families, and the association of the two conditions in the same family were not surprising in a neighbourhood where both conditions were of not very uncommon occurrence.

[1] *Proceedings*, 1920-21, xiv, p. 27.

[2] M. Gänsslen, "Ueber hämolytischem Ikterus," *Deut. Arch. f. klin. Med.*, Leipzig, 1922, cxl, pp. 210—226.

Section for the Study of Disease in Children.[1]

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

Demonstration of Cases illustrating Three Types of Chorea.

By C. J. Macalister, M.D.

Dr. Macalister showed a number of cases illustrating the results of an investigation which he had carried out over a series of years with reference to chorea. He classified these cases as: (1) The ordinary rheumatic or toxic types. (2) Stock-brained cases related to an inherited tendency. (3) A climacteric type.

He first distinguished choreas which were rheumatic or which developed endo- or pericarditis from those which did not, and which were often ascribed to fright or mental upset, or some peripheral irritation. Inquiry into the histories of both types revealed the fact that in the choreas caused by fright, &c., the children were sometimes themselves left-handed, or, if right-handed, came of a left-handed stock; whereas, in the rheumatic cases, this kind of history was only occasionally presented and probably represented rheumatism in children who had the hereditary tendency referred to. The non-rheumatic cases would therefore be spoken of as stock-brained choreas. An interesting point in connexion with these cases was the occasional occurrence of stammering in the families. Dr. Macalister regarded the choreic movements as representing a gross motor ataxia analagous to vocal stammering. These choreas probably did not occur in children of a purely right-handed strain, nor would they occur in those of a purely left-handed strain (if such were possible), but left-handedness uninfluenced by a right-handed inheritance must be extremely rare. From the histories of these cases it was evident that the explanation was to be found in a partial or repressed transposition of cerebral function, i.e., they occurred in right-handed children in whom there was a stock-brained left-handed influence at work, and in left-handed children in whom the repressed right-handed inheritance showed its influence. As a consequence of fright, emotion or peripheral disturbances, an unbalancing of coördination was brought about by some unusual relation of function on the opposite sides of the brain.

The speaker referred to Hughlings Jackson's theory that the speech centre in the left brain was a leading one, and its fellow in the right brain an automatic one—a theory accounting for the repetition of words and recurring utterances of sentences in a "barrel-organ" way in some cases of right aphasic hemipelgia. The left centre being put out of action by the stroke, the right one repeated the impression it had received at the time of the hæmorrhage. As an example of this Dr. Macalister mentioned a case in which the patient, from the time of his seizure to that of his death, repeated the words, "I want some brandy"; this was, no doubt, the result of his feeling sick and

[1] Provincial Meeting of the Section, held at Liverpool, June 27 and 28, 1924.

faint at the time of the hæmorrhage. Hughlings Jackson had explained that these expressions probably came from the right automatic centre after being initiated in the left leading one immediately before its destruction. This kind of experience possibly threw some light on the relative functions of the two centres in a normal individual, and it could well be imagined that when a stock-brained influence impressed a tendency for automatism in a leading centre or for initiation in an automatic centre, the incoördination expressed as chorea might be easily brought about, the slender balance being upset under the influence of fright or other peripheral disturbances. In discussing this type, Dr. Macalister mentioned two cases of congenital asphasia, in both of which the patients were left-handed and presented collateral sinistral histories. Neither of them was deaf; one was somewhat defective mentally, the other (aged 6 years) was absolutely bright and quite up to the standard of intelligence for her age.

A short reference was next made to the histories of non-rheumatic choreas occurring at or about the period of puberty. The speaker said that, as in the stock-brained cases, the disorder was frequently precipitated by emotional causes. These cases were due to a chemical or metabolic disturbance as distinct from the anatomical incoördination just mentioned.

The Lymphoid Apparatus as a Culture Centre for Septic Organisms.

By P. W. Leathart, M.B., B.C.

The lymphoid apparatus is usually regarded as a filter arrangement for micro-organisms and other foreign bodies. Let us take the spleen and tonsils as examples.

The Spleen.—On microscopic examination the cells of the splenic pulp are seen to be full of particles of brown pigment or fragments of red corpuscles. In many cases of infectious disease the splenic cells are observed, towards the end of the attack, to be full of organisms to which the disease is due. The spleen is evidently a blood filter.

The Tonsils.—May be compared in structure to a modified lymphatic gland—"modified" because they possess numerous crypts, the mouths of which open directly into the pharynx.

The crypts always contain numerous micro-organisms; those of the common air-borne diseases predominate.

In this sense we must look upon the lymphoid apparatus as a protective mechanism. I shall, however, presently bring forward strong evidence pointing to the fact that, whilst a lymphoid organ acts as a filter, it may become a culture centre for organisms which it has taken up. It may thus become a septic focus—latent at one time, active at another—which may cause repeated auto-re-infection, or as a "carrier" it may be dangerous to others. As is the case with all septic foci under favourable conditions, it may actually benefit the individual by gradually developing immunity on the same principle as vaccination. On the other hand, it may become a source of active infection and ill-health. It will easily be seen that if the facts are really as stated we shall have to review our methods of treatment of disease in many instances. Obviously a lymphoid body which no longer acts as a filter but becomes the seat of a culture of infective organisms, ought to be removed.

I will try to demonstrate my thesis by reference to clinical observation and pathology.

There are three routes by which infections are commonly introduced into the human body. They may be: (1) Air-borne; (2) food-borne; (3) blood-borne.

In air-borne infections—for instance, the "common cold"—the tonsils and adenoids frequently, and indeed generally, become swollen and inflamed. In the case of food-borne infections the lymphoid tissue of the intestine is frequently involved and in blood-borne infections an enlarged spleen is a prominent physical sign.

(1) *Air-borne Infection.*—Let us consider the pathology of the commonest of all air-borne infections—the common cold. Its symptoms are familiar to all of us, for all or nearly all of us experience intense discomfort from it for two or three weeks every year. The physical signs are not so well known.

They are most marked in a young child who has not yet acquired all the immunity he is capable of acquiring, and in such a child they may be said to be as follows:—

(1) The pillars of the fauces and the tonsils are redder than normal and the tonsils are often considerably swollen.

(2) Small red lymphoid granules are seen on the post-pharyngeal wall.

(3) A large red patch, distinctly redder than the surrounding mucous membrane, is seen in the "adenoid area" and this patch, like the tonsil, is generally very much swollen.

(4) The cervical glands, especially those near the angle of the jaw mainly in the anterior triangle, are always definitely enlarged and sometimes tender.

In short, wherever lymphoid tissue is present in the mouth or post-nasal space, inflammation is apparent to the naked eye, and the glands draining the area are found to be enlarged. In other words, the micro-organisms causing this infectious disease have attacked the faucial and post-nasal lymphoid tissue and from the latter, toxin has been transmitted through the lymphatic vessels to the glands, causing them to become enlarged.

This can be verified by cultures taken from the mucus either of the nose or post-nasal space. One or other of the common cold organisms is always grown —sometimes in pure culture, sometimes in mixed.

The commonest are: (1) *Pneumococcus*; (2) *Bacillus catarrhalis*; (3) *Bacillus influenzæ*; (4) *Bacillus Friedländer.*

If we make examinations at various periods during the acute stage, and at the same time make cultures from the mucus, we find that as the inflammation of the tonsils and adenoids subsides, the number of micro-organisms decreases. As soon as the tonsils and adenoid tissue have regained their normal size and appearance, the mucus will be found to be almost sterile so far as these organisms are concerned.

The clinical course of the "common cold" varies considerably of course in different individuals, but, broadly speaking, the description just given is of what happens most frequently—the lymphoid tissue is attacked, toxins are absorbed, antibodies are formed which neutralize the toxin, the patient recovers and remains well for a longer or shorter period, according to the amount of immunity he has acquired. We must remember, however, that this immunity is seldom or never complete. It usually only lasts a short time and then another cold is caught.

In the intervals between one acute cold and another the patient is free from discomfort, and—what is more important—is not a source of danger to others.

In a minority of cases, however, this comparatively satisfactory state of affairs does not occur. On the contrary in these cases although the acute stage tends to pass off, the tonsils and adenoids do not regain their normal size and appearance, the glands draining them do not decrease in size and the organisms remain, though in reduced numbers, in the mucus of the nose and post-nasal space. The disease has in fact taken on a "subacute" form. This stage may—and often does—last for an indefinite period, it may be for months or even for years, and during the whole time, since the organisms are present, the patient must be regarded as a carrier spreading infection broadcast to his healthy fellows.

In addition, during this subacute or carrier stage, the patient appears to be extremely susceptible to re-infection by the common cold organisms, for he undoubtedly reverts to the acute stage more frequently than does the normal person and he is also more liable to acquire one or other of the common complications, such as lobar or broncho-pneumonia.

Tuberculous affections, evidenced by tuberculous glands in the neck, are apparently more easily acquired in the subacute stage, as are possibly other specific diseases, more particularly rheumatic fever and acute nephritis, for in these diseases, in addition to the physical signs of the specific infection, we frequently find those of the common cold in its subacute stage.

We find then that there may be two stages in what is generally called the "common cold": (1) An acute stage which soon passes off; (2) A subacute or carrier stage in which the physical signs remain for an indefinite period and during which complications are apt to occur and other specific infections are liable to be picked up.

The acute stage is of course much more common than the subacute, for every child suffers from it many times. In the majority of cases, however, the mechanism for toxin absorption works in harmony with antibody formation; the child recovers and remains well between the attacks and requires no treatment other than medicinal and hygienic, in spite of the fact that the tonsil tissue and adenoid tissue may remain enlarged for a time and the cervical glands may be palpable.

In the exceptional cases (which, however, are not infrequent) the acute stage is succeeded not by a period of healthiness, but by the subacute stage. The one fades imperceptibly into the other, so that it is difficult to say where one ends and the other begins. Clinically, however, the two types are distinct and can generally be distinguished without much difficulty.

In the subacute stage the tonsils and adenoids and the glands draining them remain enlarged for an indefinite period; the child is not healthy in the short intervals between his acute attacks, he is a carrier, spreading disease, liable to infection by other air-borne diseases and to the manifold complications of the common cold, and is, in fact, a danger both to himself and to the community at large.

What then is the reason for this condition of things? The answer is that a culture centre of infective organisms has been established in the lymphoid tissue and that this becomes active and thus gives rise to auto-re-infection.

Treatment.—It is hardly reasonable to expect this subacute stage to be cured by means of drugs, whether they be swallowed, inhaled, sniffed up the nose or injected hypodermically. Experience has shown that they fail signally, with the possible exception of iodine, which appears to help in some cases. Vaccines, although they have had a thorough trial, are disappointing; this should not surprise us when we remember that already too much is being absorbed.

We are, therefore, forced to look for help to surgery and we find that by means of a complete removal of the tonsils with their capsules, and of the adenoid mass, the desired result is obtained—we remove the factory and stop the absorption.

Judged by results, this operation must be regarded as one of the greatest triumphs of surgery, for in a few weeks, often in four or five days, there is marked improvement in the patient's health, the enlargement of the cervical glands and the rhinitis disappear, the organisms die out and the complications, especially discharging ears, clear up.

The child, though he may still catch colds, does not do so more often than the normal child, nor is it possible for him to sink again into the subacute stage, for the organs which were responsible for that condition have been removed.

The results from partial removal are not nearly so good, in fact if this be done in a case well advanced in the subacute stage the child does not appear to receive any benefit at all, for the septic focus has not been removed. It is more than likely that the apparent benefit which is sometimes said to follow after a partial removal is really only to be observed in those cases which have been submitted to operation in the acute stage, and in which improvement would have taken place in any case.

The removal of tonsils and adenoids should always be complete, and should be confined to those cases which, so far as the common cold is concerned, are well advanced into the subacute stage. Therefore it is extremely important to be able to recognize such cases. This may seem to be a simple matter, yet in practice it does not appear to be easy, for a wrong diagnosis is frequently made. The chief reasons for this are:—

(1) That the tonsils and adenoids are by no means always obviously enlarged, and may be quite small.

(2) That it is in many cases difficult to get a view of the post-nasal space in children.

There are, however, always other physical signs, which are easy either to see or to feel, e.g., there is nearly always a rhinitis, and there is always enlargement of the cervical glands.

In addition, the history will help materially, and there is often some complication such as deafness, recurrent earache, aural discharge or cervical abscess.

Practically it is proper to advise a complete removal in any case in which there is rhinitis accompanied by enlargement of the cervical glands, and in which the history points definitely to chronicity.

Very often, too, the presence of some complication makes it easier to decide in favour of operation in a case which otherwise would be doubtful. Enlargement of the tonsils alone is not a reliable guide. In all cases the tonsils are larger during the acute than in the subacute stage. If enlargement is present, in addition to the other physical signs, it no doubt confirms our opinion, but there does not appear to be any clinical evidence that enlargement and the amount of absorption of toxin go hand in hand, for it is a common experience to see cases well advanced in the subacute stage, with large cervical glands, chronic rhinitis, and one or more complications, but in which the tonsils and adenoids are quite small, yet after complete removal the glandular enlargement and other complications rapidly and completely disappear. In my opinion many cases in the subacute stage are not recognized, because in the diagnosis too much importance is attached to the size of the tonsils and too little to the

glands in the neck and the history. The result is that many children remain untreated who would materially benefit by operation, Some of them perhaps recover in time and become healthy adults, but others contract complications such as rheumatism, tuberculosis, pneumonia, mastoiditis, or middle-ear deafness, and either die or grow up crippled in some way or another and swell the ranks of the unfit.

Thus in the common cold we have an illustration of how the lymphoid apparatus may become a cultural centre of infective micro-organisms.

That an infected lymphoid body may eventually benefit the individual follows from the following consideration.

If we consider the faucial and post-nasal lymphoid tissue, there will probably be general agreement:—

(1) That it is relatively more abundant in children than in adults.

(2) That it may be removed in the case of adults and of children aged over 4 or 5 years with great benefit and without any known ill effects, but

(3) That in the case of children younger than this, ill effects have supervened later on in life.

From this it may be deduced that the lymphoid tissue is of definite value to young children, but when they are grown up it has to a great extent served its purpose, and it is not required by adults in anything like the same degree.

What then is its value or purpose?

It is suggested that the faucial and post-nasal lymphoids are culture centres for infective organisms, and that the effect of these foci of infection is to produce gradual immunity against the common ailments.

It follows from this that if adenoids are removed when children are less than 5 years old, the condition under which immunity takes place is also removed. The function of the tonsils in infants is to act as "vaccination centres" of common ailments.

Young children, as is well known, are extremely prone to suffer from infections called by different names, such as "teething fever," "liver attacks," "chills" and "colds," none of which are so apt to attack the adult.

This is generally explained by saying that the adult is tougher or stronger, but the real reason, it is submitted, is that during his growth from childhood to manhood he has suffered from and recovered from numerous attacks of the diseases in question, and in doing so has acquired an immunity he did not possess as a child.

The average child, in fact, appears to spend a large portion of the first four or five years of his life in acquiring immunity to pathological organisms.

(3) *Blood-borne Infection.*—I need not enlarge much on the subject. The most important sign of infective endocarditis, and of suppurative and other fevers is an enlarged spleen. By puncturing the spleen in these conditions we often obtain the specific microbes, though a culture of the blood gives a negative result.

Possibly the most instructive condition which favours my hypothesis is malaria. It is well known that a patient who has been resident for years in a district where a fresh infection is impossible may yet suffer a relapse from this disease. The spleen remains enlarged in an old malarial subject, and it is very likely that it is the focus from which re-infection takes place. It is suggested that in suitable cases of this and other infections, such as subacute endocarditis, it may be possible by removal of the spleen to remove the

focus of infection and to cure the patient. Of course this will only apply to conditions in which there are apparently no other foci of infection.

Mr. PHILIP G. CAPON showed a series of models, photographs and radiograms illustrating:—

(1) Dental deformities produced by bad habits, with the results of orthodontic treatment.

(2) Dental conditions in various stages of rickets.

(3) True Hutchinsonian dentitions and dentitions simulating that condition.

(4) Various dentitions giving rise to toxic absorption.

Specimens from Three Cases of Cerebro-spinal Leptomeningitis due to *Bacillus Influenzæ*.

By ERNEST GLYNN, M.D., F.R.C.P.

PROFESSOR ERNEST GLYNN showed museum specimens of the brain and spinal cord from three out of four cases of acute diffuse suppurative cerebro-spinal leptomeningitis, due to a *pure* infection with bacilli proved to be of the influenza group. He drew attention to the fact that the disease was not infrequent. There had been four fatal cases in three years at the Royal Liverpool Children's Hospital, which possessed about 120 beds and had a death-rate of about 150 per annum. All four patients were aged 2 years or under; in three the disease was associated with broncho-pneumonia and in two with acute fibrinous pleurisy. *Bacillus influenzæ* was also found in the bronchial secretion of the only two cases examined, but pneumococci were not found.

X-rays in the Diagnosis of Miliary Tuberculosis.

By HUBERT ARMSTRONG, M.D.

With Radiographs by C. THURSTAN HOLLAND, M.Ch.

Mr. Thurstan Holland much regrets that the date of this meeting coincides with that of his summer holiday, so that he cannot expound the technical side of the question. At his suggestion I have adapted a paper I had already written for publication in collaboration with him, but as he has not seen the new version he must not be considered to have given it his imprimatur.

I intend to deal with practically one aspect only of what is a large subject. The main point which I wish to illustrate is the utility to the clinician of X-ray examination in the detection of tuberculosis—more especially of miliary tuberculosis—in the absence of any physical signs of disease. The subsidiary points are questions which I have not solved and on which I seek enlightenment: Do miliary tubercles always show in the radiographs? and, if they do: how soon after the onset of the disease do the shadows, as seen in the three illustrations, begin to appear?

These illustrations are from more or less recent cases, of which the following are abbreviated records:—

(1) Edith C., aged 7 years, admitted March 22, 1921. The child came from an Orphanage under my care, and no reliable past or family history is obtainable. The Sister-in-charge of the Orphanage drew my attention to the girl on account of the recent appearance of deep cyanosis, occurring on the most trivial exertion. I saw the child more than once, and while suspecting miliary tuberculosis could not find the least sign of its presence in the lungs or elsewhere.

On admission: Child undersized, but nutrition good. Lips, cheeks and extremities become deep purple on slight exertion. Pulse 140 on examination, but usually charted between 90 and 110. Heart sounds fœtal in rhythm. Respirations 26. Temperature irregular—between 99° and 102·8° F. Tongue furred and teeth decayed. Fleeting erythematous rashes somewhat resembling measles on trunk and extremities. The rest of the examination gave entirely negative results. No blood examination was made. For the next week there was nothing further to note than a loss of 1½ lb. in weight, an occasional night sweat, and a temperature tending to keep more regularly within the

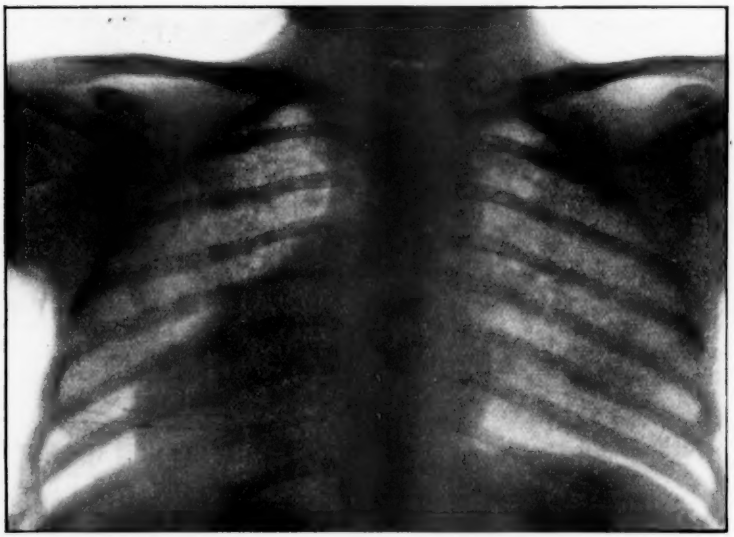

FIG. 1.

range of 100° to 102° F. Food was taken badly, and there was constipation so that aperients were needed. The radiograph (fig. 1) was taken on March 31, at which time all that could be said of the lung signs was that the breath sounds at the left apex were a trifle harsh and that there were no adventitious sounds anywhere. By the time, a week later, that I had secured the transference of the patient to the Fazakerley Sanatorium, râles had made their appearance all over both lungs, the temperature was becoming hectic, and both pulse and respiratory rates were mounting. The child died a fortnight afterwards.

(2) Edith B., aged 7 years, admitted April 22, 1921. A sister was in a sanatorium. The child had had measles, with double pneumonia, at 18 months. The present illness dated back only a fortnight, during which she had vomited occasionally, eaten little, been very listless, slept much, and lost weight. Bowels said to be regular.

On admission: The child weighed 29 lb. 9 oz. Temperature, 100·5° F. Pulse 120. Respirations 30. No other abnormality except a furred tongue and a flat abdomen with thickening and shortening of the mesentery, retracting the intestines. I diagnosed tuberculous peritonitis of the plastic type—probably an accurate diagnosis, though not going far enough. For twelve days there were no developments other than a gradual rise of temperature to the higher level of 102° F., constipation, and the loss of a few ounces in weight. A Widal reaction at the end of a week was negative for *Bacillus typhosus* and *paratyphosus* A and B. On May 2 and 3 the temperature fell, without any improvement occurring in the general condition. A few crepitations now became audible at the base of the left lung, and these were succeeded by rapidly increasing

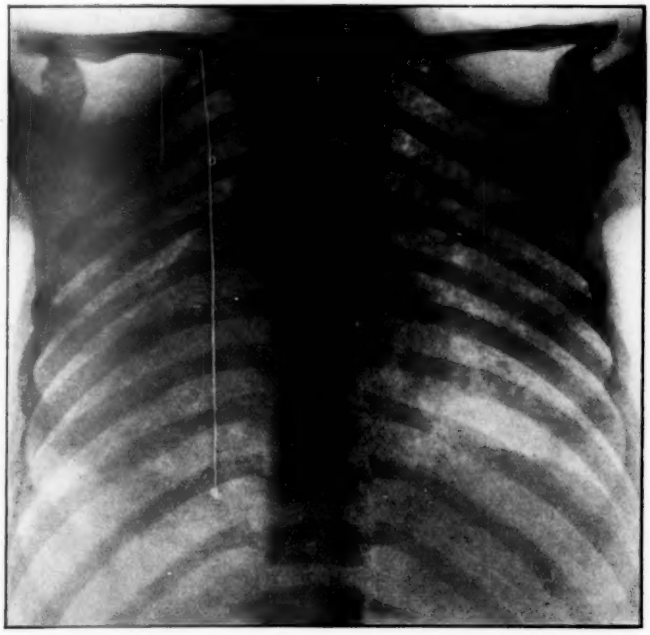

FIG. 2.

moist sounds of general distribution. On May 4 the temperature rose again, reaching 104° F. before death on May 10. The radiographic report received on May 6 confirmed the diagnosis of general tuberculosis arrived at after the exclusion of typhoid.

In this case it will be observed that the radiogram (fig. 2) was not taken until adventitious sounds had begun to make their appearance, but it shows such advanced lesions that we must presume they had been present for some time before these physical signs were in evidence.

Mr. Thurstan Holland considers the radiogram in the next case to be the best of the series.

(3) Arthur W., aged 11½ years, admitted March 28, 1924. He was one of nine children, one of whom has tuberculous cervical glands. Family much intermarried, and

a cousin died with tuberculous meningitis. He himself had had scarlet fever when aged 4 years, measles and whooping cough subsequently, and recurrent bilious attacks (probably acidosis) with some frequency. He had been ill with headache for a week, cold chills and fever for two days, and cough for one.

Condition on admission: Well nourished; tongue coated; abdomen tender on palpation; loose motions after oleum ricini; veins over thorax and abdomen prominent. Slight cough and a few scattered râles only in the lungs. Temperature 100° to 102° F., pulse about 120, respirations 28 to 32.

Four days later the morning temperature had fallen to normal, pulse descending to 108 after having reached 140, and the lungs were absolutely clear. This improvement proved quite transient, and despite the onset of finger clubbing I thought it as well to exclude

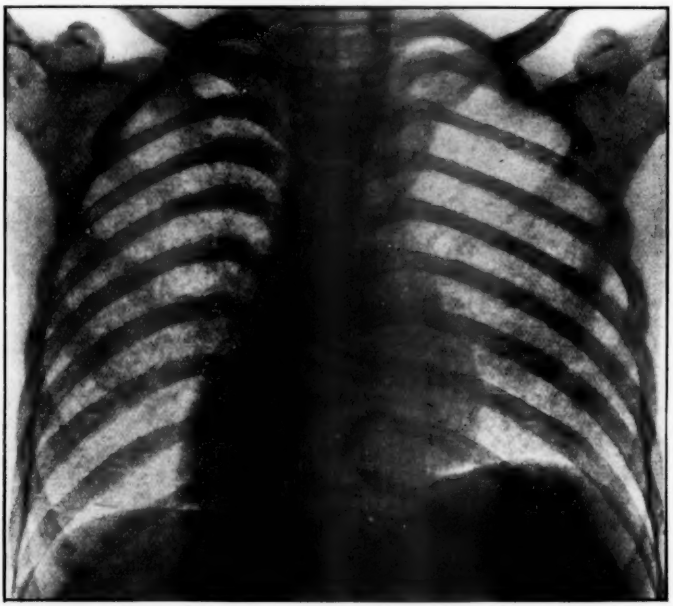

FIG. 3.

other infections than tubercle. The Widal reaction was negative for *Bacillus typhosus* and *paratyphosus* A and B, while blood cultures and subcultures remained sterile for four days. Definite cyanosis of cheeks, ears and hands, began to appear about ten days after admission, the temperature to assume a very erratic course, the finger-clubbing to increase. Nevertheless, on April 11 there was still only an odd rhonchus with very slight impairment of percussion resonance over the left lung in front; but the radiogram (fig. 3) showed advanced miliary tuberculosis. The boy was transferred next day to a Poor Law hospital.

No post-mortem was obtained in any of the cases, so it was impossible definitely to fix the portal of entry of the tubercle bacillus, nor does the radiogram help us. But in the absence of any other diagnosable focus it may fairly be assumed that these cases were like the majority in childhood, and that

diffusion of the disease took place from an affected gland either in the abdomen or thorax, with—or, equally likely, without—a primary lesion in the bowel or lung.

The tendency to dissemination, in childhood, is beautifully shown by the radiograms, and again it may be assumed, with absolute certainty, that miliary tubercles existed in almost every organ in the body. Carr[1] states that of the fatal cases of tuberculosis in childhood something like one-third die of a general miliary tuberculosis, in some of whom, as in these three cases, the pulmonary lesion predominates. In others the symptoms of tuberculous meningitis form the clinical picture.

The progress of the disease was rapid, but not phenomenally so. When the meninges are attacked death ensues earlier, but the total duration of the illness so far as it can be fixed by the onset of symptoms was under two months in the first case, in the second between five and six weeks. Rapidity of progress is not confined to cases of miliary tuberculosis, but is noted in cases of phthisis of the adult type, rare before later childhood. With these I am not specially concerned here, nor with the much commoner cases in which the starting-point is at the root of the lungs. I will only remark that, owing primarily to the depth of the lesions in the more chronic cases, and secondarily to the rapidity of the fatal termination, before any large cavities have had time to form, it is quite uncommon to obtain physical signs pointing to their presence, even though small ones are often found post mortem. The X-ray will demonstrate them earlier. The irregularity of the spread makes another type of case, impossible to differentiate from broncho-pneumonia, in the absence of sputum. Here even the radiologist will not always commit himself to more than reporting "shadows representing areas of consolidation." I may however remark in passing that in all cases of pulmonary tuberculosis, the radiographs almost invariably show more extensive lesions than physical examination reveals, unless the physician, in the interpretation of the physical signs, adopts the notation that 2 + 2 = not 4, but 6 or 8. The point on which we lay the chief emphasis is perhaps not a new one, but bears repetition when so well illustrated. To many it may be almost inconceivable that the lungs should be riddled to such an extent as is shown in Mr. Holland's films of my cases, with an entire absence of physical signs. But the patients were carefully examined, not only when we merely suspected the nature of the illness, but after we had seen the radiograms; when, in the first case, there had not yet developed anything to confirm the photograph, and, in the second, only a few sub-crepitant râles had been heard at one base. The reason is to be found in the fact that there is no consolidation of the lung tissue itself, or breaking down of the tubercles; so that the air distribution throughout the lung is even, and more than enough to mask the dullness which so apparently large an amount of solid matter scattered through the lungs might lead us to expect. Possibly there may have been some slight impairment of percussion resonance, but as it would be evenly distributed there was no standard by which it might be judged. The absence of any alteration in the *quality* of the breath sounds is less surprising, since there is no continuous mass of solidity to conduct bronchial breathing to the surface, and air entry to all parts of the lungs was not yet sufficiently impaired for one to be able to asseverate a difference from the normal. The first patients eventually developed the physical signs of

[1] Carr, J. W., Article on "Tuberculosis," Garrod, Batten and Thursfield's "Diseases of Children."

a generalized bronchitis. If there be a superadded catarrhal infection, such signs may show themselves early; and in fact, as in the third case, may disappear under suitable treatment, thus complicating the diagnosis and raising a fallacious element in the prognosis. Otherwise, any adventitious sounds are not caused by the tubercles—growing, as they do, in and around the blood-vessels—until from their increasing size and number they begin to occlude the bronchioles, or until the failing strength of the patient gives rise to passive congestion and ineffectual cough.

Now, at what stage do these miliary tubercles begin to show in the radiogram? Do they, in fact, always show at all? I believe not. In a quite recent case—that of a girl, aged 14 years, from another orphanage—I first saw the patient and admitted her to hospital on December 4, 1923, before the child had made any complaint herself, the Matron having picked her out for my examination because she thought her "looking off colour." She was found to be running a temperature of anything between 100° and 104° F. with a pulse of about 120 and an average respiratory rate of 32. She was painfully thin, but had never been robust. The physical signs were those of bronchitis. I sent her down to the X-ray room fully expecting the report "miliary tuberculosis," but Mr. Holland's verdict was negative. Clinical examination, blood cultures and so on, threw no fresh light on the case, and a fortnight later I had another X-ray examination made of the chest, and again received a negative report. The diagnosis of acute pulmonary tuberculosis became perfectly obvious, but on February 8 Mr. Holland again reported no X-ray evidence. The child then continued to go downhill, gross tuberculous masses became palpable in the abdomen, and she was too ill to be carried about for further radiographic examinations. Unfortunately, a few days before she died, my ward was closed for cleaning, and I had to send the girl to a municipal hospital, where I believe no post-mortem examination was made.

This case did not run quite so short a course as the others, as the patient lived some four and a half months after the onset of recognized symptoms, though her death appeared imminent for weeks before it occurred. Yet for two and a half months the radiograph showed no abnormality. I admit that the diagnosis was not clinched by autopsy, or by finding tubercle bacilli — all efforts to obtain satisfactory sputum failing. But the patient was seen by colleagues and others who had no doubts about its correctness. Why, then, should the X-rays exhibit miliary tuberculosis in one case and fail to do so in another? In my experience this is not so in the case of chronic phthisis. As I have said, the radiograph almost always shows more in that form of pulmonary tuberculosis than the physical signs have demonstrated. No doubt the answer is to be found in differences in the histological and chemical formation of the tubercles, their calcium content possibly, and the amount of fibrosis present, though I have not, as yet, any evidence to offer. Where permission for post-mortems has to be obtained suitable cases for the investigation of this point are not frequently forthcoming, especially in a children's general hospital which fights shy of "open" tuberculosis. Meanwhile the ascertained facts are that miliary tuberculosis will often show up on the radiogram in the entire absence of physical signs (as apart from symptoms) and that, on the contrary, even in the presence of typical signs of that condition X-rays will sometimes fail to demonstrate them.

Section for the Study of Disease in Children

Instrument used with the Shipway Apparatus in Operations for Cleft Palate.

By R. J. MINNITT, M.B.

Dr. R. J. MINNITT showed a Shipway apparatus, and a motor pump with sucker for extraction of blood from the pharynx in cleft palate operations. He pointed out that the success of the anæsthetic given for these cases depended upon the position of the catheter carrying the warm ether vapour, in relation to the sucker, and also upon the heat of the water in the thermos flask of the Shipway apparatus.

Observations on Asthma.

By C. P. LAPAGE, M.D., and F. S. ADAMS, M.D.

I.—Dr. LAPAGE.

IT is largely with the question of causation of asthma that Dr. Adams and myself will deal this afternoon, and especially with those causes related to metabolism and to local lesions.

Asthma is one of the manifestations of the exudative diathesis. (Those commonly accepted are urticaria, eczema, hay-fever, angio-neurotic œdema and asthma.)

I hold, however, that we must consider the possibility of including troubles like cyclical vomiting, bilious attacks and certain nervous signs and gouty symptoms.

Causes.—If we examine the causes of asthma we find that they can be arranged as follows:—

FACTORS DETERMINING ASTHMA.

- Individual is { exudative / sensitized
- External irritants { (1) Foreign proteins { animal / vegetable / egg / milk / bacteria ; (2) Climate
- General system excess of { Food { Fat / Acid ; Exertion ; Emotion
- Internal irritants (Local lesion) { (1) Nose and throat ; (2) Lungs and bronchi ; (3) Intrathoracic glands ; (4) Adhesions, fibrosis

The action of pollens and other external irritants opens up a wide field for discussion but we can hardly enter into it this evening.

Inheritance.—The majority of cases of asthma are in individuals of the exudative diathesis. Illustrative family histories can readily be found.

It is my personal opinion as a result of clinical observations that there is often a gouty family history in such cases. I have therefore made out a fresh list with manifestations of the exudative state, adding in brackets those which I think should be included.

Manifestations of the exudative diathesis {
(1) (Curd dyspepsia)
(2) Urticaria
(3) Eczema
(4) Hay fever
(5) Angio-neurotic œdema
(6) Asthma
(7) (Cyclical vomiting)
(8) (Bilious attacks)
(9) (Gouty signs)
}

I feel fairly sure when I include cyclical vomiting among the signs of exudative diathesis. This cyclical vomiting is often coincident with a local lesion in the abdomen, similar to that in the chest. I have frequently noticed a close relation between enlarged glands in the abdomen and cyclical vomiting attacks, the vomiting attacks culminating and subsiding with the glandular trouble.

The local lesion determines the site and weakens the tolerance in one system and a metabolic shock readily brings on an attack. A certain form of curd-dyspepsia is also probably an exudative sign and it is not necessarily due to proteins; it is quite likely to be due in these cases to faulty absorption of fats. Dr. Adams will deal with this point later.

It is particularly however with the question of metabolic excess and with that of local lesion that we shall deal this afternoon.

Metabolic Shock.—To my mind, in an exudative subject an attack is often produced by a metabolic shock. Such a shock is caused by either excessive food, especially fats and acids, or excessive exertion or excessive emotion. It is a common experience to see attacks of asthma brought on by such factors and this also applies to cyclical vomiting.

As a consequence, in treating manifestations of the exudative diathesis, I usually begin by limiting fats and acids as the first step in dieting. Since the subjects of asthma are often thin, the intolerance of fats is a considerable difficulty.

Local Lesions.—Next comes the question of local lesions. Those of the nose and throat are well-known as causing hay-fever and asthma, and catarrhal conditions of the lungs and bronchi may be the cause of asthma in an exudative individual.

I wish however to point out the frequency with which intrathoracic glands and local lesions in the chest act as determining factors. How they act it is not very easy to say, but I do not think that their action is entirely mechanical. There may be a sensitization to bacterial products and these products may arise in the local lesion and therefore be autogenous. As with cyclical vomiting I have noticed that the culmination and subsidence of the frequency and severity of asthmatic attacks coincide with the conditions of the local chest lesion.

Actual Lesions in Cases of Asthma.—The following figures show you that tuberculosis of the chest is a common accompaniment of asthmatic attacks in children. Out of sixty-eight cases, of which fifty-five were estimated by clinical and X-ray examination and thirteen by clinical examination without X-rays, fifty (73 per cent.) showed tuberculosis of the chest. In sixteen cases (23 per cent.) there was no demonstrable tuberculosis and two were doubtful.

The lesions were: bronchial glands, thirty-one; mediastinal, six; cervical glands, five; fibrosis of roots of lungs, eighteen; adhesions, five; chronic bronchitis, eleven. Thus the common lesions are those of the bronchial glands or fibrosis of the roots of the lungs or chronic bronchitis. [Slides of radiograms of the chest in cases of asthma were shown demonstrating this point.]

Another case which may be cited in illustration is that of a boy who was under my care at Pendlebury Children's Hospital. This boy suddenly coughed up some quite large pieces of débris from a calcareous gland. Luckily the boy did not choke, and as a result of the masses coming away he was much better, and his asthmatic attacks subsided.

Dr. Adams and myself have noticed much confusion of thought between

the exudative diathesis and spasmophilia. I should like therefore to contrast the two.

The exudative diathesis is constitutional, and is an inherited state lasting through life but subject to variations. These variations depend on factors such as climate, sensibility to foreign irritants, local lesions in the body or metabolic shock produced by excess of food, exercise or emotion. Spasmophilia, on the other hand, is a temporary state passing off as a rule in the course of a few weeks, having a seasonal incidence and probably being due to the absorption of a toxin from the bowel. Sometimes epidemics of tetany occur.

In the exudative diathesis there is a tendency to catarrhal manifestations, often of sudden onset, in one or other part of the body. These manifestations may be interchangeable, and probably their site is determined by the presence of some local lesion or by the readiness of access to foreign irritants. In spasmophilia there is a temporary state of increased nerve excitability, giving rise to the tendency to spasm in various parts of the body. The condition lasts for weeks or months.

The manifestations of the exudative diathesis have already been detailed; those of spasmophilia are laryngismus, tetany, convulsions and nerve phenomena, including Chvostek's and Trousseau's signs and increased reaction to electrical stimulation. Dr. Adams will describe in detail the calcium tests.

II.—Dr. Adams.

Spasmophilia has been said by many authorities to be associated with the exudative diathesis, and I have tried to find out whether there is any such real association. I have on several occasions observed Chvostek's sign in children with an asthmatic family history, for example in twin boys who had asthma at the age of 3 months. At the age of 8 months, when dentition began, they showed Chvostek's sign, which passed away in three weeks; and again at 20 months, when they had an attack of diarrhœa. In another case with a similar family history the child caught influenza at the age of 8 months when she was cutting her third tooth. After four days she went into a condition of carpopedal spasm, and Chvostek's sign was present. In thirty-six hours the carpopedal spasm had disappeared, but Chvostek's sign persisted for two days longer.

The electrical reactions of a number of children with asthma and a number with spasmophilia were tested for Erb's sign of increased irritability of the peripheral motor nerves. Of twenty cases of uncomplicated asthma which had been tested, fifteen had normal electrical reactions, and only one showed the complete Erb's reaction. Five cases of uncomplicated spasmophilia all had abnormal reactions and showed Erb's sign. Four exudatives with Chvostek's sign were tested: two asthmatics, one eczema, and one urticaria. All gave Erb's sign.

Raudnitz found Chvostek's sign so common that he said it could not called a sign of disease, but out of a large number of young children tested at the Manchester Children's Hospital, Chvostek's sign was found in only a few, and these showed Erb's sign as well.

Many writers consider that in asthma there is a condition of vagotonia, and that the part of the vagus supplying the respiratory tract is abnormally sensitive to small stimuli. From my experiments it is evident that in the majority of asthmatics the peripheral motor nerves do not share in the increased excitability. The hyperexcitability of the motor nerves in spasmophilia is not due to the same cause as the vagotonia in asthma.

It has been shown by Howland and Marriott that in spasmophilia the blood-calcium is much diminished, and that it might be as low as 50 per cent. of the normal amount. A number of cases of asthma have been tested, a few of spasmophilia, and a number of children suffering from other diseases. All the cases of spasmophilia showed a marked calcium deficiency. Twelve cases of asthma were tested, and seven showed a calcium deficiency, not so great as in spasmophilia. Four tuberculous children who were not suffering from asthma showed no calcium deficiency.

I do not conclude from these experiments that calcium deficiency is a factor in the production of asthma, but that children with the exudative diathesis are liable to faulty calcium metabolism, and may in consequence develop spasmophilia. Exudative infants are not easy to feed artificially on account of their liability to digestive disturbances, and their intolerance of fat which hinders the intake of the fat-soluble vitamin. Fat-dyspepsia would soon cause calcium deficiency, because the fat curds in the stools consist for the most part of calcium soaps, and calcium cannot be assimilated in such cases although abundantly present in the food.

Observations on Cases of Epidemic Encephalitis in Children, seen in the recent outbreak.

By J. C. Spence, M.C., M.D.

[ABSTRACT.]

In this paper, which is being published in full elsewhere, Dr. Spence drew attention to the extreme variation in symptoms and signs which cases might show in any epidemic. He discussed the signs which were present when the cord was chiefly affected. In a certain number of these cases in which pain was an early and prominent feature, diagnosis was often a matter of difficulty, and seven of his cases had been admitted to hospital as acute abdominal emergencies. He showed slides illustrating the typical pathological changes affecting various levels of the cord.

Case of Congenital Obliteration of Bile Ducts.

By Hubert Armstrong, M.D.

Male, aged 5 months. Eighth child. No history or appearance of syphilis, and no improvement under anti-syphilitic treatment. Jaundice began when child was three days old. Meconium—according to mother's statement—and subsequent motions all colourless: urine contains bile pigment. Skin and conjunctiva jaundiced, child marasmic and slowly failing. Liver $1\frac{1}{2}$ in. below costal margin; gall-bladder and spleen not palpable.

In reply to questions Dr. Armstrong said he had not administered bile-salts; nor had he contemplated cholecystenterostomy as he had no proof of the presence of the gall-bladder. There had not, as yet, been any hæmorrhages.

Case of Plagiocephaly.

By Hubert Armstrong, M.D.

Male, aged 7 months, nursed at mother's left breast only as the right breast was not secreting. The calvarium, especially in its posterior half, bulges on the right side, producing the usual deformity. This obliquity appears to be the

result of the action of gravity, uncompensated by alternate feeding on either arm.[1] When mentioned at all it is generally dismissed in a few words by text-books, and it is frequently unrecognized.

DISCUSSION.

Dr. ERIC PRITCHARD said that the patient had also a deformity of the chest which he (the speaker) had frequently found in association with the cranial asymmetry.

Dr. NORMAN B. CAPON said that although rupture of the tentorium cerebelli and cranial distortion were frequently associated, it did not seem permissible to ascribe the cranial asymmetry in Dr. Armstrong's case to birth-injury. Post-mortem evidence had shown that when an infant had recovered from the immediate effects of birth-trauma, dural lacerations had healed satisfactorily.

Dr. ARMSTRONG (in reply) said that he had not noticed the chest deformity, but would look out for it in future. The obliquity was far too frequently found in association with unilateral breast feeding for it to be merely coincidence.

Case of Multiple Exostoses.

By HUBERT ARMSTRONG, M.D.

With Radiographs by C. THURSTAN HOLLAND, Ch.M.

S. B., MALE, aged 11¼ years. Well grown but spare. Lower end of sternum and xiphisternum conspicuously depressed, forming a deep pit, 5 in. in diameter.

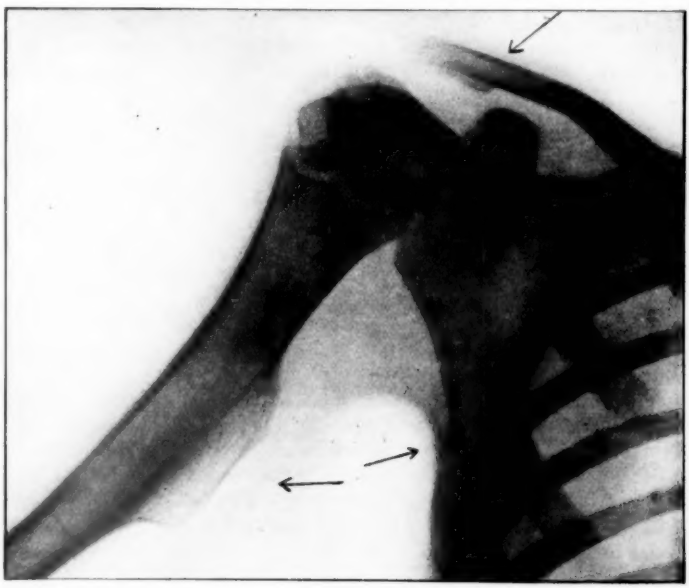

FIG. 1.

This developed during the second half of his first year. He is said to have had chronic bronchitis since he was aged 6, measles at the age of 4 and an operation

[1] "Cranial Asymmetry due to Postural Causes," Hubert Armstrong, *Lancet*, March 4, 1911.

for left inguinal hernia at the age of 6 months. The bony nodosities have been present and slowly progressive since infancy. The largest is a thick bony ridge on the inner surface of the right humerus (lower half of upper third) (fig. 1). Others, irregularly circular, are prominent on the lateral aspects of the femoral condyles, and on the heads of the tibiæ (fig. 2). Intermediate in size are three on the infraspinous surface of the left scapula, three on the spine of the right

FIG. 2.

scapula, and one on the head of each fibula. Others varying in size from a millet-seed to a pea are seen or felt on the clavicle, sternum, ribs and long bones additional to those enumerated. They are painless and cause the patient no inconvenience. Patient was admitted to a medical ward for observation, with a history involving the possibility of latent tuberculosis, but that diagnosis has been negatived.

Section for the Study of Disease in Children.[1]

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

The Treatment of Pyloric Stenosis of Infants.

By Leonard G. Parsons, M.D. (Birmingham).

When Dr. Dingwall Fordyce asked me if I would speak on this subject at this Meeting I imagine he thought that there would be some divergence of opinion on the subject of the treatment of infantile pyloric stenosis and that this paper might serve to provoke a discussion. In the remarks I have to make I have kept that idea in view, for I have little to add to my papers already published on this subject, except to say that I am more convinced than ever that the Rammstedt operation should form part of the treatment of every case of pyloric stenosis.

A recent paper on the treatment of pyloric stenosis by Dr. Leonard Findlay[2] has as its sub-title "Medicine v. Surgery," which I think is a wrong conception of the question at issue, because medicine plays a great, probably as great a part as surgery, in the successful issue of a case of pyloric stenosis in which the Rammstedt operation forms a part of the treatment.

Medical Treatment.

The medical treatment of pyloric stenosis, whether by dieting alone or by dieting and lavage, is too well known for me to discuss it in detail. I would, however, combat Findlay's opinion that it is wrong to give small feeds, although I agree with his observation that many cases when fed vomit the feed, but that if another feed is given immediately after the child has vomited, the second feed is kept down.

There are two other methods of medical treatment which should however be mentioned: (1) The administration of atropine and (2) thick cereal feeding. I have not had any personal experience of these methods in pyloric stenosis, although I have made use of them, sometimes successfully, in the treatment of pyloro-spasm. I have, however, seen cases of pyloric stenosis treated with atropine by some of my colleagues and I have not been impressed by the results.

(1) *The Atropine Treatment* has been largely emphasized by Haas in America, but the most recent paper on the subject is, I think, one by Johannesen of

[1] Provincial Meeting of the Section, held at Liverpool, June 27 and 28, 1924. Continued from *Proceedings* 1924, vol. xviii (Sect. Study Dis. Children), p. 28.

[2] *Brit. Journ. Child. Dis.*, 1923, xx, p. 1.

Copenhagen. This paper is based on seven cases only, and as a result of his experiences of the method in these cases the writer says that this form of treatment is capable of producing recovery in every case of pyloric stenosis. The usual method of giving atropine sulphate is to begin with a dose of one-thousandth of a grain and to give this fifteen to thirty minutes before a meal. The dose can be increased to two-thousandths of a grain or even more. Johannesen says that it is ten to fourteen days before the full effect of the drug is manifest and that the drug should be continued for nine to ten weeks after vomiting has ceased. Flushing is usually stated to be the sign of over-dosage by atropine, but I have seen distension of the abdomen so marked that the drug had to be stopped without any evidence of flushing and with a dose of one one-thousandth of a grain only. The atropine solution (half a grain to the ounce of distilled water) should be a fresh solution and should be made up every two or three days.

(2) *Treatment by Thick Cereal Feeding.*—This method has been largely advocated by Sauer and he prefers it to the Rammstedt operation. The food (farinaceous) is made so thick that it has to be placed on the back of the child's tongue by means of a spatula or spoon. It is a little difficult to understand the rationale of this method of treatment, since at first sight it would appear to be more difficult for food of this nature to pass through the pylorus than ordinary milk clot. The following is, however, a possible explanation. In adults the stomach is emptied by a combination of two processes, peristalsis and what has been called by Stiller the peristolic function, i.e., the tonic contraction of the stomach around its contents. The infant's stomach does not possess a peristolic function, but this function may be evoked if milk is replaced by food of more solid consistency; under these conditions a powerful tonic contraction around its contents occurs. It may be that the development of this peristolic function by giving thick feeds may be the reason for the good results recorded by observers who have used this method of treatment. The same explanation would also account for the success of thick feeding in severe cases of pyloric spasm, of which I have had personal experience. The undoubted recoveries that have occurred in cases of pyloric spasm under this method raises the old question as to whether many of the cases in which cure has been claimed for medical treatment of pyloric stenosis have not been cases of pylorospasm. Haas indeed regards pylorospasm and pyloric stenosis as differing manifestations of the same disease. I know that it is possible to make a mistake in diagnosis between the two conditions unless a pyloric tumour is felt or efficient X-ray examination is carried out, because I have had laparotomy done in two cases for pyloric stenosis in which the operation revealed a normal pylorus.

What are the results of medical treatment?

(1) *Recovery.*—This undoubtedly can occur, but if it does: (*a*) The treatment has to be a very prolonged one and (*b*) the result is in doubt until the child is about six months old, great anxiety on the part of the mother thus being engendered, so that if she is breast feeding there is the possibility of the loss of breast milk, which diminishes the chance of recovery.

(2) *Failure.*—With most observers this has been the result in the majority of their cases; sudden fatal collapse, or diarrhœa and death, may occur at any time during the course of treatment and even when the baby appears to have been progressing favourably.

Operative Treatment.

Various operative procedures have been practised for the relief of pyloric stenosis, but only two, I think, are now made use of to any extent, e.g., the operations of Loreta and Rammstedt. These operations are to be preferred to a short circuiting operation like gastro-enterostomy, because the food continues to pass by the normal

channels and the physiology of digestion is less liable to be altered. Gastro-enterostomy is also a more lengthy proceeding and the resulting shock is more marked.

The Loreta operation is particularly associated with the name of Burghard in this country. Mr. Burghard has had a highly successful series of results, particularly in private patients. During the last eight years he has operated on forty-six of Dr. Still's private cases with only one death. He is, I believe, the only surgeon who has had really good results from this operation—as Still says, "in the hands of some surgeons it has had very little success." One advantage the Rammstedt operation possesses over the Loreta operation is that it gives uniformly better results in the hands of all the surgeons who make use of it. The best results reported in Britain are those of Poynton, Higgins and Byrdson, who had 73 per cent. of recoveries in fifty-seven cases.

The Loreta and the Rammstedt operations both aim at the same thing, i.e., the division of the muscular coat so that the imprisoned mucous membrane may bulge or herniate and thus produce or increase the lumen of the pyloric canal.

My experience of operative treatment has been almost entirely confined to the Rammstedt operation. Gastro-enterostomy was done in eight of my cases with two recoveries. I have had but one experience of the Loreta operation, and in that case the patient died.

I believe that the Rammstedt operation should be performed in every case of pyloric stenosis. I do not propose to go into details about the operation or the pre- and post-operative treatment, since these have already been given, but I would like once more to stress the importance of a few post-operative phenomena, and to draw attention to two other points. First, that the operation will fail if the division of fibres is not absolutely complete, even the merest shred of circular muscular tissue being sufficient to prevent the bulging of the mucous membrane, and, secondly, the importance of giving, after the operation, small feeds at first and cautiously increasing them. If this method of feeding be followed, I feel convinced that the liability to post-operative diarrhœa—frequently a fatal complication—is considerably diminished. By feeding in this manner large increases in weight are rarely seen, and in my opinion there is not any cause for dissatisfaction if the child's weight remains stationary after operation for a fortnight, or even longer, since weight is rapidly put on after four to five weeks, when the necessity for giving small feeds has passed away.

Diarrhœa occurs in cases treated medically or surgically. Like all atrophic children these infants are liable to attacks of diarrhœa, although the liability to such attacks is even greater in pyloric stenosis than in marasmus. I have given reasons elsewhere for thinking that the diarrhœa is largely due to the inability of the child's intestine to absorb the large amounts of food that suddenly pass through the pylorus after operation. Small amounts of food, such as the intestine has previously been dealing with, can be managed satisfactorily, but large amounts will produce diarrhœa. In other words, the intestine must pass through a period of training before it can absorb large amounts of food.

Vomiting usually ceases after the operation, but occasional vomits may occur for three or four up to fourteen days. As a rule, these are small in amount, and if they are at all troublesome, a wash-out with bicarbonate of soda (1 per cent.) usually completely stops them. They are probably due to the gastritis which practically always accompanies pyloric stenosis, and the liability to these occurrences is rendered less by washing out the stomach for one or two days before operation, and the use of gas and oxygen as an anæsthetic.

Atrophy occasionally occurs after operation, and the case may remain an atrophic one for weeks before the child puts on weight satisfactorily. This is a small group, but it is important to recognize its existence.

Post-operative pyrexia is not uncommon, and, as is well-known, hyperpyrexia

occurs. The liability to hyperpyrexia is considerably diminished by the use of gas and oxygen as an anæsthetic, but the most important method of preventing the occurrence of pyrexia is the administration of continued rectal salines for twenty-four to forty-eight hours after operation, and the avoidance of the use of hypertonic solutions if a subcutaneus saline is given before the operation. The probable cause of the pyrexia is dehydration of the child (inanition fever) due to the disease, its further dehydration by the use of hypertonic salines (cf. the work of Sansum, Balcar and Woodyatt), and a certain degree of dehydration as a result of the operation itself.

Comparison of Results of Non-operative with Operative Methods of Treatment.

	Hospital cases			Private cases			Total		
	Total	Cured	% Cures	Total	Cured	% Cures	Total	Cured	% Cures
Treated medically ...	37	5	13·5	4	0	0	41	5	12·1
Rammstedt's operation	59	27	44	11	11	100	70	38	54·2

In submitting these results I would suggest that they are truly comparable for the following reasons:—

(1) They are cases coming under the observation of the same observer, and therefore it is easier to determine the cause of any improvement in treatment.

(2) Roughly speaking the cases occur in two groups. At the time when medical treatment was being carried out practically every case was treated medically, and from the time that Rammstedt's operation was introduced every case, without any selection, has been operated upon. The only cases treated medically in the latter group have been those in which operation was refused. Better figures for the Rammstedt operation could be obtained by selecting the cases, but as a guide as to which is the better method of treatment every case, even the moribund cases, must be treated in the same way. It may be added that the recovery rate for eighteen cases operated upon in the year 1923 was 70·3 per cent.

(3) I have separated hospital and private patients in each category, although the separation as far as the Rammstedt operation results are concerned, with the exception of one case, means a separation into cases treated in the public and the private wards of the same hospital.

Many cases that swell the mortality returns of cases treated medically should really be deleted from consideration, or classified under the heading of death from lack of treatment. Several of this type of case have, however, recovered after the Rammstedt operation. I think that the fact that private cases submitted to the Rammstedt operation do so much better than hospital cases is due to the earlier diagnosis in the former group, and I also believe that the sooner operation is done the better the chance of recovery. The operation should be done in every case; it is true that slight degrees of pyloric stenosis do occur, and may recover under medical treatment, but it is probably also true that the slighter cases may become more severe as the patients grow older, and may even develop into the severest type, a type which everyone admits is suitable for the Rammstedt operation. It is impossible to say, in a mild case, whether it will remain a mild case or develop into one of the most severe forms; and if operative treatment is postponed until the severe symptoms develop much valuable time is lost and the chances of recovery after the operation are prejudiced.

I cannot conclude this short review without recording my thanks to my surgical colleagues, particularly Mr. Seymour Barling, for valuable help in the treatment of these cases.

DISCUSSION.

Dr. J. C. SPENCE (Newcastle) said that Dr. Parsons' advocacy of the need of early diagnosis in cases of congenital pyloric stenosis, and of their prompt treatment by Rammstedt's operation was one with which he entirely agreed. Recent figures of the result of that method of treatment from Great Ormond Street Hospital and other hospitals showed that so rapid an improvement was taking place that shortly we could look forward to results in which the operative mortality would be no higher than 5 to 10 per cent. Quoting a case he had recently seen, he asked whether the operation could not be successfully applied to cases of pylorospasm as well. He knew that it was possible to treat certain cases medically with success. But it was a trying and difficult process. And the choice appeared to him to be between a few days' extreme anxiety of operative treatment and several months' long drawn-out anxiety of medical treatment.

Dr. LAPAGE (Manchester) said that with regard to the treatment of cases of pyloric stenosis, he had dealt with more than a hundred cases of this trouble, excluding cases of pyloric spasm; and that as a result he recommended operation in most cases. In fact, in the fulminating cases operation might be almost as urgent as in a case of intussusception. However, he had had experiences which showed that operation was not always indicated. Some cases which were not of the fulminating type came late to the specialist. In other cases consent for operation was refused; in others, factors intervened which made operation impossible.

He quoted the following cases to illustrate the point:—

The first case was one of a child with a non-fulminating degree of pyloric stenosis, brought after the symptoms had been present for five weeks. There was definite peristalsis, a palpable mass, but no vomiting. This absence of vomiting was an important point. The case was not one of pyloric spasm, it was a true pyloric stenosis, but, if there was no vomiting and one could keep the child nourished, it had to be considered whether an operation was advisable or not. The child was put on belladonna and lavage treatment and its nutrition could just be kept up without the occurrence of vomiting. If a child's nutrition could be kept up without vomiting, and if there was evidence of subsidence of the pyloric trouble, it was questionable whether operation was the best course. However, in this case the trouble was prolonged and, as there was no evidence of subsidence, operation was decided upon. But the child developed a severe attack of influenza, followed first by a pneumonia, secondly by an otitis, and lastly by a gastro-intestinal catarrh; the duration of these complications was in all some six or seven weeks. The pyloric condition had, perforce, to remain in the background; and it did so, the patient making a complete recovery.

In other cases the parents had flatly refused operation and the trouble had subsided slowly. It was important to note, however, that in all these cases vomiting had been absent or only occasional, though food enough to keep up nutrition had been given.

With regard to the conditions of the pylorus in cases in which the children had survived without operation, radiograms, showing the emptying of the stomach in a child of 8 years who had had a definite history of pyloric stenosis as a baby, were exhibited. The X-ray showed that the stomach now emptied quickly enough to be regarded as normal. The child had been brought for consideration of the question of appendix removal.

The following case of a baby cured by lavage before the days of Rammstedt's operation, and when the mortality from gastro-enterostomy was very high, showed that the tumour

might remain even though a normal diet was being given. This child, at the age of 3 years, had appeared to be normal in every way and was taking an ordinary diet. It contracted pneumonia and died. At the autopsy the pylorus was seen to be still very much thickened and the stomach moderately increased in size. This case was important as throwing some light on the condition of the pylorus in later years in these cases. His feeling was that this child would have been the better for operation; but the case had occurred in the early days of the study of pyloric stenosis work and operation was then a very serious consideration.

With regard to the atropine treatment, he had tried belladonna, but only in moderate doses, namely, one minim with each feed; and if the feeding was carried out every two hours or at shorter intervals it amounted to a considerable dose. He (Dr. Lapage) found that it had a very definite effect, and for cases of pyloric spasm it was of great value.

Section for the Study of Disease in Children.

President—Dr. H. C. CAMERON.

Case of Congenital Hypertrophy of Lower Limbs.

By DENIS BROWNE, M.B., F.R.C.S. (introduced by T. TWISTINGTON HIGGINS, O.B.E., F.R.C.S.).

A. J. M., MALE, aged 2½ years.

Brought to hospital because his mother could not get any shoes to fit him.

At birth feet and legs were noticed by the doctor to be unusually large, but otherwise the child was normal. He has always been exceptionally strong and healthy, and he learnt to walk when one year old.

The child is normal in every way except for the formation of both lower limbs. The thighs and calves are extremely thick, hard, and muscular, while the feet are very large and square, with wide spaces between the toes. When he stands the big toe turns inwards and grips the ground, suggesting a likeness to toes of anthropoid apes. The muscular strength of the limbs is in proportion to their size: the boy can stand up with ease from a sitting position unaided by his hands, which at 2½ years is distinctly abnormal.

The muscular reactions, knee-jerks, and plantar response are normal. X-rays show normal development of epiphyses and ossification centres, but with the metatarsals thicker and more widely spaced than in the normal condition.

At first glance the large calves might suggest pseudo-hypertrophic muscular atrophy, but the condition of the muscles is entirely different. A somewhat similar appearance may also be produced by angiomata of one or more limbs, but there is no sign here of any such condition.

There seems no indication for treatment other than getting shoes made to order, but it will be interesting to see how the child develops, and whether the ratio between his legs and the rest of his body is maintained.

DISCUSSION.

Dr. R. C. JEWESBURY reminded members that he exhibited a somewhat similar case, in a boy, now aged 9 years.[1] His muscular hypertrophy was more general than in the case now shown. The boy referred to stripped like a boxing man, there was tremendous development of the deltoid and arm muscles, and the muscles of the thighs and legs were also overdeveloped, but to a less extent. The strength was in proportion to the development. Electrical reactions in all muscles were normal. Mentally he was backward, but not very much so. He had a curious facial spasm, which became so marked that, especially in times of excitement, he had a difficulty in opening his mouth or speaking. The mother could not be certain at what date the excess of muscular development began; he was first brought for advice because of the facial spasm.

[1] *Proceedings*, 1922, xv (Sect. Study Dis. Child.), p. 34.

Dr. B. MYERS said the present case appeared to be on all fours with one which he had seen eighteen months ago and which ended fatally. The patient came into hospital with another complaint, and was found to be affected with hemi-hypertrophy of the right leg, the left leg being normal. There was one and a half inches increase in the circumference of the thigh. The child caught an infectious disease and died at a fever hospital. Dr. J. D. Rolleston kindly asked him to be present at the necropsy. The right leg was seen to be three-quarters of an inch longer than its fellow. It was decided to open the two limbs in identical positions and compare bones, muscles, &c., of the two sides. The two femora were of nearly the same thickness, the right being just a little thicker. Comparing muscle with muscle, there was no very appreciable difference. The increase in the circumference of the right thigh was found to be due almost entirely to excess of fatty tissue containing a distinct amount of fibrous tissue. Why had that child one limb longer than the other? Under one foot there was a very large pad of fat, which accounted for practically all the difference.

Dr. H. C. CAMERON (President) said that the cases which had been described by Dr. Jewesbury and Dr. Myers were interesting, but differed obviously from that now under consideration. Cases such as Dr. Myers described, in which the hemi-hypertrophy involved only the subcutaneous tissue, were sharply to be distinguished from the cases in which the bones shared in the hypertrophy. But in no form of hemi-hypertrophy did there seem to be the element of deformity seen in the present instance. He had seen bilateral malformation of the feet of this character but not associated with muscular hypertrophy. Had Mr. Browne noticed any diminution of the deformity? Though cases of hemi-hypertrophy were seen, often of slight degree, in children, there were no cases among the males who came to the recruiting stations during the war. In Mr. Lockhart-Mummery's interesting case,[1] careful measurement at different periods showed the smaller limb gradually catching up with its fellow, until by adult age there was only a minimal difference.

Dr. EDMUND CAUTLEY said that the condition of this patient was very different from hemi-hypertrophy; it was apparently a pure muscular condition. He suggested it would be well to have a portion of the muscle excised and examined.

Mr. DENIS BROWNE (in reply) said that there was undoubted bony deformity; it was not a pure muscular case. Skiagrams showed that though the ossification of the bones was normal, they were much more widely spaced in the foot than they should be. He agreed it would be valuable to have an excised piece of muscle examined; but if the increased size were due to an admixture of fibrous tissue, one would not expect the extra strength which the patient showed.

A Case of Pink Disease.

By RUTH DARBY, M.B.

A GIRL, 1½ years old, the only child of young parents. When brought to hospital, the parents said she had been increasingly irritable for three months, and during the later part of that time had refused all solid food, and even liquid food was only given with difficulty. She lay in bed with her head curled round and burrowed under the pillow, and showed a great aversion to light. Her hands and feet were very œdematous and red, and there were two or three blisters on each foot. There was some peeling of the palms. The knee-jerks were quite absent, and I think they are still absent. She has twelve teeth, but all are very loose, and the gums are very red and œdematous, but no hæmorrhage is evident. There seems to have been nothing about the diet to suggest scurvy. In the urine there was only a trace of albumin. The blood showed slight leucocytosis; there were 13,000 white cells, of which 60 per cent. were polymorphs and 32 per cent. lymphocytes. No abnormal cells were seen. At an earlier stage there was apparently complete anæsthesia of the legs and a partial anæsthesia of the arms. Since admission her temperature has not risen above 99·4° F. Pulse and respiration normal.

[1] *Proceedings*, 1923-24, xvii (Sect. Study Dis. Child.), p. 1.

DISCUSSION.

Dr. DONALD PATERSON said he thought Dr. Darby was performing a useful service in showing this case, as many people believed that "pink disease" was not a disease. The clinical picture was typical, and was recognized as such by those who had seen cases. In two cases he had recently seen, the history and the clinical state were identical with this case. As a rule, the cause of their being brought for advice was either anorexia or insomnia, because these symptoms worried the parents most. The ages of the patients ranged from 6 months to 2 years. As this was the period of teething, a common diagnosis was dentition. But when loss of weight continued for a fortnight, it was seen that this diagnosis would not hold, and the practitioner was in doubt. When the mother brought the patient she was usually worn out with the care of the child. The rash was transient; but if seen during an acute exacerbation it was typical. The child might be brought when the rash was fading, but there was the well-marked history. In bed the child turned on its face, rubbing its face backwards and forwards in the bed-clothes, and rubbing its hands together, and he knew of no other condition except mental deficiency with a tic or habit, which produced these movements. The marked hypertonia was unlike any other condition he had seen in children. Perspiration was continuous, and the child's hands were sodden; the teeth were loose. In a number of cases of the condition under the care of Dr. Parsons (of Birmingham), one of the striking features was the occurrence of teeth in the stools. The hair came out in large clumps, and in two cases which Dr. Thursfield and he had shown there was a bald patch on one side of the head, where the patients had pulled at their hair. In an older child with the condition there was anæsthesia of hands and feet. In one case there was a slight squint, but as a rule there was none. When a child with pink disease was seen the temperature might be normal. Dr. Darby's case and another case were admitted on the same day, and they had a different ending. One of the children was attacked with a sudden broncho-pneumonia and died in twenty-four hours. The other child had been shown to-day. The outlook in regard to the pink disease was, as a rule, excellent; but he had seen death occur in four cases: one of miliary tuberculosis, one of intussusception, another of intercurrent diarrhœa, and the fourth of pneumonia. The pathology, which had been worked out by Dr. Greenfield, of the National Hospital, showed typical peripheral neuritis. He (the speaker) saw ten or twelve cases of this disease a year, and it was important that the profession should recognize it as a definite entity.

Dr. F. PARKES WEBER asked whether there had been any disease of the nose or naso-pharynx in this case. A. H. Byfield (*American Journal of Medical Science*, Philadelphia, 1920, xx, pp. 347-365) reported nasal complications in some cases. In one case (Case 8) the tonsils and adenoids were removed and the nasal sinuses drained.

Dr. HUGH THURSFIELD said that Dr. Paterson was largely responsible for the proof of the hypothesis which he (the speaker) had advanced, on clinical grounds, that this was a polyneuritis. Dr. Paterson secured the nerves from one of the fatal cases, and with Dr. Greenfield proved the existence of neuritis. The first case of the kind he had seen came to him as a surprise. It was the chance observation by an Australian who was going round the hospital with Dr. Paterson and himself, and had read Dr. Byfield's paper, which led to a recognition of the nature of the case. Since that date they had recognized that it was a fairly common disease. He did not think there was good evidence as to the origin of the infection, and he had not convinced himself that there was anything in the nose-and-throat theory. He did not think it was contagious, and he had not heard of two cases having occurred in one family. It was distressing to parents, and in a hospital ward these patients kept the other children awake. The prognosis was good; that was the experience in Australia, the country in which it was first recognized. Dr. Byfield called it acrodynia, and Dr. Paterson and himself, in their first enthusiasm, called it dermato-polyneuritis, and another name given to it was erythrœdema. There was a case worth noting in Baltimore, that of a boy aged 12 years, which was published by Dr. Emerson. That boy had had the disease more than two years before his recovery from it.

Dr. DONALD PATERSON said the treatment was summed up in attention to the anorexia and the insomnia. It was necessary to get the child to sleep in order to keep it alive. Hence hospital treatment was valuable, as there were relays of nurses. After sleeping, the patient took food, and then the symptoms began to abate. The skin irritation was increased

by the child scratching itself, but prevention of the scratching would induce sleep. Chloral and bromide had but little effect. He had not known of more than one case occurring in a family, and he did not think nasal symptoms were of any importance as affecting the condition.

A Case of Cœliac Disease with Glycosuria.

By F. J. Poynton, M.D.

Frederick W., aged 7 years. This patient was brought to the hospital on account of extreme wasting and distension of the abdomen.

Since he was a baby he has always passed pale, bulky stools, and has had a prominent abdomen. Except for this, at 6 years old he was a healthy plump little

Cœliac disease with glycosuria.

boy. At this age he had an attack of severe diarrhœa and passed a number of pale fluid and foul-smelling stools.

Soon after this attack he began to lose weight rapidly, and at the age of 7 was emaciated. He weighed 24 lb. and had a very prominent abdomen (19 in. in circumference). His urine was found to contain much sugar.

On admission to hospital, August 2, 1924, patient was passing 6 per cent. of sugar in his urine. Blood sugar, 0·44 per cent. Stools contained 48·2 per cent. of fat—74 per cent. of which was split fat and 26 per cent. neutral fat. No acetone in the urine.

He was put on a diet high in protein content and low in carbohydrate and fat, and he then became sugar-free, but did not put on weight. He was therefore given insulin and a higher though still reduced percentage of carbohydrate and fat. His weight increased steadily (3 lb. in three months), and with occasional exceptions he remained sugar-free. He was brighter, happier, and better in every way.

The abdomen was still distended however, and the stools remained pale and bulky.

My thanks are due to Dr. G. A. Harrison, who carried out the biochemical examinations in this case.

DISCUSSION.

Dr. EDMUND CAUTLEY said he felt doubtful whether the case should be regarded as cœliac disease; it was more of the type of pancreatic inadequacy, or what had been called pancreatic infantilism. The history accorded with his view. In cases of cœliac disease no definite disorder of the pancreas nor of any other organ had been found post mortem.

Dr. H. C. CAMERON (President) asked whether there had been any definite following up of hospital cases of cœliac disease, and if so, what was known of the future of these children Had other cases developed glycosuria later on?

Dr. THURSFIELD said he agreed with Dr. Cautley's remarks. Some years ago Dr. Hutchison, with Dr. Panton, examined many of these cases by every chemical method then known, and was unable to find any evidence of pancreatic insufficiency. The same negative result had been arrived at by other workers. In the present case he thought there was much evidence, both on the clinical and the laboratory sides, that there was disease of the pancreas. Therefore it seemed to fit in very well with Byrom Bramwell's case and another reported shortly afterwards by Dr. Rennel. If a proper pancreatic extract were available he did not see why the same result as Dr. Bramwell had obtained should not be secured in this case.

He was not aware of any systematic following-up of cases of cœliac disease. These patients were apt to die of some intercurrent disease, but if they survived they seemed to acquire later a considerable degree of ability to absorb fats. At Great Ormond Street the belief was that these patients failed to absorb their fats. Many years ago he saw a boy—now a Cambridge undergraduate—who, until his seventh year, had to be kept on a rigid diet, but from that age he gradually became able to absorb and use the fats eaten, and at 20 he was a reasonably robust person, though he still had a distaste for any quantity of fat. If these patients passed the critical age of 5 to 7, they generally did well.

Dr. DONALD PATERSON said that he saw this patient first at the out-patients' department at Great Ormond Street, and had sent him into hospital. He then diagnosed his case as one of cœliac disease. A specimen of the urine was not obtained. There had been large "porridgy" stools from an early age, and the child remained infantile. Therefore, clinically, it seemed to be a case of cœliac disease. The liver was small, and the abdomen large. On the next visit there was sugar in the urine, and glycosuria was found during acute exacerbations of diarrhœa in cœliac disease. It was interesting to connect cœliac disease with diabetes. There must be some cause for the inability of the child to absorb its fat. Dr. Spence and he had examined a number of cases in which there had been glycosuria during their acute upsets, but blood sugar examinations were not done extensively then.

Dr. VINCENT COATES, referring to glycosuria in pancreatic disease with infantilism and cœliac disease, drew attention to a recent article by Cecil Clarke and Hadfield in the *Quarterly Journal of Medicine*[1] in which the post-mortem examination on a child who had suffered from pancreatic disease showed more islet tissue than normal. There had been no glycosuria during life. A case recently under his care had been diagnosed as pancreatic infantilism. The patient was a girl of 17½ years, 43½ in. in height, who had bulky fatty stools but no sugar in the urine. She had been considered to be a case of cœliac disease.

Dr. POYNTON (in reply) said that one did not advance the subject much by saying a condition was one of pancreatic infantilism. He had not realized that glycosuria was so frequent in cœliac cases. He had seen post-mortem examinations on cœliac cases, but

[1] *Quart. Journ. Med.*, 1924, xvii, p. 358.

no gross pancreatic change was found; was it believed, nevertheless, that the organ was not damaged? It might be damaged without any gross change in the cells being revealed, especially in such an organ as the pancreas. He did not think that cœliac disease was only a question in which fat was concerned; in bad cases all forms of digestion were more or less defective. The condition could not, in many cases, be organic, because these patients got well. There was no acetone in the urine in the case he was showing that day.

The Subsequent History of a Series of Cases Operated upon for Pyloric Stenosis.

By B. E. Schlesinger, M.B., B.Ch.Camb.

Recently a number of cases operated upon for *pyloric stenosis* by the *Rammstedt* method have returned to Great Ormond Street for inspection.

They constitute most of the cured cases from a continuous series of thirty-seven, all of which were treated in the same ward, under the care of Dr. Thursfield, during the last four years.

Of these thirty-seven, twelve died in hospital (eleven after operation and one after five days' medical treatment only), making an immediate mortality of nearly 32 per cent. In addition, three cases were discharged as cured (two after surgical and one after medical treatment) but succumbed to some intercurrent disease at intervals of one month to one year later. Of the remaining twenty-two cases, four are untraceable, leaving thus eighteen, which have all been examined during the last month.

It is a most striking fact that one and all of these children, after an unhesitating climb up the scale, appear now to have attained an almost abnormal fitness, and from dehydrated miserably wasted infants to have become healthy and vigorous.

The mothers also informed us that their children had reached this pinnacle of well-being without once looking back and that they even seemed to suffer less, if at all, from the usual small gastric upsets of infancy. This fact has frequently been noted by writers on the subject. Dr. Still states that he has seen or heard of many of his old cases and has found them to be particularly fine healthy children. Dr. John Thomson, writing on a series of 100 cases, was able to follow up the histories of thirty-eight. He found that five of these patients had died from intercurrent disease; while of the remaining thirty-three whom he saw again, many were healthy schoolchildren. He also mentioned that the first case to recover under operative treatment, a patient of Dr. Nicholls, had, then, in 1918, just been passed A1 for the Army.

This possible optimistic prognosis once the operation is over, and more important still, once the dangers of gastro-enteritis have been successfully avoided, is fully borne out by the subsequent development of our cases.

The children range from the ages of 5 months to $4\frac{1}{2}$ years. On looking into the question of their present weights it appears that those patients who are still under 1 year of age have not as yet had time to make up for their initial loss, and are under the normal weight, whereas once past the age of about 15 months, they are most of them 1 to 5 lb. heavier than a child normally weighs at the corresponding age. Thirteen of this series have now reached ages between 15 months and $4\frac{1}{2}$ years, *and most of these are over weight*. Their height is similarly affected.

On measuring the height of seven of these patients, it was found that in every case but one they were 1 in. to 4 in. taller than the average child of that age. The cause of this development above par has been explained on the assumption that the diet is observed with special care after the operation; yet a mother can do no more than breast-feed her child in the normal way, and in all of these cases this was the food until the child was weaned. Besides, it is difficult to conjecture how 4 in. can be added to a child's stature, however carefully one may feed it.

On inquiring into the diet up to the age of 1 year, it was found, as above stated, that eleven were breast-fed, two were fed on cow's milk, and four on a dried milk preparation. The mode of feeding did not seem to have any direct bearing on the development of the child and they seemed to thrive equally well on any of the three methods, in fact two of the three children who were not quite up to the above mentioned high standard were fed on the breast.

In other developmental details they were the same as other children, cutting their first teeth at the average age of 7 months and walking when they were about 13 months old. There was no complaint with regard to the digestion; the stools were all said to be quite normal, while there was no tendency whatever to a recurrence of the vomiting.

Perhaps the most curious feature found on examination was a pyloric tumour, still palpable in four cases at the ages of 6 months, 3 years, 3 years, and 3½ years. The fact that the pylorus was still thickened did not give rise to any untoward symptoms and these children seemed to progress in line with the others, in whom no trace of a tumour could be made out. As further evidence of the slow subsidence of the tumour, a case may be quoted in which death occurred from tubercular meningitis a year after operation. At autopsy the pylorus was found still to be thickened and adherent to the under surface of the liver. The old scar on the pylorus, though still visible, was becoming filled in with what appeared microscopically to be partly fibrous and partly muscular tissue. So much for the remote progress.

The immediate operative mortality is still very high. It was mentioned above, that of thirty-five cases upon whom an operation was performed, eleven died shortly afterwards, making thus a mortality of nearly 32 per cent., in other words—one child died for every two operated on and cured.

Mr. Tyrrell Gray, in his paper read at the Bradford meeting of the British Medical Association (1921)[1] deals exhaustively with the causes of post-operative deaths and their investigation. He insists emphatically on the absolute necessity of pre-operative gastric lavage and infusion of saline, and his figures bear this out conclusively.

In pyloric cases in which pre-operative lavage was adopted, he had 56 per cent. recoveries, while in those cases in which lavage was omitted the number of recoveries fell to 29 per cent. Further, among the cases infused preceding the operation, the mortality was only 17 per cent., whereas those children who had not received infusion died in 83 per cent. of the cases.

In the whole series under investigation both these precautions were taken.

It is well known that the particular dangers of this operation are in the first instance *shock*, in the second *enteritis*. Five children died from subsequent enteritis, five a few hours after operation from shock and collapse, and one who appeared to be progressing favourably, with no diarrhœa and vomiting, succumbed as late as five days afterwards.

On further investigation at autopsy, additional factors were discovered in a number of these fatal cases. Blood in the peritoneum was found on two occasions, and in four cases it was evident that there had been poor relief, or none, of the pyloric stenosis. These might be classed as possibly controllable factors.

As the surgical technique in the Rammstedt operation has improved year by year and surgical shock has been limited by the adoption of gas and oxygen as the anæsthetic of choice, the mortality has been reduced by more than half. Also the increased care in the preparation of the patients and attention to their diet afterwards has gone a long way in assisting towards an ultimately successful result. This is fully borne out on examination of our series, where we find that the mortality has been reduced from 45 per cent. in 1920 to 13 per cent. in 1923, while the last nine patients to be operated on have all recovered.

[1] *Brit. Med. Journ.*, 1921, ii, p. 891.

Medical as opposed to *surgical* treatment of pyloric stenosis was only adopted in two of the cases from this ward, and the results are by no means impressive. Both children died, one five days after the beginning of treatment, while the other fell an easy victim to bronchitis three weeks after discharge from hospital apparently cured.

A few short notes on the condition of the infants at the time of diagnosis might prove interesting.

Constipation is generally recognized as the rule, although not essential in the diagnosis of pyloric stenosis. In the thirty-seven cases under discussion, this was present in all but two, where there was *diarrhœa*. This last condition should make one carefully reconsider the diagnosis; it was certainly present in three out of the four cases of pyloro-spasm, all of them fatal, which were mistaken for, and operated upon for, a true stenosis when, however, nothing was found.

Vomiting, on the other hand, was, naturally, invariably complained of, and its peculiar projectile character noted by the mother, in all except eight cases.

The usual peristalsis was not noted in six of the children, and the tumour was not palpated in four, but in each of these extraordinary cases either the one or the other was observed.

Sex and Age Incidence.—Thirty-one cases in the series were boys and six were girls, a ratio of five to one. The latest onset of symptoms was at 4 months of age, while in four cases the vomiting began from the time of birth.

As has often been observed, *phimosis* is a phenomenon commonly associated with a *hypertrophied pylorus*, and such writers as Mr. Tyrrell Gray and Mr. Reynolds even go so far as to correlate it as an accessory factor in producing pyloric stricture. A degree of phimosis was found in ten of the thirty-one boys in our series, thus making the percentage incidence as high as thirty-three.

In three cases X-rays were employed as a possible aid to diagnosis, but they were found to be useless if not misleading. Two otherwise obvious cases of pyloric stenosis, which were submitted to X-rays, may be quoted. In one the barium was held up at the lower end of the œsophagus, while in the other the meal was seen to pass the pylorus at once with ease. In addition, in the third case, where the X-ray picture strongly suggested a *stenosis*, the condition at operation proved to be a "mare's nest," and the revised diagnosis of *pylorospasm* had to be made when it was too late.

Finally, on investigating the percentage mortality in relation to the general condition of the patient on admission to hospital, one's natural expectations are realized. Of those classed as in a "good condition," with a small amount of wasting, an elastic skin and fontanelle not depressed, 67 per cent. recovered after operation. In the cases which showed wasting, but as yet no dehydration, those which could be classed as in "fair condition," the recoveries were 60 per cent. But among those which would be termed in "bad condition" the number of recoveries dropped to 37 per cent.

These figures emphasize two of the points in Dr. Poynton's and Mr. Higgins' paper published this year:—

(1) Delay the operation as little as possible.

(2) Regard no patient as too ill for operation.

The strapping, healthy boy of to-day, belying completely his condition of three or four years ago, when he was admitted to hospital a wasted, sunken-eyed and vomiting infant, may truly be regarded as a "brand snatched from the burning."

Summary.

(1) The immediate prognosis should be guarded for at least four days after operation, especially in those patients who are not breast-fed.

(2) The subsequent history of cured cases is entirely satisfactory.

(3) The operation should be performed as soon as the diagnosis is established and the pre-operative preparations are complete.

(4) The diagnosis should not be made unless at least two of three cardinal symptoms are present at the same time, while particular attention should be paid to the condition of the bowels, and to the presence of a tumour.

(5) Little reliance should be placed on X-ray examination.

My thanks are due to Dr. Thursfield, through whose kindness I was enabled to see these recovered cases, and who encouraged me to base this short paper on their results.

DISCUSSION.

Dr. H. C. CAMERON (President) agreed that the rate at which the stomach was emptied was very inconstant, and one must not conclude, because a bismuth meal passed the pylorus, that one was not dealing with pyloric stenosis. He did not like to belittle the importance of X-ray examination in cases of obstructive vomiting in early infancy, because hypertrophic pyloric stenosis was not the only cause for it. Duodenal obstruction was not infrequently confused with pyloric, and the early diagnosis of duodenal obstruction was to be confirmed only by X-ray examination.

Dr. F. J. POYNTON said his experience with X-rays for the diagnosis of hypertrophic stenosis of the pylorus had not been helpful, and, except in doubtful cases, he had not employed them. Others who had had experience, however, held the opposite view. A celebrated German surgeon, who had done much pioneer work on the subject, attached much importance to them. He hoped Dr. Schlesinger would make his pronouncement about X-rays in these cases rather less sweeping in view of the present divergency of opinion.

Dr. JEWESBURY referred to a previous discussion before the Section on this subject. A valuable paper was read by Dr. Fildes and Dr. Forest-Smith.[1] Many cases of pyloric stenosis had come under his notice, and he attached very great importance to the aid afforded by X-rays in diagnosis. The patients in his cases, forty to fifty, had been X-rayed. Perhaps the method in which the bismuth mixture was given had some influence on the result. One objection raised in the paper was that bismuth caused spasm of the œsophagus, and that therefore the substance never got really into the stomach. That difficulty had been overcome in his own series by introducing the bismuth fluid through a catheter tube into the stomach, and skiagrams were taken immediately after the meal was taken, and then at regular intervals subsequently. In most cases, no bismuth had left the stomach after considerable intervals, and in all the cases only a very small amount had succeeded in getting through when there was true pyloric stenosis. He intended to persist in this practice wherever he considered that pyloric stenosis might be present.

The extraordinary decrease in the mortality in this condition was probably due to two things. The first was the surgical technique. Operation having been decided upon, it was of the greatest importance that the surgeon selected should be one who had had experience of this particular operation. The operations in all his own cases of the condition had been done by Mr. Max Page. Having done this operation a number of times, a surgeon acquired the special dexterity needed in the case of these delicate infants. The next important cause of the lowered mortality was the after-treatment. The mistake in the past had been in over-feeding after the operation by the introduction of too much food into an intestine which had so far been quite unaccustomed to deal with it. Experience had shown the wisdom of practically starving the infant for some hours after the operation had been done, and then gradually increasing the amount of food.

He wished it to be understood that he did not rely entirely on X-rays for diagnosis, but, taken with other clinical signs, he considered the evidence obtained from the X-ray examination of very great value.

Dr. EDMUND CAUTLEY said the discussion, so far, had been really outside the scope of the paper. The extraordinary divergence of opinion as to the value of X-rays in diagnosing the condition was an important matter. He disagreed with Dr. Jewesbury as to the value he attached to the rays, and on this point supported the author of the paper. It was, he contended, impossible to tell, from an X-ray examination following a bismuth meal, whether

[1] *Proceedings*, 1924, xvii (Sect. Study Dis. Children), p. 55.

one was dealing with spasm or with pyloric hypertrophy. It was because of that, he thought, that cases were operated upon which were merely cases of spasm. He agreed that the X-ray was valuable in the diagnosis of pyloric obstruction from duodenal obstruction.

An essential feature of the diagnosis of the condition was a palpable pylorus; except in a very fat child, the pylorus could be felt in all cases, though possibly not at the first attempt.

He agreed with practically all Dr. Schlesinger said in regard to prognosis. The ultimate prognosis of the condition was very good, the children were very well when once they had recovered from the effects of the operation and the subsequent feeding, i.e., if they escaped diarrhœa. The immediate prognosis depended on several factors. First, it depended on whether one was dealing with a private case or with a hospital case. In private cases the prognosis was good even before the adoption of Rammstedt's operation. In his twenty-seven years' experience of this affection he had only come across fatalities in three private patients. One happened in the early days when the parents refused operation until too late and the child died of shock; the second patient died of peritonitis; the third from hyperpyrexia after a Rammstedt operation. In private, patients were in better condition than were hospital cases, they were seen earlier; one could select the surgeon, and all the advantages of good nursing and after-treatment could be ensured. Hospital results were sometimes good, sometimes bad. Taken generally, the prognosis after operation depended on the degree of marasmus before operation, that is, practically, on the degree and duration of the obstruction.

Dr. SCHLESINGER (in reply) said that he was glad to have had the opportunity of revising his opinions on the value of X-rays in the diagnosis of pyloric stenosis.

Regarding those cases in which the condition was one of high intestinal or duodenal obstruction he would have thought that the diagnosis could have been made without the aid of X-rays since in these babies the clinical signs were apparent from birth. Such signs were the passage of meconium only and the vomiting of all feeds. In the four cases in the present series, in which vomiting was a feature from birth, it did not occur at first after every feed but gradually became more severe.

Recently there had been two cases at Great Ormond Street Children's Hospital. Both infants had only passed meconium and showed vigorous peristalsis but in the opposite direction to that seen in pyloric stenosis. The patients were practically moribund on admission and at autopsy the diagnosis was confirmed. The obstruction in one was at the duodenum and in the other about eighteen inches down the jejunum.

Section for the Study of Disease in Children.[1]

President—Dr. H. C. Cameron.

Four Cases of Endocrine Defect.

By H. Gardiner-Hill, M.B.

CASES.

(1) Pituitary Obesity, with Overgrowth.—A tall boy, who, at the age of 14 years, was very considerably overgrown, and weighed 23 st. A skiagram was taken of his sella turcica, and it was found to be not more than one-fourth of the normal size. His bone development was very advanced. His sugar tolerance was much increased, and his basal metabolism very low: after taking 50 grm. of glucose there was little sign of increased sugar in the blood. The basal metabolism was minus 50 per cent. He has been treated for two years on thyroid extract and whole-gland pituitary extract, and during that time he has lost more than 1 cwt. in weight. His weight is now 13 st. 5 lb., and he is able to get about and help his father in his work. His basal metabolism has gone up to just above the normal, and his sugar tolerance is almost normal. If we were to suspend treatment I think both his sugar tolerance and his basal metabolism would become abnormal again; we tried stopping it for a month or two, and that happened.

(2) Overgrown Pituitary Obesity, not yet Treated.—A boy whose sella is small and his sugar tolerance is increased as in Case 1.

(3) A small boy aged 12 years, very distinctly undergrown. We thought that the anterior lobe of his pituitary was hypoplastic. His bone development is delayed; it is about that of a child aged 7 years. His rate of growth during the last four years has been steadily diminishing. We have not yet commenced to treat him, but shall give him anterior lobe of pituitary.

(4) Pure Hypothyroidism, probably Juvenile Myxœdema.—The basal metabolism when patient came in was minus 30 per cent., the bone development delayed. She was sent up because she was backward and undergrown. She has grown 23 cm. in the last eighteen months, that is, during treatment with thyroid extract, as opposed to the average rate of growth of 7 cm.

Discussion.—Dr. Walter Carr, referring to the first two cases, asked whether Dr. Gardiner-Hill had experimented by giving thyroid or pituitary alone, and watched the effect as contrasted with that of giving the two together.

Dr. Jewesbury asked whether Dr. Gardiner-Hill had seen many cases of undergrowth which he regarded as due to pituitary hypoplasia.

Dr. H. C. Cameron (President) expressed the gratitude of the Section to Dr. Gardiner-Hill for having brought this group of cases together, so valuable for purposes of study. He said he did not know how common, or how uncommon such cases were. In certain families, for reasons of heredity, lesser degrees of overgrowth and undergrowth were alike encountered. It was difficult to say when such cases were beyond the ordinary range of normality. In certain stocks there might be found children of great bulk and rapid growth, with atonic muscles and adiposity, occurring at intervals in the family. He asked whether a history of such familial tendencies was obtained by Dr. Gardiner-Hill.

Dr. Gardiner-Hill (in reply) said experiments had been made by giving the drugs separately, and in many cases thyroid was fairly successful, but the patient became intolerant when a larger dose was given, as less thyroid could be taken alone than with pituitary extract. Whole-gland pituitary increased the tolerance to thyroid, but pituitary alone did not reduce the weight of the patient. Their sugar tolerance curves had been examined before and after

[1] Clinical meeting, held at St. Thomas's Hospital, on Friday, November 28, 1924.

treatment with pituitary alone, and it was found that whole-gland pituitary extract apparently improved the patient's tolerance to a certain extent, but not to an extent to cause loss of weight. Cases of undergrowth due to pituitary hypoplasia were very rare. There were attending hospital a number of cases resembling the Lorrain type of infantilism, very thin children with undeveloped genitalia, &c., but there was no evidence that the condition of the hospital cases was pituitary in origin. The cost of the treatment in the case in which 8 stone in weight was lost, was £10 for the year; but less would be required afterwards to keep the patient in good condition. The dosage started with ½ gr. of each extract (thyroid and whole pituitary gland) a day, and this was followed by a dosage, gradually reached, of 5 gr. of each given three times a day. The first patient tolerated 10 gr. three times a day without showing any untoward effect. In the cases demonstrated he did not think the question of family history entered. There were many cases in which fathers and mothers showed abnormal physical measurements, either in height or weight, and he regarded them as endocrine defects of moderate degree. Children of this kind showed a tendency to appear particularly at the end of a large family.

Case of Persistent Ascites.

By R. C. Jewesbury, M.D.

Patient, a girl, aged 10 years. There is a family history of tuberculosis. She lost a younger sister from tuberculous meningitis. This child has had measles, mumps, and a year ago pneumonia. The ascites was first noticed about eighteen months ago, and from that time it has persisted. Tapping has been necessary on twelve occasions, and at the last one, thirteen pints of fluid were removed. A rather irregular large liver can be felt, but no other definite mass; no glands are palpable. She also has an old fibroid condition of the right lung, and marked displacement of the heart. The ascitic fluid withdrawn shows, on examination, a considerable number of lymphocytes. The whole course of her illness has been completely afebrile; and she is free from pain.

The question is, what is the cause of the ascites? One thinks first of tuberculous peritonitis. In favour of that is the fact that there is a lymphocytosis in the fluid, but the complete absence of fever is against it. The long duration of the ascites is unusual. In order to try to determine whether or not it is tuberculous, we have inoculated a guinea-pig with the fluid; it was killed at the end of six weeks, and no evidence of tuberculous disease was present. No tubercle bacilli were present in the ascitic fluid.

I shall be glad to hear whether other Members have seen cases of chronic ascites, and what they think the origin of this case is.

Discussion.—Dr. F. Parkes Weber said he thought the most probable explanation of the case was that it belonged to the class of polyserositis, and what had been called pericardial pseudo-cirrhosis of the liver. This class (in the sense in which he referred to it) included cases in which there might be no pericardial disease; sometimes there might be no marked degree of perihepatitis. The great point was that the ascites in these cases was brought about by an obstruction to the portal circulation, caused by pressure outside the portal vein or inferior vena cava, or by a weakness of the heart due to old pericarditis with great adhesion, or was due in part to both factors. In some cases tuberculosis had undoubtedly been the cause of the whole disturbance. He had never seen a case of this class at so early an age as that of the present patient.

Dr. Pierret (Paris and La Bourboule) said he considered the case tuberculous. He had heard with great interest the observation of Dr. Jewesbury, and he thought the first fact in the history of this child was the mediastinal tuberculosis. The liver was also tuberculous, and he considered that the ascites was only secondary to the perifibrosis of the liver and cardiac deficiency due to mediastinitis. For that reason tubercle bacilli were not found in the ascitic fluid. There were really two reasons why those bacilli might not be found: the first was because there were probably none, ascites being due to cardiac deficiency and cirrhosis and not to tuberculous peritonitis, and the second, that it was necessary to centrifuge large quantities of fluid for the purpose of finding them. Even then, one could not be sure of finding bacilli in the bottom of the centrifuge tube. At times some might be found lying on the upper layer of the centrifugalized liquid. It depended on the density of the latter. He therefore suggested continuing the search by methodical centrifugalization.

It was a case of "cirrhose cardio-tuberculeuse d'Hutinel" (so-called in France), first described in Paris, 1893, a syndrome which usually began with tuberculosis of the pleura, with, subsequently, tuberculosis of the mediastinum, and often of the pericardium with adhesions. Later, the process extended to the liver.

He therefore thought Dr. Jewesbury was right in thinking that the whole of the process was of tuberculous origin, but that the tuberculous peritonitis causing ascites was not the primary but the last event to have appeared. Dr. Jewesbury pointed out himself the history of an antecedent pleurisy. He (Dr. Pierret) believed these cases were always fatal.

Dr. WALTER CARR said he always considered tubercle the most likely cause of ascites in a child, either through pressure of enlarged tuberculous glands on the portal vein, or, less probably, owing to some cirrhotic process in the liver set up directly or indirectly by tubercle. In either case the ascitic fluid would present simply the characters of an ordinary dropsical effusion and we should not expect to find tubercle bacilli in it. He inferred from Dr. Jewesbury's remarks that the fluid in this patient did not show any indication of an inflammatory origin, such as spontaneous coagulation, &c., and that, therefore, there was no reason to suspect the existence of chronic peritonitis, either tuberculous or due to any other cause.

Case of Muscular Dystrophy (Landouzy-Déjerine type).

By R. C. JEWESBURY, M.D.

A GIRL, aged 10 years, who has been under observation for a short time. There are two other children in the family, quite healthy. Since the age of two years this child has had an expressionless face, and has been unable to close her lips. When she was a small baby sucking, apparently, was not interfered with. The condition began at about the second year. Mentally, she has always been very backward. At first sight she appears to have a complete facial paralysis. The only movement she can make in the face is a slight contraction near the alæ nasi, which imparts to her a somewhat sneering expression. She also has marked muscular atrophy of the shoulder girdle, and when extending an arm shows very definite "winging," more marked on the right side.

I have formed the opinion that it is probably a case of muscular dystrophy of the facio-scapulo-humeral type, as described by Landouzy and Déjerine, though on inspecting her face one does not see any sign of muscular atrophy; that is possibly hidden by the subcutaneous fat. It is unusual to find these cases associated with mental defect or any defect in the nervous system. I shall be glad to hear observations on the case, and especially suggestions as to diagnosis.

Discussion.—Dr. WALTER CARR asked how Dr. Jewesbury accounted for the prominence of the jaws and lips, which gave a prognathous aspect to the face, not usually seen in cases of muscular dystrophy. Also, there was a large amount of subcutaneous fat in the face, whereas muscular dystrophy as a rule was associated with wasting of fat as well as of muscle.

Dr. JEWESBURY (in reply to Dr. Carr) said he had no explanation to offer. What might have produced the prominent appearance of the jaw and lips was, that the child was very conscious of her condition, and often closed her lower lip by pushing it up with her hand; the massage effect of that movement, often repeated, might have caused a hypertrophy of the soft parts. He did not think there was any actual bony enlargement.

Demonstration of Cases and of a Specimen.

By R. C. JEWESBURY, M.D.

(1) TWO CASES OF CLEIDO-CRANIAL DYSOSTOSIS.

Case I.—Female, aged 13 years: ten children in family; six girls, four of whom are affected; four boys, two of whom are affected. Mother also affected. Only the outer third of the clavicle present on the two sides. The shoulders can be made to touch beneath the child's chin.

Case II.—Female, aged 9 years: only child. Husband's brother said to have some chest deformity. December, 1921: Well developed child. Head circumference 19½ in. Sutures and fontanelles closed. Central portions of clavicles missing and shoulders could be made to meet anteriorly. Webbing of toes. Fifth digits of both hands abnormally short.

Condition first described by Barlow in 1883. Named by Marie and Sainton in 1898. In 1905 Villaret and Francoz tabulated seventy-five cases, and in 1910 Fitzwilliams analysed sixty cases.

Often hereditary. Seldom seen in more than two generations. Transmitted by males and females. Sexes equally affected. ? A partial osteogenesis.

(2) Fat Dystrophy.

Female, aged 12 years. Entire absence of subcutaneous fat in upper half of body only noticed since the age of 6 years. Cause *unknown*. Condition *stationary*.

(3) Infantilism following Acute Nephritis.

Female aged 9 years. History of acute nephritis in 1917 accompanied by œdema which lasted six weeks.

1920.—Very undersized and backward child. Urine: low specific gravity, albumin and granular casts present. Blood urea: 35 mgm. per 100 c.c.

Albumin and granular casts in urine persisted and bony changes first noticed in 1923.

	Phosphorus content of plasma	Calcium content of plasma	Blood urea	Urea concentration test	Blood-pressure. Systole
May 4, 1922 ...	5·0 milligrammes per 100 c.c.	10·1 milligrammes per 100 c.c.	162	0·6	100
August 6, 1922 ...	4·7 ,,	9·4 ,,	122	0·5	102
April 12, 1923 ...	5·6 ,,	8·5 ,,	223	0·9	106
			Normal, 20-30 mgr. per 100 c.c.	*Normal*, 2-4 per cent. 2 hours after 15 grm. urea	

(4) Bilateral Nerve Deafness.

Female, aged 6 years. Complete deafness following acute illness in November, 1922, lasting six weeks. Illness characterized by lethargy, headache and vomiting. When first seen, December, 1922, was completely deaf in both ears and very emotional. No abnormality was found in either drum. No other signs found in central nervous system. Wassermann and Sachs-Georgi reactions of blood and cerebro-spinal fluid negative. No improvement has occurred in hearing. Deafness. (?) Post-encephalitis lethargica.

(5) Multiple Exostoses (Familial).

Female, aged 14 years. Father and elder brother and sister have similar exostoses. Patient shows exostoses on ribs, both wrists, right ankle and both femora. No disability except pain on pressure. Very slowly advancing.

(6) Hypopituitarism.

Female, aged 13 years. First seen, October, 1921. Weight, 7 st. 1 lb. Excessively fat, overgrown child. Irregular clonic movements of face, tongue and larynx. Intelligence and sexual development normal. Patient showed double primary optic atrophy with contraction of left visual field in temporal half. X-ray: pituitary fossa normal. Basal metabolism, 33·3 per cent. Sugar tolerance curve: Fasting level, 0·118 grm. per cent.; ½ hour after 30 grm. glucose, 0·204 grm. per cent.; 1 hour after 30 grm. glucose, 0·093 grm. per cent. Urine after 150 grm. glucose showed slight glycosuria. Present weight, 9 st. 8 lb. Condition slowly advancing, progressive loss of vision, and frequent headaches.

(7) Polyarthritis.

Female, aged 4 years. First seen September, 1924. Six months' history of pain in feet and knees. One week's history of swelling of ankles. Irregular fever of long duration.

On admission, both knees and ankles showed slight effusion with periarticular thickening, and there was a purpuric rash on the legs.

Since admission, general condition has become worse and other joints have become involved. Wassermann reaction negative. Blood-count: no leucocytosis. Blood-culture sterile. Cause not known. The only possible septic foci are the tonsils, which will be enucleated.

(8) Infantilism.

Female, aged 1 year 7 months. Patient did fairly well till 1 year old when she weighed 17 lb. First seen, July, 1924. Three months' history of diarrhœa with pale-coloured stools. Weight 13 lb. 8½ oz.

Since admission there has been no diarrhœa, the stools being pale and bulky. Fæces: total fats, 42·5 per cent.; fatty acids, 27·3 per cent.; neutral fats, 14·95 per cent. There has been intermittent glycosuria. Sugar tolerance curve normal. No glycosuria after 15 grm. of glucose. Treatment: Low fat diet; liq. pancreat; sunlight. Present weight, 14 lb. 5 oz. Stools: no diarrhœa, but light-coloured small stools. No albuminuria.

(9) Generalized Muscular Hypertrophy with Mental Backwardness.

Male, aged 12 years. First seen March, 1921. Marked muscular development all over. Speech difficult. Facial spasm on speaking. Mental development slightly below normal. Generalized muscular hypertrophy especially marked in shoulder girdle. Electrical reactions normal.

The boy resembles a miniature boxing champion. His strength is in proportion to his muscular development. His disposition is idle, and he dislikes games and exercises.

(10) Mongolism.

Twins, female, aged 1 year, one of whom is a typical Mongol, while the other twin shows no traces of Mongolism.

(11) Tabo-Paresis.

Male, aged 11 years. Mother: aortic regurgitation, gummatous periostitis of skull. Wassermann negative. Three children living, five miscarriages, eleven died in infancy. Patient first seen in March, 1920. Brought up on account of interstitial keratitis. At that time the gait was noticed to be ataxic and child was dull and backward. Wassermann reaction: positive (blood), negative (cerebro-spinal fluid). November, 1920: Wassermann reaction of blood negative. Patient now shows corneal haze in both eyes, pupils irregular and sluggish to light. Some ataxia, knee-jerks present. Mentally very deficient. Treated with novarsenobillon and mercury. No evidence of improvement after treatment.

(12) Congenital Syphilis; appearance at age of 9 years.

Female, aged 9 years. Mother: Wassermann reaction positive + + +. One other child, 14 years old, said to be well. No history of disease in infancy. One year ago right arm and leg noticed to be weak. No history of acute illness at the time. *Now* Wassermann reaction positive +. Right arm and leg spastic. Hutchinson's teeth. Both pupils dilated and irregular. Inactive to light and accommodation. Haze of both corneæ. No improvement after treatment (N.A.B. and Hg).

(13) Congenital Syphilis; appearance at age of 3 years.

Female, aged 3½ years. Mother: Wassermann reaction positive + + + untreated. Children: (1) Born 1919. Died, scarlet fever, at 1 year 4 months. (2) The patient. (3) Born 1922. Died, bronchopneumonia at 7 months. (4) Born 1923. Healthy child. Wassermann reaction negative. First seen: October, 1924. Wassermann reaction positive + + + +. Three months' history of swelling of knees. On examination both knees were swollen, practically painless, slight effusion in both knee-joints, considerable degree of periarticular thickening. Left elbow also affected. Three weeks later while under treatment patient developed interstitial keratitis of right eye. Treatment: novarsenobillon intravenously and Hg. Pot. iod. by mouth.

(14) Polyarthritis.

Male, aged 12 years. First seen August, 1918. Patient gave two months' history of swelling of small joints of both hands. Axillary and inguinal glands were enlarged. Spleen was not palpable. Wassermann reaction negative. The arthritis has persisted and other joints have become involved, particularly the wrist-joints. No cause has been discovered.

(15) Specimen of Spleen with Sections of Spleen.

Male, aged 8 years. The spleen was enlarged to about four times its normal size. The section showed a simple hyperplasia. First seen October 6, 1924. Three years' history of hæmatemesis and melæna occurring every two or three months accompanied by increasing pallor. On examination: well developed child, extremely pale and well covered. No jaundice. The spleen extended to the level of the umbilicus and felt hard and smooth. The liver edge was just palpable below the right costal margin. The abdomen was slightly distended but there was no free fluid. Other systems normal. Splenectomy, October 23, 1924. After operation patient's temperature became irregular, ranging between 97° and 103° F.

Nothing definite was found to account for the temperature, and patient had three further attacks of hæmatemesis after the operation and died twenty-four days after operation.

(The four cases following have been under treatment in the Orthopædic Department.)

(16) Infantile Paralysis: Tendon Transplantation and Arthrodesis.

Boy, aged 10 years, with paralysis of the tibialis anticus, following an attack of infantile paralysis in infancy. In November, 1920, peroneus longus and extensor longus digitorum were transplanted into the internal cuneiform. Two years later there was a marked valgoid deformity, and the foot was stabilized by arthrodesing the mid-tarsal and subastragaloid joints.

The result is a firm stable foot, shortened but without deformity, and one which permits of good function without the use of instruments. Arthrodesis is generally required, with or without tendon transplantation in order to stabilize the foot in cases of paralytic deformity.

(17) Synostosis of Radius and Ulna—Congenital.

Girl, aged 8 years, with the forearms held in fixed pronation, and with complete loss of the power of supination, either active or passive.

X-ray shows bony fusion of the upper ends of radius and ulna. The prognosis is poor, as operative attack in such cases is singularly disappointing in its results.

(18) Congenital Deformity of Right Femur.

Child, aged 5 years, who first came under observation when 14 days old. At that time the right femur was shortened and was not more than half the length of that on the left. There was, as is usual in these cases, fixed flexion deformity of the right hip-joint.

The fixed flexion at the hip was overcome by repeated manipulation and stretching, and the child is shown walking well, without deformity except for the shortening. This is compensated for by a high boot. Congenital malformation of the femur is not very uncommon, and if the fixed deformity is overcome the patients can get about well, once the shortening is adequately compensated.

(19) Result of Treatment of Congenital Dislocation of Right Hip.

Girl, aged 3 years, who first came under observation at 1 year 11 months, with congenital dislocation of hip. This was reduced under anæsthesia by manipulation in August, 1923.

This patient is now shown, thirteen months later, walking well and with no evidence of deformity. The end-results of treatment in congenital dislocation of hip largely depend upon the age at which the patient comes under treatment. In children under 3 years, some 75 per cent. of cases should yield a good anatomical and functional end-result.

Section for the Study of Disease in Children.

President—Dr. H. C. Cameron.

DISCUSSION ON PNEUMOCOCCAL PERITONITIS.

Dr. H. C. Cameron (President of the Section).

In opening this discussion on pneumococcal peritonitis in children I propose to speak only of symptoms. Dr. Nabarro will deal with the bacteriology, and also with that very vexed question of the path by which the infection reaches the peritoneum, whether through the blood-stream or by extension from a previous enteritis. A study of the cases at the bedside affords me no answer to the problem, nor does it explain why so very great a preponderance of cases in every experience and all statistics should occur in little girls between the ages of four and fourteen. On these points I would make only two observations. It has been said that primary pneumococcal peritonitis is, indeed, confined to the female sex, and does not occur in the male. I admit the overwhelming preponderance of females, but I think that statement too absolute. I have twice met with pneumococcal peritonitis in boys aged four and six respectively, which, so far as I could tell, was primary. In infancy, I think it is likely that males and females are attacked in equal numbers. I saw recently a typical case in a male infant with Dr. Dundas Irvine, in which after a laparotomy in the fourth week of the illness performed by Mr. Hughes, the child made an uninterrupted recovery. The second observation I would make is that in a minority of the cases the history of the onset affords some support to the view that the peritoneum is reached by an extension from a preceding enteritis or colitis. It is not unusual to find a history of slight illness, with colic and diarrhœa, preceding by some days a moment when the onset of peritonitis is announced by a great exacerbation of the pain, by a high temperature, by delirium, expressionless face, rapid pulse and abdominal rigidity.

Thus, a girl aged 9 years had been away from home on a visit. For six days she had suffered with diarrhœa, colic and a feeling of chilliness. She returned home on a Saturday. That night her temperature was 104° F., she was delirious, obviously extremely ill and complaining of intolerable abdominal pain. On the Tuesday following, a laparotomy showed extensive pneumococcal peritonitis. On Thursday she died. In one case this preliminary diarrhœa, whatever its nature, affected more than one child in the family. In a case which I saw recently with Dr. Robertson of Dulwich, three or four of the children had been ill in this way. One of them had had herpes labialis. All recovered after some days except a little boy aged 4, whose illness had lasted, before I saw him, for four weeks. At first appendicitis had been feared, but later it was clear that an exudate of fluid had taken place into the abdominal cavity, and the likelihood of tuberculous peritonitis had been considered. He was admitted to Guy's Hospital and the same evening many pints of pus from which the pneumococcus was cultivated were removed. He made an uninterrupted recovery. This history of preliminary diarrhœa is not common, but it is, I think, too frequent to be accidental.

In the endeavour to simplify the clinical description of the disease various subdivisions of the material available have been suggested. Thus the cases may well be considered under two headings—those which are primary—that is to say, those in which no other focus of infection such as pneumonia, endocarditis, pericarditis, &c., is present—and those which are secondary, in which the peritonitis is an incident in a more widely spread infection of the body. To-night we confine ourselves, for the most part, to the first or primary group. Certainly, where other incalculable factors, such as a simultaneous pneumonia, are present, we cannot usefully consider the relative merits of different lines of treatment.

A second division which I notice in most published descriptions is a vertical cleavage of the material into two groups—those which show a localized peritonitis and which in general are marked by lesser constitutional disturbance and a lower death-rate, and those which show a generalized peritonitis, constitutional symptoms of the greatest severity and a very high death-rate. One can recognize the distinction between cases with severe and with relatively mild symptoms, but I have seen nothing to support the pathological deduction that in the first the general peritoneal cavity is involved, in the second only a part of it. In cases which have survived the active stage and present themselves late in the disease, with what has been called an abdominal empyema, I have not found that it makes any difference to prognosis at this stage—for the most part favourable—whether the residual collection of pus is free in the peritoneal cavity, or confined to one or other segment of the abdomen. In the early stages of the disease it is not possible to make any certain separation between peritonitis that is local and peritonitis that is general. The spread of the infection may take place with very great rapidity.

A girl, aged 20 years, was admitted to Guy's Hospital on November 11, 1911. On November 8 she had felt ill, had abdominal pain and diarrhœa. On November 9 she stayed in bed. On the following two days she went back to work. On the afternoon of the day of admission she was suddenly seized with pain and vomited. On admission, at 10 p.m., her temperature was 104° F. The abdomen moved badly and the appendicular region was rigid. I saw her at 11 p.m., and got Mr. Hughes to operate. The wall of the cæcum was stiff and rigid and there were a few flakes of lymph on the peritoneal coat. She died thirty hours later, the temperature continuing high to the end. At the autopsy there was acute follicular ulceration of the cæcum and colon, spreading to the base of the appendix. The peritoneal cavity, which at the operation thirty hours before had been empty, was completely filled with thick purulent lymph from which a pure growth of pneumococcus was obtained.

A more useful division appears to me a horizontal one into stages. As in typhoid fever and other continued fevers the diagnosis of the disorder can sometimes be made only by noting the orderly appearance of a succession of phases, in each of which the symptoms alter in character in a way that can be anticipated and predicted. With this method of division, also, the difficulties are great because, in my experience, it is very rare to have the opportunity of examining and recording the whole course of any one case of pneumococcal peritonitis from first to last. Cases which come under our survey at the outset are too often of maximum severity and end fatally within a few days. On the other hand in the great majority of cases, in the late and quiescent stage, which I have encountered, the patients have indeed had an illness of acute onset some weeks before, but it has not been realized in the early stages that the peritoneum has been affected.

Nevertheless, I think it is possible to divide the stages of the disease into three—a stage of onset, a stage with pneumococcal fever, and a stage with residual collection of pus within the peritoneum.

(1) *The Stage of Onset.*—Whether or not the case shows a prodromal phase of diarrhœa, the onset of the peritonitis appears sudden and definite. Within a few hours the aspect of the child may be that of severe and alarming illness.

A little girl, aged 7 years, shopping with her mother, was seized with abdominal pain, and great difficulty was experienced in getting her to walk a mile or so home. That night when I saw her she was almost comatose, with muttering delirium and expressionless face. She died three days later.

In less severe and fulminating cases the confusion which is likely to arise is with appendicitis. The suddenness of the onset may arouse the fear of perforative peritonitis, but the rarity of gastric or duodenal ulceration in childhood makes such an explanation highly unlikely. In pneumococcal peritonitis delirium at the onset is a very constant and prominent symptom. The bearing and attitude contrast with what is the rule in appendicitis, when the child is alert and acutely conscious

of the severity of the pain. In pneumococcal peritonitis there is apt to be drowsiness and confusion, an expressionless, flushed face, toneless voice or active delirium. Herpes has seldom helped me by its presence. The leucocytosis, usually high in the later stages, is often little marked at the onset. Abdominal pain and tenderness, diarrhœa, and vomiting are the rule. Rigidity of the abdominal wall is often slight or absent. The pulse is very rapid, the breathing sometimes fast, with hyperpnœa, the temperature often at a height which alone suggests a diagnosis other than that of appendicitis.

(2) *The Stage with Pneumococcal Fever.*—In cases which survive the first four or five days, a certain amelioration of the symptoms often appears. This improvement may coincide with the pouring-out of lymph into the peritoneal cavity. For two or three weeks the temperature may continue high, with intermittent abdominal pain and with a somewhat swollen and tumid abdomen. In the past such cases have often, not unnaturally, been confused with typhoid fever. I have notes of three cases which were admitted to Guy's Hospital, the patients having been transferred from the fever hospital to which they had been sent as examples of typhoid fever. In typical cases, after a period of sustained pyrexia of varying duration, the curve tends to become remittent or intermittent as in empyema, while gradually the evidence of a collection of pus within the abdomen becomes appreciable.

(3) *The Stage with a Residual Collection of Pus within the Peritoneum.*—In this stage, just as the symptoms of empyema gradually evolve from the preceding pneumonia, so with the termination of the pneumococcal fever there is unmasked the persisting local disturbance within the peritoneum. The pus may be free in the peritoneal cavity.

In one case the pressure within the abdomen was so great and the diaphragm was pushed so high that a mistake in the diagnosis was thus caused. So extensive was the bilateral dullness of the bases of the lungs that a needle was passed into the eighth intercostal space and pus withdrawn on both sides. On the left side a piece of rib was removed and a drainage tube was inserted. Only later was it discovered that the exploring needle and the drainage tube had both passed below the diaphragm and that the abdominal cavity was being drained in this way. Recovery followed upon a laparotomy.

Sometimes the collection is subdiaphragmatic only, much more frequently the lower part of the abdomen is the site involved.

It is important to recognize how latent the symptoms may be. As a rule, in this stage the difficulty is to avoid confusion with tuberculous peritonitis. This is shown by the history of the following three cases from Guy's Hospital:—

W. J. T., male, aged 10 years, was admitted in September, 1906. On August 24 he had been taken ill with pain and swelling of abdomen, vomiting and diarrhœa. Until admission he had appeared to improve, but had remained languid and sleepy. The abdomen was held tight, moved badly, and the left flank was dull. Tuberculous peritonitis was diagnosed. He was discharged, much improved, on November 15, 1906. Seven months later, June, 1907, he was readmitted. Since discharge he had often had attacks of pain, and had wasted. *Three weeks before, a swelling at the umbilicus had burst.* Again a diagnosis of tuberculous peritonitis was suggested. Tuberculin treatment instituted, without improvement. As the pus was free from fæcal odour, a cultivation was made, and a pure growth of pneumococcus obtained. A vaccine was prepared and administered, and he went out on February 3, 1908. The sinus had almost healed.

D. S., female, aged 5 years, was admitted April 5, 1911. Six weeks before had had an acute illness. At first appendicitis was diagnosed, later pneumonia. After the first week the abdominal pain disappeared, but the child remained without appetite. Before admission it was recognized that there was free fluid in the abdominal cavity. A diagnosis of tuberculous peritonitis was made, and the child was kept out of doors. On April 30 a *swelling appeared at the umbilicus*, and became so prominent that it was decided to operate. As soon as the peritoneum was opened pus poured out; three pints were collected. A pure growth of pneumococcus was obtained. Recovery followed, and the child was discharged well on July 8.

W. A., female, aged 8 years, admitted July 9, 1903. On April 20 she had suddenly had abdominal pain. An acute illness of many weeks' duration followed, which was diagnosed as typhoid fever. The abdomen was swollen and contained fluid. *On the day before admission a fistula formed at the umbilicus.* Mr. (now Sir Arbuthnot) Lane operated, and one and a half pints of *greenish-yellow* pus escaped. The child recovered, and went out on September 3. In March, 1905, the child was well.

In this third stage the prognosis is good. Recovery almost invariably ensues. I have met with only one case which ended fatally, apparently as the result of extreme emaciation and weakness, caused by a fæcal fistula having formed. Recovery has been the rule, whether the pus has lain free within the peritoneal cavity, with the usual signs of thrill and movable dullness, or whether there has been a localized abscess in the upper or in the lower part of the abdomen. The cases which have given most anxiety have been those in which the pus was loculated and which required very many incisions at separate times to evacuate it. My one fatal case was of this nature. One little girl whom I saw, in consultation with Sir Arbuthnot Lane and Mr. Hughes, had no fewer than five abscesses opened at considerable intervals, yet made a good recovery. As might be expected the tendency to spontaneous evacuation of pus is well marked. Cases have been recorded in which the pus found an outlet by the bowel, and even by the bladder. But by far the commonest portal for the spontaneous escape of pus is the umbilicus; this was the method of evacuation in all three cases of which I have just read notes. I have encountered this occurrence in several other instances. When a tuberculous abscess points at the navel it does so very slowly. Gradually the umbilical cicatrix becomes fixed and the skin around it reddened and infiltrated. In contrast with this the pneumococcal pus escapes suddenly and rapidly with very little reddening or fixation of the umbilicus. I have seen pus escape suddenly and unexpectedly in great quantity during the night. In the morning when this was reported to the surgeon, he was inclined to disbelieve the story, because a very close examination of the umbilicus seemed to show no sign of a fistulous opening. A few nights later the sudden discharge repeated itself and was seen to come through a slit-like opening at the side of the umbilicus with absolutely no signs of reddening or thickening. Later, much pus, from which the pneumococcus was cultivated, was evacuated by an abdominal incision. The quietness of the escape is characteristic of the pneumococcal infection. Similarly one notes how often a sudden, unexpected and painless discharge from the ear will be found due to a pneumococcal infection.

Such, in brief, are the symptoms which characterize the three stages of pneumococcal peritonitis. In clinical medicine great detail or precision in description is apt to suffer from the drawback that the total of exceptional or atypical cases grows thereby to formidable proportions. Anyone who studies a large series of examples of pneumococcal peritonitis will find many which present a very different course. For instance, the temperature may be subnormal throughout, although the pulse is almost always extremely rapid. Nevertheless, I think that the broad outlines of these three stages recur with sufficient constancy to make their detailed study serviceable.

Treatment.

Upon this important point I still have an open mind. It is very difficult to be dogmatic. Is immediate laparotomy and drainage the best policy or is it best to await the termination of the pneumococcal fever and then to evacuate the "abdominal empyema" at as early a stage as possible? It is clear that in a very high percentage of cases the infection proves fatal within a few days. The child is overborne by the shock of the onset. It is equally clear that when the pneumococcal process is at an end and the case presents itself as that of a collection of pus pent up within the abdominal cavity, recovery after evacuation may be confidently

expected. The question then narrows itself down in this way. Does immediate laparotomy and drainage diminish the severity of the intoxication? Does it assist in that struggle between antigen and lytic antibodies on the result of which death or recovery depends? For in pneumococcal peritonitis there is, of course, no question of the extirpation of a dangerous focus of infection. For many years I have carefully examined all the charts of cases of pneumococcal peritonitis which I have encountered, on the look-out to note any favourable effect upon the temperature or the pulse-rate which might be attributed to the operation. It is, I think, rare for the curves to show any alteration which could be so attributed. In the majority of cases, too, the first operation, carried out at the height of the initial illness, has not obviated the necessity for a second operation to evacuate pus, if the patient has survived to reach the post-pneumonic quiescent stage, when the final triumph of the antibodies has taken place.

Operative interference is almost invariably necessary, but the difficulty of decision concerns the time when it is best carried out. I think that in most cases that moment should be selected when localizing signs have appeared in the abdomen and when the termination of the pneumococcal process has been reached. This is often indicated by a change in the character of the temperature chart, and by an alteration in the general symptoms and appearance of the patient—just such an alteration as accompanies the change from a lobar pneumonia to the resultant empyema. Until that time, rest, opium, and ice to the abdomen may be prescribed. Dr. Fraser, who, I think, advocates immediate laparotomy, has shown very clearly the good results obtained from blood transfusion. Anti-pneumococcal serum may possibly afford direct assistance. If the diagnosis is clear I prefer to rely upon such means and to delay the laparotomy until the conclusion of the struggle with the pneumococcal infection. The treatment of pneumococcal peritonitis would thus fall into line with that of pneumococcal infections elsewhere, with that of empyema, pyopericardium, and pneumococcal arthritis. The difficulty of being sure of the diagnosis, of course, remains. It is better to perform an early exploration of a case of pneumococcal peritonitis than to permit a case of gangrenous appendicitis to remain unrelieved. Before we withhold operation we must be very sure of our facts.

Mr. George E. Waugh.

Pneumococcal peritonitis is the one form of peritonitis from which all the ordinary signs and symptoms of inflammation of the peritoneum are conspicuously and characteristically absent. By that I mean that within a few hours after the onset of an acute abdominal crisis there is no persistence of abdominal pain, tenderness, reflex muscular rigidity, or immobility of the abdominal wall. Except for slight preliminary vomiting and two or three loose motions the function of the alimentary tract is unimpaired, and throughout the course of the illness, whether for better or worse, food is taken and retained by the patient whenever the appetite is not impaired by fever, and the bowels act with daily regularity. The only evidences therefore of a persistent inflammation are a swinging temperature and an accelerated pulse-rate—signs that are of no value in attempting to determine either the site of the lesion or the ætiological factor. Some key to the understanding of this silent lesion may be found in a study of the characteristics of the pneumococcus when it has wandered away from its normal habitat—the lungs and pleura—and settled elsewhere. In all other parts of the body it is essentially an organism that causes no pain, or at any rate only a transitory discomfort within a few hours of an initial invasion, and this freedom from pain persists throughout the steady progress of the disease. This holds equally true for an involvement of bones, joints, the middle-ear, the mastoid, the pericardium, and, lastly, the peritoneum. Those who are familiar with multiple pneumococcal lesions will recognize how obscure may be the diagnosis

of a pneumococcal effusion into the hip-joint, the middle-ear and the pericardium, because of the freedom of the patient from pain and tenderness in those regions. Its behaviour therefore in the peritoneal cavity is merely a specific instance of one of its general characteristics. So much, then, for the important *negative* characters of an acute abdominal disease, the true nature of which is likely to suffer from delayed recognition, to the disadvantage of the patient if the ordinary signs of peritonitis are expectantly awaited. Many an observer has remarked at the late stage of the disease that he has sought daily for signs of peritonitis and could find none—a perfectly accurate observation. But as the disease progresses one *positive* sign of the presence of the pneumococcus in the peritoneal cavity makes its appearance, and this is of the greatest value. A slowly and steadily increasing tumescence of the abdomen occurs, which is seen earliest, by those accustomed to await its appearance, in the subcostal grooves, the groins, and the region of the umbilicus. This tumescence is due to an increase of the contents of the abdominal cavity by the outpouring in bulk of the green, non-irritating pneumococcal pus, and it is not due to distension of the intestines. An acid test of the value of such a clinical description, both in its negative and positive aspects, may be provided best by details of cases.

Case I.—In the year 1908 a baby girl aged 14 months was brought to me at the Hospital for Sick Children, Great Ormond Street, with a history of six days' feverish illness, that had begun with a sharp pain in the region of the left iliac fossa, lasting for a few hours only. She had vomited once, and diarrhœa had been present for a few hours—since then fever had been the only noticeable trouble. On examination the left side of the pelvis and of the abdomen as high up as the iliac crest was found to be full of fluid, and a diagnosis of pneumococcal peritonitis was made. As there was a vaginal discharge and the pain of onset had been experienced on the left side of the lower abdomen, the conjecture was hazarded also that the infection had begun in the left Fallopian tube. A few hours later I evacuated by laparotomy the usual large collection of green pus, and then found that the left tube was swollen by œdema and inflammation to the size of that of an adult, with beads of pus oozing through a patent ostium into the pelvis. The tube was then removed, the peritoneal cavity drained, and the child made a speedy recovery. This is one of the youngest cases of salpingectomy; but although a pure growth of the pneumococcus was obtained from the abdominal pus, a mixed growth containing some pneumococci was obtained from a vaginal swab.

Case II.—By way of contrast, a failure to attach sufficient importance to the positive sign of tumescence, with probably disastrous consequences to the patient, may be quoted. In 1922 a well-nourished boy aged 7 years was sent to me at Great Ormond Street with the diagnosis of "appendicitis." He had already been ill for eight days, and the attack had begun with pain in the right side of the abdomen, one vomit, and a few loose stools. Except for fever he had remained free from abdominal signs and symptoms until within twenty-four hours of his admission to hospital, when a similar attack of pain had been experienced in the left side of the abdomen. Routine examination revealed the presence of a large left-sided empyema, and this appeared to be sufficient explanation of his illness. The general condition of the patient was so satisfactory that the slight tumescence of the abdomen, to which I called attention, I attributed to good nourishment. The pneumococcal empyema was operated upon a few hours later, and during the few following days it proceeded to heal rapidly, but without any abatement of the toxæmia, and without any detectable change in the physical signs in any other part of his body. He died on the fifth day after operation; at the post-mortem examination the peritoneal cavity was found to contain a large quantity of pneumococcal pus. It was a sad failure so signally to misinterpret the great significance of the tumescence; the patient was a strong healthy child with no trouble elsewhere; and I have no doubt that drainage of the abdomen would have saved his life.

Lastly, an interesting contrast may be drawn between the early stages of an acute pneumococcal and of an acute tuberculous peritonitis. In the former, as we have seen, after the onset there is a progressive toxæmia, together with a striking absence of the signs of general peritonitis; in the latter all the signs of a widespread peritonitis are present, with a striking absence of the signs of toxæmia.

Treatment must naturally follow upon diagnosis, and for these purposes the disease may be considered conveniently in relation to "the invasion stage," "the intermediate stage," and the "residual stage." It must be recognized frankly that patients may recover without a diagnosis having been made and without any specific form of treatment having been administered. Evidence to this effect is supplied by "residual" cases, with large encysted abscesses of pneumococcal pus in the peritoneal cavity, presenting themselves for treatment a few months after an illness that has received but scanty attention in the febrile stage, whilst the subsequent swelling of the abdomen is the only trouble of which the patients complain.

Such a case, in a girl aged 12 years, has just been under my care at the Hampstead General Hospital in association with my medical colleague, Dr. Scott Pinchin. She had suffered an unnamed febrile illness in April, 1924, and about one month later, when she was quite well, her mother noticed that the abdomen was swollen. For two months she was treated at another London hospital for tuberculous peritonitis, and finally she came under our care on November 5, 1924. The diagnosis appeared clearly to lie between that of an ovarian cyst, a mesenteric cyst, or an encysted abscess of pneumococcal origin. (It may be noted that when encysted fluid—as distinct from ascites—occurs in a case of abdominal tuberculosis the patient is obviously ill, with additional signs of tuberculosis in other parts of the abdominal cavity; in cases of encysted pneumococcal fluid the patient is apparently well, with an abdominal cavity that is obviously healthy elsewhere.) At a laparotomy on November 6, 1924, I evacuated from three to four pints of green pus that grew a pure culture of pneumococcus, and after a short period of drainage she made a complete recovery.

Treatment in the "residual" stage is thus simple, standardized and effective. It cannot be so in the "invasion" stage, with its atypical characters, until some specific test for the presence of the pneumococcus in the peritoneal cavity has been evolved. Unfortunately, too, the clinical syndrome of that stage is quite indistinguishable from an acute inflammation of the whole structure of a "concealed" appendix—that is, one hidden beneath the liver, behind the root of the mesentery, or in the pelvis. Stress must be laid upon the use of the words "inflammation of the appendix," a condition that is far more grave than "distension of the appendix," with its typical clinical syndrome described so accurately by Murphy as long ago as the year 1894, and so totally unlike that of an inflamed appendix and of pneumococcal peritonitis. Now, to wait until inflammation of a "concealed" appendix has at last become "manifest" by involvement of the adjacent area of the peritoneum, is to permit the patient to drift into that group of appendicular disease which exacts each year its heavy toll of fatal cases; indeed when once the patient has entered this group the chances of recovery under any form of treatment are distressingly slight. With an apprehension, then, born of this dilemma in the invasion stage it has been my rule to operate at once—sometimes finding "concealed" appendicitis, sometimes pneumococcal peritonitis—and as a rule with great benefit to either condition. It is important when the abdomen is open that the surgeon should recognize with precision that pneumococcal peritonitis is present. The pathological appearance to the unaided eye is quite distinctive. At the earliest time of operation after the onset—my own experience is one of eight hours—free fluid is already present in the peritoneal cavity, which is always cloudy, odourless, and frequently contains a few flakes of white lymph. Over a wide area the coils of small intestine are of an angry red colour, with injected blood-vessels, their lustre is slightly dimmed, and these appearances are shared by the appendix, to an equal or less degree, but never to a greater degree. If the appendix is involved in the general inflammation I remove it, and I insert a drainage tube for forty-eight hours. It would be difficult to explain exactly how the tube acts, but it appears to possess some advantage. The free fluid teems with the pneumococcus, but by this method the patient is usually doing well by the third day and the illness has abated.

Two cases have come under my care in which failure to recognize by eye the true pathology of the lesions as revealed at the first operation, and the omission of a drainage tube, appear to have been decisive factors in leading to grave complications. Both were cases of girls aged 13 years and 14 years respectively, with the history that just seven days before they came under my care the appendix had been removed within a few hours of the onset of an acute abdominal attack, and the abdomen closed without drainage. They had become subsequently desperately ill, and on the seventh day presented a perfect clinical syndrome of advanced pneumococcal peritonitis. In each case the abdomen was reopened at once, and large quantities of green pus evacuated, from which pure growths of pneumococci were obtained subsequently. They fortunately recovered, but before the second operation the family doctors each made the interesting remark that they had not been entirely satisfied that the appendix showed such evidence of inflammation as had been alleged by the surgeons. Merely, then, as a personal experience summarizing the treatment of the "invasion" stage, I have never seen a fatal case in which operation was performed within thirty hours of the onset, while accidentally and incidentally many cases of a concealed appendix have been saved from disaster.

Finally, as to the "intermediate" stage, when the tumescence of the soft abdomen is such a marked feature of the illness, the need for immediate operation appears to be determined only by the degree of toxæmia that is already present or by its tendency to increase. With the knowledge that the illness may come to an abrupt and favourable termination spontaneously, cases that have reached the "intermediate" stage may be judged upon their merits. A climbing pulse-rate is the most important indication for operative interference.

In 1922 a girl, aged 11 years, was admitted to my ward at Great Ormond Street on the sixth day of her abdominal illness. From the time of onset her temperature and pulse-rate had been recorded regularly by a district nurse, and the chart showed that while the temperature had been swinging from 103·4° F. to 98° F. the pulse-rate had remained constant at about 100. She had the typical swollen, soft abdomen, and the prophecy was made that provided the pulse-rate remained materially unaltered the illness would probably come to an abrupt end on about the tenth day. It actually did so on the twelfth day, and although she was kept under observation for a further six months there was never any evidence to suggest the presence of unabsorbed fluid in the peritoneal cavity. Presumably in this group of cases the pneumococcus is entirely killed; since in the "residual" cases which come to operation many months after the original illness the pneumococcus can always be cultured from the evacuated pus.

These remarks are intended to apply solely to those cases in which peritonitis is the only pneumococcal lesion that can be found, or where it is associated with a chest lesion that has run a comparatively benign course. Its sinister reputation appears to me to be quite undeserved and to have originated from a confusion between this type of case and pneumococcal septicæmia, in which the infection of the peritoneum is merely an incident in a disease in which every serous cavity of the body is involved. Surgery can offer no help in the latter disease, but after twenty years' experience of the former I still approach it from the surgical standpoint with profound optimism.

Dr. D. Nabarro.

Like most micro-organisms the pneumococcus shows considerable variations in its pathogenicity towards animals. The rabbit and mouse are very susceptible, so that a small dose of a pneumococcus culture usually kills the animal in twenty-four to forty-eight hours and the organisms swarm in the blood—a true pneumococcal septicæmia. The pigeon, on the other hand, is immune, whereas the dog, sheep, guinea-pig and rat occupy an intermediate position. Man is only partially susceptible, so that an infection by the pneumococcus is usually localized, the commonest lesion being a pneumonia. By direct extension, pneumonia is sometimes complicated by empyema, and more rarely other serous membranes; the pericardium, peritoneum, and meninges may also become infected. Children are more susceptible to a pneumococcus infection than are adults, and pneumococcal pleurisy, pericarditis, peritonitis and

meningitis are more frequently met with in childhood than in adult life. When I was pathologist at the Evelina Hospital, some twenty-five years ago, I saw several cases of multiple serous membrane infections with the pneumococcus, but my more recent experiences at Great Ormond Street lead me to believe that these generalized infections are not so common as they were. If my surmise be correct, it is probably to be explained by the advances which have been made in the treatment of empyema in children, so that the pneumococcus is not given the opportunity to infect the other serous membranes. The variety of peritonitis complicating a pneumonia, pleurisy, pneumococcal arthritis, &c., is usually known as secondary pneumococcal peritonitis, and it need no longer detain us from a pathological or bacteriological point of view. It is the primary form of pneumococcal peritonitis, in which the abdominal lesion is the only evidence of infection, and in which the peritoneum is primarily infected, that is perhaps the more interesting, from the point of view both of diagnosis and pathogenesis.

Primary pneumococcal peritonitis is not a common disease, so it does not fall to the lot of any one individual to be able to study a large series of cases. The most important papers on the subject are those by Dr. J. E. McCartney and Mr. John Fraser in the *British Journal of Surgery*[1] and by Dr. McCartney, in the *Journal of Pathology and Bacteriology*.[2] The authors of these papers are to be congratulated upon their good fortune in having had so many cases to study and also upon the thoroughness with which the clinical and pathological (including the bacteriological) details have been worked out.

The first point of interest is the greater prevalence of the disease at the Royal Hospital for Sick Children in Edinburgh than at the Hospital for Sick Children, Great Ormond Street. At the Edinburgh institution the admissions for pneumococcal peritonitis were: in 1902, four; in 1911, seven; and in 1920, fifteen cases. The authors think that while the proportional occurrence has remained constant, better diagnosis and earlier surgical interference account for the higher numbers in 1920. At Great Ormond Street our experience is very different. I have gone through the records of acute peritonitis for the past nine years (1916-1924) and, excluding cases of tuberculous peritonitis and septic peritonitis following appendicitis, I have found not more than fifteen cases in all, less than half being of pneumococcal origin. The accompanying table gives details of all the cases I have discovered. It shows the relative paucity of the cases as compared with the Children's Hospital in Edinburgh, although the patients are drawn from similar classes of the community. The second point, and one in which both records agree, is that all the patients are females. This is an important point when considering the portal of entry of the infecting organism into the peritoneal cavity. I regret that the investigations in our cases were not carried out so thoroughly and systematically as in those of McCartney and Fraser. These authors examined swabs from the pelvic peritoneum, from the upper part of the abdomen, and from the vagina and throat. Blood-cultures and urinary precipitin tests were also carried out. In all ten cases examined, a pneumococcus was isolated from the abdominal cavity, vagina and heart-blood, the same type of pneumococcus being isolated from all three situations (Type I in eight cases and Type II in two cases). Judging by the tests used by McCartney for identifying the pneumococcus, namely: (1) the bile-solubility of the organism; (2) its capacity to ferment inulin, and (3) the typing with Rockefeller type sera—there is no doubt that a pneumococcus was isolated by that author in every case. It is otherwise with the cases investigated by myself. Of the thirteen cases studied bacteriologically, seven showed a coccus morphologically like the pneumococcus in smears, or cultures, or both, but all of these may not have been true pneumococci, as the bile

[1] *Brit. Journ. Surg.*, 1921-22, ix, p. 479.
[2] *Journ. Path. and Bact.*, 1923, xxvi, p. 507.

CASES OF ACUTE PERITONITIS ADMITTED TO THE HOSPITAL FOR SICK CHILDREN, GREAT ORMOND STREET, 1916-1924 (9 YEARS).

Year	Name	Age	Sex	Clinical history	Pathology and bacteriology	Result	Organism
1916	C. G.	Two years six months	F.	Two days' history. Pain in abdomen. No vomiting. Thin watery pus. Temperature 104° F.	Pneumococci in smears and culture	Death fifth day after operation	*Pneumococcus*
1916	E. K.	Six years	F.	Ten days' history. Slight abdominal pain. Pain worse and vomiting day of admission. Temperature 103°-104° F. Operation: Abdomen full of pus	Mixed growth long-chained streptococci and pneumococci	Death day after operation	*Pneumococcus* and *Streptococcus*
1916	I. B.	Two years	F.	Ill: One month. Diarrhœa and pain in upper part of abdomen with a lump. Wasting one month	Pus from abscess, streptococci grown	Death	*Streptococcus*
1917	K. B.	Four years six months	F.	Four days' history. Pain in abdomen, drowsy. Peritoneum covered with pus. Temperature 108° F. before death	Streptococci in abdominal lymph	Death	*Streptococcus*
(?) 1917	M. B.	Six years	F.	Inguinal glands enlarged four days before admission. Pain in abdomen two days, vomiting one day. Temperature 102° F. on admission. Operation: Free fluid in abdomen with flakes of lymph on intestine. Suppurating gland in groin opened later	No organisms seen in, or grown from, blood-stained fluid received	Recovery	0
1918	No cases						
(?) 1919	E. H.	Nine years eleven months	F.	"Influenza" five weeks ago, with "pain in side" two weeks later. No vomiting or diarrhœa. Takes food well. On admission: No tenderness, rigidity, or pain. Abdomen full. Clinical diagnosis ? pneumococcal peritonitis		Recovery	—
(?) 1919	W. M.	Two years two months	F.	Diarrhœa and pain in abdomen, three weeks. Wasting. Temperature on admission 104° F. Post mortem: Free fluid in abdomen with strands of fibrin. No bacterial examination made, but diagnosed pneumococcal peritonitis		Death day after admission	—
1919	G. D.	Three years	F.	Abdomen, pain three weeks before admission. Diarrhœa two days. Vomiting two to three hours. Temperature 102° F. At operation: Large abscess cavity, intestine walled off. Two or three subsequent operations	Green odourless pus; pneumococci present in pus and grown on agar	Recovery	*Pneumococcus*

1920	No	cases					
1921	A. M.	Nine years	F.	Pain and vomiting forty-eight hours. *Nil* in history	Pneumococci present in pus and grown on agar	Death after operation	*Pneumococcus*
1921	K. K.	Nine years three months	F.	Pain one week. Vomiting three days. Vaginal discharge one week before pain. Operation: Turbid fluid in pelvis. Tubes congested	No organisms seen or grown, but gonococci had been found in vaginal discharge	Recovery	[*Gonococcus* probably]
1922	F. B.	One year eleven months	F.	Acute inflammatory arthritis of hip, with large abscess of thigh. Laparotomy. No free fluid; no pus. Post mortem: Flaky pus in abdominal cavity. Typical pneumococcal lymph. No pneumonia. Left hip-joint eroded.	Streptococci in pus from thigh; pus from hip-joint and peritoneal cavity: pneumococci and streptococci	Death	*Pneumococcus* and *Streptococcus*
1922	L. M. M.	Three years six months	F.	Vomiting and pain in abdomen. Temperature 103° F. on admission on second day of illness. Post mortem: Lymph all over bowel; also in pelvis and ? in tubes	Smears from tubes and peritoneum = pneumococci	Death	*Pneumococcus*
1922	L. B.	Nine years	F.	Three days' history. Vomiting all the time. At operation: Tumour of mesentery (cavernous angeioma). Removed with 3 in. of gut. Flakes of lymph in peritoneum. Red blood corpuscles in urine	Post mortem: *left-sided empyema*, abdomen full of pus. Pneumococci in pus and grown from heart-blood	Death on tenth day	*Pneumococcus*
1923	R. H.	One year four months	F.	Three days' history. Pain in abdomen. No vomiting. Bowels open. Temperature 101° F. on admission. Pus present at operation. Semi-solid, foul smelling. Anti-pneumococcus serum, and later anti-streptococcus serum given	*Bacillus coli communis* and *Streptococcus fæcalis* isolated (non-pathogenic for mouse). The same two organisms isolated from vaginal swab	Recovery	*Streptococcus* and *Bacillus coli communis*
1923	G. E. M.	Eight years ten months	F.	History six days. Started: Headache, pain in abdomen, vomiting and abdominal distension. Delirious on fourth day, passed flatus; very ill on admission (sixth day). Temperature 102° F., pulse 120, respiration 48. Dry friction in chest, abdomen very distended. At operation clear fluid obtained from peritoneal cavity. Few cells only	No organisms seen. Cultures sterile. Post mortem: scattered broncho-pneumonia. Brain congested. No peritonitis	Death after operation	0
1924	No	cases					

SUMMARY: *Pneumococcus* alone ... ... 5 cases
Pneumococcus and *Streptococcus* ... 2 ,,
Streptococcus alone ... ... 2 ,,
Streptococcus and *Bacillus coli communis* ... 1 case
No organisms found ... ... 3 cases
No bacteriological examination ... ... 2 ,,
Total—15 cases

solubility and animal inoculation tests were not carried out in every case. It is a fact, but one not sufficiently widely known or appreciated, that certain strains of the intestinal streptococci (*Streptococcus fæcalis* or enterococci) morphologically and in their action upon sugars, closely resemble pneumococci. They may even show a halo or capsule in films. If, therefore, we were to rely for our diagnosis upon morphology and the sugar reactions alone, cases in which these cocci were found would be labelled pneumococcal instead of streptococcal or enterococcal. The enterococci are not bile-soluble, are only feebly, if at all, pathogenic for mice, and are practically all heat-resistant, most strains surviving an exposure of thirty minutes to a temperature of 60°C.

One of the cases in my series is of particular interest in this connexion. It was that of a child aged 1 year 4 months who was admitted with a three days' history of pain in the abdomen; no vomiting; and temperature 101° F. The abdomen began to swell on the third day of the disease and on admission was very tense. No frequency or pain on micturition. The clinical diagnosis of pneumococcal peritonitis was made and the patient operated upon immediately after admission. Some thick, evil-smelling pus was removed and sent to the laboratory for investigation. Pending the investigation the child was given 50 c.c. anti-pneumococcic serum on the day of operation—November 10, also on November 11, 13, and 15, so convinced was the surgeon that it was a case of pneumococcal peritonitis. The organisms isolated, however, were *Bacillus coli communis* and *Enterococcus* or *Streptococcus fæcalis*. The latter was not pathogenic for a mouse. The same two organisms were isolated from the vagina. The child made a good recovery.

I think that possibly one or more of the earlier cases in this series, although I have set them down as pneumococcal, may have been enterococcal in nature, inasmuch as reliance was placed upon the morphology and the sugar reactions, which, as I have already said, are now known to be insufficient to diagnose pneumococci from enterococci. I venture, therefore, to express the opinion that primary pneumococcal peritonitis is not a common disease of London children, and moreover, that some of the cases considered to be pneumococcal in origin are really enterococcal or streptococcal.

We may now briefly consider how the infecting organism—whether pneumococcus or enterococcus—reaches the peritoneal cavity. This has been ably set out by McCartney and Fraser in the papers already referred to, and as the result of their clinical observations and of the experimental investigations of McCartney, these observers conclude that the infection reaches the peritoneum from the vagina by way of the genital tract. The clinical evidence they adduce in support of their hypothesis is based on the following observations: (1) The disease occurs only in quite young girls, usually between 3 and 7 years of age; (2) unlike pneumonia, infection is commoner in the summer months when the children are more likely to sit about and to wear fewer underclothes; (3) the girls belong to the lower classes, so that they are as a rule not well cared for and the perineal parts of their bodies are almost invariably dirty. This is a potent cause of vulvo-vaginitis, in which condition the vaginal secretion may be neutral or even alkaline in reaction instead of acid, as is normally the case; (4) virulent pneumococci may be isolated from the vaginal secretion of these children; (5) the disease occurs suddenly in healthy children, the first symptom being sub-umbilical pain; and (6) on laparotomy, it can be shown that the disease begins in the pelvis and in early cases is confined to the lower portion of the abdomen. The bacteriological evidence they adduce includes the following points: (1) Pneumococci, of the same type as that causing the peritonitis, were isolated from the vagina in all the cases examined; (2) at the autopsy, the peritonitis was the only lesion present; and (3) the disease was reproduced in a young monkey by vaginal inoculation with pneumococci, after the vaginal cavity had been rendered alkaline with dilute sodium carbonate. The organisms ascended the genital tract and did not give rise to any symptoms until they reached the peritoneal cavity.

The other possible routes by which the infecting organism might reach the peritoneum are: (1) the blood-stream, as first suggested by Michaut; and (2) the bowel, as suggested by Stoos. There are several weighty arguments against the hæmal route, one being that in many cases of pneumonia, pneumococci can be demonstrated in the blood, and yet peritonitis is a rare complication of pneumonia. Secondly, if hæmal in origin, why should the peritonitis begin in the pelvic region as McCartney and Fraser found so often to be the case? And, thirdly, why should the disease affect only girls? These last two objections would weigh equally against the intestinal route of the infection.

It has been suggested by some clinicians that, in addition to these severe cases of pneumococcal peritonitis, there may be milder cases of the disease in which there is recovery after a short, mild illness. In the absence of any operative measures and the opportunity of bacteriological diagnosis it is impossible to give a definite opinion upon this point. If such cases of mild illness, associated with abdominal pain and vomiting, are really attacks of peritonitis, I would suggest that possibly this might be enterococcal in origin rather than pneumococcal. I think this may be quite probable, since in this class of patient the vulvo-vaginitis is not at all an uncommon condition and in many hundreds of cases of vulvo-vaginitis which I have examined at the clinic at Great Ormond Street Hospital the large majority of patients showed an infection of the vagina with intestinal organisms, namely *Bacillus coli* and Gram-positive, short streptococci. Less than one quarter of the cases of vulvo-vaginitis in all the children that I have examined show the gonococcus, and only a very small proportion—and these occur in older girls—the diphtheroid bacillus.

To summarize therefore the main points of my remarks:—

(1) Acute primary pneumococcal peritonitis is a rare disease in London children.

(2) As it occurs solely in girls, I believe the infection travels from the vagina to the peritoneum by way of the genital tract.

(3) Some of these cases of acute peritonitis are due to the enterococcus and not to the pneumococcus.

(4) It is possible that there may be milder forms of the disease which quickly subside and are recovered from, and that these are more likely to be due to the enterococcus than the pneumococcus.

(5) The disease is commoner in girls of the poorer classes, as, owing to lack of cleanliness of person and underclothing, the condition of vulvo-vaginitis—of which intestinal organisms are the cause—is frequently present.

Dr. Hugh Thursfield (Chairman)

said that Dr. Cameron and the staff at Guy's Hospital appeared to see more cases of this disease than were seen in hospitals in the northern parts of London. It would be interesting to hear from gentlemen who were principally concerned with Northern London whether that was in fact the case.

He did not think he could remember seeing a case of what Dr. Cameron would call primary pneumococcal peritonitis in a child, though he had seen cases of the condition in adults. He had seen cases in which the diagnosis of peritonitis was very obscure, and in which the doubt was never resolved. He gathered from the remarks of Mr. Waugh that, at an operation, there could hardly be any doubt whether or not one was dealing with a case of pneumococcal peritonitis. In that case he could say that he, personally, during a good many years at Great Ormond Street Children's Hospital, had never seen a case of primary pneumococcal peritonitis.

Mr. R. A. Ramsay

said he felt rather surprised when Dr. Cameron laid such stress on the mistakes in diagnosis which might be made between pneumococcal peritonitis and typhoid fever or tuberculous peritonitis, because he had never himself seen one of these. The cases he, as a surgeon, saw which caused confusion were those in which the question was whether the disease was acute appendicitis or not. All the cases which came under his observation had been exceedingly acute. The onset had been very sudden; the patients had been taken into hospital usually within forty-eight hours of the onset, sometimes within twenty-four hours of it, and the peritonitis had been so acute and so little distinguishable from that due to a gangrenous appendix, that they had been operated upon at once. In only one of the cases of that nature that he had seen had there been a recovery. He was sorry he could not be as optimistic as Mr. Waugh: in his experience the mortality from the condition had been very heavy in the early and fulminating cases. Of these he had seen only three in about the last six years. Shortly before the war a paper was published in the *St. Bartholomew's Hospital Reports*, vol. xlviii, p. 137 *et seq.*, by Moreton and Noon, in which it was evident that the authors had difficulty in collecting six cases in seven years in that hospital. Therefore the figures for the north side of the river seemed to be fairly low.

An important point in the diagnosis at operation was the absence of smell in the effused fluid in the case of pneumococcal peritonitis. When there was no bad odour in the abdominal fluid, it showed that it was neither appendicular nor from the bowel, and if there was sufficient history to distinguish it from tuberculosis it made the diagnosis clearer.

One patient of his on whom laparotomy was done recovered. This particular case was diagnosed as appendicitis, and the lack of odour in the pus made him think it was pneumococcal. Pneumococcus was isolated and Type I pneumococcal serum was used intraperitoneally, and the child recovered. He had since heard of another, a fulminating case in which the patient was given the same serum, and also recovered.

With regard to residual abscess in the peritoneum, in his case in which the patient recovered, five weeks after operation, when the temperature was coming down slowly, a painless abscess developed in one forearm, two days later one in the upper arm, and four days later one on the buttock. These abscesses contained pneumococcus in pure culture. Directly the abscesses appeared there was an improvement in the child's condition and complete recovery followed.

He asked whether any members had had experience of treatment of this disease by serum and vaccines, or had had experience of distant metastatic abscesses.

Dr. E. Bellingham Smith

said he had an abstract of the notes of eight cases, seven of which had occurred at a children's hospital since 1920, and one at the Great Northern Hospital, which had been operated upon four years ago.

He agreed with what Dr. Cameron said about these cases, and distinctly disagreed with Mr. Waugh's statements. Seven of the eight cases were in children, one of whom was a boy, so that the condition did not occur exclusively in girls.

Dr. Nabarro had said it was possible to mistake the pneumococcus for the enterococcus: he (the speaker) asked whether it was possible to mistake the pus of enterococcus or streptococcus for the pus of the pneumococcus. In all the cases which he was describing on the present occasion, the pus appeared to have been absolutely typical of pneumococcal peritonitis; it was greenish, faintly sweet-smelling, not foul. That was not the case with the pus of the streptococcus; was it characteristic of that of the enterococcus?

The ages of the children of whom he was speaking ranged from 3 months to

8 years. One of the points he desired to stress in regard to these cases was that in all, the preliminary symptom was diarrhœa, something more than merely one or two loose stools. He regarded that as a pathognomonic sign of this disease. If there was generalized abdominal pain and rigidity with diarrhœa and toxic symptoms, such as drowsiness and a flushed or cyanosed face, high temperature and rapid respiration, it could be concluded that the case was one of pneumococcal peritonitis.

Mr. Waugh said that in pneumococcal peritonitis, after the first few hours following the primary invasion, the condition was well recognized by the absence of pain. In five of the cases to which he (the speaker) was referring, in which the patients lived respectively twenty-four hours, two, three, four and five days, there was acute abdominal pain and tenderness throughout the illness. All these cases were operated upon, three within twenty-four hours of the onset:—

With regard to treatment, of the three cases he saw which survived, one, he supposed, would be classed as residual; it was residual in the sense that it was made residual. Mr. Twistington Higgins operated upon the patient for him. The child was admitted in a toxic state, with abdominal rigidity, a high temperature, a pulse of 160, and respirations 40 to 50; at first, there was much doubt as to whether it was a case of double lobar pneumonia with a very distended abdomen, or whether it was acute pneumococcal peritonitis. The patient was placed in a sitting position in bed and treated on the lines of a pneumonic. The temperature remained up, exactly as in a case of typhoid, steadily high, round about 104° F. for three weeks, then it slowly came down. At the time the temperature began to come down, a dome-shaped swelling like a bladder was found in the pelvis. He asked Mr. Twistington Higgins to see the case, and he operated, and removed two or three pints of greenish pus, which was found to contain pneumococci. The second case was admitted with what was supposed to have been a three weeks' illness of pneumonia. On admission a large pneumococcal abscess was found occupying the whole of the right side of the abdominal cavity. The third case was exceptional in that the patient survived operation eight hours after the onset. This operation, however, in no way ameliorated the general symptoms or in any way affected the cause of the pyrexia, or the rate of the pulse and respiration. The illness ran a three weeks' course as in the first case and terminated favourably after jaundice. A ninth case, not previously mentioned, was of interest, as illustrating the dangers of surgical interference in the early stages. A boy, aged 5 years, admitted with general abdominal rigidity, tenderness and toxæmia of twenty-four hours' duration. On the fourth day there was a note that the child was much better; the temperature had fallen somewhat, the pulse-rate had dropped to 120, and the respirations to between 30 and 20. The note added that there was still tenderness in the right iliac fossa, and it was therefore decided to operate as appendicitis was suspected. At the operation general peritonitis with greenish pus and pneumococci were found, and a little later the child died. But for the operation he believed that this would have been a residual case, though it started as a typical, acute, pneumococcal peritonitis.

The chief point he wished to make was on the subject of treatment, and to say that the only chance for these acute cases was to sit the patient up in bed and to give support through the preliminary three or four weeks of the illness, hoping that subsequently a residual abscess would form. Early operation in most cases he believed would prove fatal. Where recovery took place after early operation, it was more than likely that the case would have reached the residual stage, with less risk without operation.

He did not think there should be so much difficulty about the diagnosis of these cases. He agreed it was a hazardous differentiation to have to make from acute appendicitis and peritonitis in the first instance, but the appearance of these children—sometimes collapsed, always toxic, and often having delirium—more nearly resembled that of acute pneumonia, also the onset was nearly always associated with diarrhœa. He was not prepared to accept the view that infection was entirely by the way of the Fallopian tubes. He considered that in primary pneumococcal peritonitis the bowel occupied the same position to the peritoneum as the lung to the pleura in pneumonia, and that many if not all these cases began as a pneumococcal enteritis.

Mr. C. Max Page

said he wished to comment on Dr. Bellingham Smith's remark that operation in these cases was tantamount to killing the patients. His own (the speaker's) experience did not conform to this view. He doubted if operation were beneficial in acute cases, and could not entirely agree with Mr. Waugh in considering that the prolonged use of a drainage tube was of any value.

It was important that the right line of treatment should be settled. He thought it wrong to say that operation should be limited to the late stages, when localized abscesses had formed. There were acute cases in which a diagnosis from appendicitis could only be made by those with special experience of the condition. In case of doubt he considered that an exploratory laparotomy should be undertaken. He had operated on about ten such cases, in three of which the patients had recovered, two with the formation of residual abscesses, the other without.

He did not think operation necessarily assisted the control of the inflammatory process, but its result appeared to justify its use as a diagnostic measure in certain cases.

Mr. E. C. Hughes

said he remained unconvinced as to the advisability of operating in cases of acute pneumococcal peritonitis in the early stages.

Dr. Cameron had referred to two cases—in one of which he (the speaker) had operated—in which there was œdema of the cæcal wall, and the patient died a few days after the operation. In another case he operated six times, finding on each occasion a large abscess, before the patient recovered. In the acute stage nothing was found, though the diagnosis of acute appendicitis had been confidently made in the first place. If he were able to diagnose acute pneumococcal peritonitis, he would not operate in the acute stage. But he could not feel that diagnostic certainty; he was afraid of overlooking acute appendicitis. He had recently seen, with Dr. Cameron, two cases of acute pneumococcal peritonitis. One of them they saw three or four weeks after the initial illness, and there was considerable expansion of the abdomen, with a swelling presenting at the umbilicus. He opened a large collection of pneumococcal pus under the umbilicus, which was shut off from the peritoneal cavity as no coils of intestine were seen.

The other case was that of a patient who, six weeks before, had had an acute illness of forty-eight hours' duration, of a very vague character. There was some loss of weight, peevishness, and loss of appetite, with general distension of the abdomen, which had not been noticed by anyone until Dr. Cameron saw the child. There was a reddening and hardening of the umbilicus, and the question was whether this was tuberculous or pneumococcal peritonitis. He explored the abdomen, under the impression that it was pneumococcal, and expected to find a good deal of pus. But on reaching the peritoneal cavity he came to encysted collections of pus, no pocket containing more than a drachm. They were convinced they were here dealing with a pneumococcal and not with a tuberculous case.

Mr. J. M. Pinkerton

(speaking of the relative incidence of pneumococcal peritonitis in North and South London) said that when he was resident in a small hospital in South London he saw three undoubted cases within two years, all girls. One, aged 14 years, was diagnosed as a case of appendicitis, and she presented the exact appearances which Mr. Waugh described when the abdomen was opened. She recovered after a second laparotomy some weeks later, for residual abscess. Two of the three cases were proved, by pathological examination, to be pneumococcal peritonitis. All three patients were operated upon, and all recovered.

Mr. J. A. Cairns Forsyth

said his experience of pneumococcal peritonitis had not been very great, but the clinical appearance of cases he had seen exactly followed the description given by Mr. Waugh. In no case that he saw was there any prodromal diarrhœa. The patients, with one exception, were little girls of about 8 to 10 years of age. They were seized with sudden pain, vomited once, in some instances twice, and they had not what could be called diarrhœa, nothing more than two or three stools in twenty-four hours. In all the cases he referred to, the patients were operated upon within forty-eight hours.

He believed, with Dr. Nabarro, that in little girls infection reached the abdomen by way of the genital tract.

The last case he operated upon, in July last, impressed that forcibly upon him. It was that of a girl aged 8, in good circumstances, who was suddenly taken ill at school, with abdominal pain and vomiting, and was taken home suffering great pain. He saw the child that evening, when she was flushed, had a high temperature, and was complaining of pain in the abdomen, but there was no distension. On palpation of the abdomen and examination by the rectum no tenderness was elicited. The next morning she seemed better. Later he saw her, when she was drowsy and delirious, but he could only detect a little rigidity on the right side over the appendix. He again examined *per rectum*, and there was no tenderness, but he operated on account of the delirium and toxæmia. He found the abdomen full of purulent fluid, the Fallopian tubes œdematous, crimson, and of the size of adult tubes. The appendix seemed normal but he took it away. The cæcum was normal, and he passed a drain down into Douglas's pouch, and the patient made a good recovery. He felt later that he ought to have removed the tubes, and the subsequent history supported that idea, as last November she was operated upon for double pyosalpinx.

He would diagnose acute pneumococcal peritonitis from the profound toxæmia and the absence of abdominal rigidity. This was absent in all the cases he had seen.

Dr. Robert Hutchison

said that after listening to Dr. Cameron and Mr. Waugh he felt he knew all about the subject, but subsequent speakers directly contradicted what had previously been said. Mr. Waugh laid down clearly the points of distinction between the pneumococcal and other forms of peritonitis, laying stress on the absence of pain, but Dr. Bellingham Smith described typical cases of peritonitis, proved to be pneumococcal, in which there *was* pain.

Then in regard to treatment, one speaker said one must never operate in the acute stage, as the result was invariably fatal; another said that operation was the only thing to do.

It seemed to him there was still considerable lack of unanimity as to the diagnosis, and, what was still more important, on the matter of treatment.

Dr. Cameron (in reply)

said that although pneumococcal peritonitis was often relatively painless, he thought that Mr. Waugh had somewhat exaggerated this feature. In the diagnosis he had been most helped by the almost invariable presence of delirium and by the sudden rise of temperature to a considerable height. These were symptoms unlikely to occur at the onset of appendicitis. He thought that help could sometimes be obtained in doubtful cases by the exploration of the abdomen and withdrawal of fluid by means of a capillary tube, as recently suggested in America. He did not find these cases so rare as had been suggested by certain speakers.

Dr. NABARRO (in reply)

said that the pus due to coli infection smelt badly, but that due to streptococcus might not have any objectionable odour.

In the cases at Great Ormond Street Children's Hospital initial diarrhœa had not been recorded as a symptom, though there had been looseness of the bowels. In most of the cases there, pain was the initial symptom, after which came vomiting, and then the bowels acted freely.

Section for the Study of Disease in Children.

President—Dr. H. C. Cameron.

Case of Cerebral Tumour.

By W. M. Feldman, M.D.

The features of the case, that of a girl, aged 11 years, were headache, vomiting, impairment of vision, and squint. There was some œdema of the discs and exudates on the retinæ. Paresis appeared in the right sixth and seventh nerves and nystagmus chiefly to the right. Wassermann reaction of blood positive, of cerebro-spinal fluid negative. Vision improved with Hg and KI, but later gait became unsteady and dysdiadochokinesis developed. It was suggested that the tumour was cerebellar.

Dr. W. G. Wyllie believed the tumour to be in the vermis, because of the slight and variable nystagmus and because other symptoms were distributed indiscriminately on both sides. He advised against attempted removal, since the medulla might swell up and cause death in a few hours.

Congenital Duodenal Stenosis.

By Eric Pritchard, M.D.

A male child, aged 7 weeks. Vomiting after every feed from birth, sometimes projectile and occasionally bile-stained. Basal systolic murmur. No peristalsis seen or tumour felt. No enlargement of stomach detected. In hospital vomiting did not follow every feed, and for one interval of four days was in abeyance.

X-ray Examination.—Hour-glass stomach and dilatation of first part of duodenum. Fatal collapse without operation.

Post Mortem.—Three-chambered heart from patency of interventricular septum. First part of duodenum dilated and hypertrophied, the dilatation ending at the level of the bile-papilla, the lower lip of which was prolonged into an encircling band causing partial obstruction.

Discussion.—Dr. J. M. Smellie thought the condition was not rare, and referred to five cases he had seen, and one other diagnosed as pyloric stenosis and apparently cured by operation. In another case not successfully operated upon it was found after death that the second part of the duodenum was buried in the head of the pancreas.

Dr. Vincent Coates spoke of an infant seen four days after birth, who had already been vomiting for three days. There was a dilated stomach, also tetany. Projectile vomiting, bile-stained. Partial duodenal obstruction was diagnosed, but since no peristalsis was seen and there was no vomiting for thirty-five minutes after a 2 oz. feed of water, a surgeon decided not to operate. The child died two days later after projectile vomiting. Post-mortem: gross dilatation of stomach, the first and part of second part of the duodenum being held up by fan-shaped membrane to under-surface of gall-bladder.

Dr. H. C. Cameron (President) knew of only two cases on record in which successful operation had been performed on very young infants, nor did he know of any successes among the few who had survived longer.

Male Twins, one of which is a Mongol.

By Bernard Myers, C.M.G., M.D.

Aged 4 years 9 months, one a typical Mongol, the other fairly normal but had convulsions twice. Mother aged 42. Previous birth 15 years before, the father of that child being former husband. Dr. Myers referred to a paper by Halbertsma, in the *American Journal of Diseases of Children*, 1923, giving a list of fifteen cases of Mongolism in one of twins of opposite sex and two cases of Mongolism of both twins of the same sex.

A Case of Amaurotic Family Idiocy.

By W. G. WYLLIE, M.D. (for Dr. HUGH THURSFIELD).

THE case was a typical one in a Jewish male child, aged 14 months. He was the only child, was blind, and showed the characteristic cherry-red maculæ, papery-white discs, general spasticity and amentia.

A Case of Renal Infantilism.

By A. MONCRIEFF (for Dr. HUGH THURSFIELD).

A GIRL, aged 8½ years, complaining of attacks of vomiting for past eighteen months. Double talipes equino-varus and right facial palsy of long standing. Weight 33½ lb. (usual for age 51 lb.), length 41 in. (usual for age 48½ in.). Heart slightly enlarged. Systolic blood-pressure 160 mm. Hg. Urine: Sp. gr. 1,002, albumin present, few blood-corpuscles, no casts. Urea concentration tests showed very deficient concentration. Blood urea, 107 mgm. per cent. Blood cholesterol 268 mgm. per cent. No changes in optic fundi. Small mass palpable in abdomen, (?) possibly left kidney.

Dr. H. C. CAMERON (President) referred to two cases which he had seen in which extreme dwarfing and the typical bony deformities had been accompanied by bilateral renal calculi.

Case of Myoclonus of Unverricht.

By W. G. WYLLIE, M.D. (for Dr. ROBERT HUTCHISON).

THE outstanding feature of this case is the frequent occurrence of myoclonic movements over almost the whole body synchronously, the upper lip being included in the movements, and the eyes blinking at the same time. The movements are much stronger at the proximal parts of the limbs. During most of the attacks the child remains conscious. In the more severe attacks, however, the head bends forwards and approaches the feet, and the eyes appear glazed and vacant, as in an attack of *petit mal*. The Wassermann reaction was negative.

The child was born at full term, was breast-fed, is backward mentally, and is making no efforts to speak or walk. He sat up at 6 months. The father is healthy, the mother very nervous; the child's maternal grandmother is demented with chronic epilepsy. The case, therefore, seems to belong to that group of cases of myoclonus associated with epilepsy, which was first described by Unverricht in 1891.

The pathology of one or two cases has been worked out by Clark and Prout in America, who found in the pyramidal cells of the third layer of the cortex degenerative changes that closely corresponded with the changes found in the cells of the second layer of the cortex in chronic epilepsy. They considered the two conditions closely associated.

Dr. J. M. SMELLIE read a paper on "Some Unusual Sequelæ of Tuberculous Tracheo-Bronchial Adenitis in Children," which will be published in full in the *British Journal of Children's Diseases*.

[*May* 22, 1925

The Principles of Serum Administration in Cerebro-spinal Fever and the Results of the Treatment.

By Dr. KSAWERY LEWKOWICZ.

(Professor of Children's Diseases, Jagellonian University, Cracow, Poland.)

IN an article published last year in the *Lancet*, I have already expounded a new conception of meningococcal infection in its form as cerebro-spinal fever. This disease must be regarded as a primary ventriculitis (encephalo-cœlitis or choroido-ependymitis), while the lesions of the meninges constitute only a secondary phenomenon. In this view, I then felt quite alone,

not knowing that, almost simultaneously, an article by Dopter, a well-known authority on the subject, had appeared in the *Paris Médical*. This author admits that hitherto we have been too much imbued with the idea that ependymitis is a complication of sub-arachnoiditis or meningitis, and considers that we cannot fail to be impressed by the probability of the new notion introduced into the pathogenic history of cerebro-spinal fever, according to which the exact contrary standpoint is taken.

But even though we are now in complete agreement with Dopter as regards the principal problem, in certain less essential questions a divergence of opinions still appears to remain. For instance, it has to be emphasized that, according to my investigations and contrary to the opinion of Dopter and French authors, even in a sub-arachnoid space blocked by adhesions, no autonomous meningitis can ever exist independently of the ventricular infection or require a serum treatment on its own account.

Thus, our therapeutic indications become very simple, at least in principle.

Disregarding sub-arachnoid or general infection, let us bring about a sufficiently intense serum action in the ventricles and maintain it uninterruptedly for several days, till complete disappearance of meningococci, verified by examination of the ventricular fluids, takes place.

We must not infer that the application of these principles necessarily implies a serum administration through the ventricular puncture in each case. On the contrary, as we shall see, antimeningococcal serum, injected intraspinally, penetrates into the ventricles in about four-fifths of the cases, in quite a sufficient concentration, and thus, only in about one-fifth of the cases, intraventricular administration becomes indispensable.

Let us now turn to the question of the results that can be obtained by different serum applications. As regards our own observations, it was not until the beginning of 1922 that our therapeutic practice in this respect was fixed, and the value of the injected serum as well as of its concentration in the ventricles during the treatment were systematically determined in each observation. It was also at that time that I made up my mind to begin the treatment, on principle, with intraspinal serum injections, in order to ascertain in what proportion of cases this therapy failed and should be replaced by the intraventricular application of serum.

Since that time I have had thirty-seven cases that have been treated by serum.

There were ten children in their first and one in its second year. As regards the former, the specific infectious process was overcome in six cases (i.e., in 60 per cent.), but, as in three of these cases the treatment was not begun until very late in the course of the disease, recovery was not complete, and secondary hydrocephalus persisted as a dangerous sequel.

Recovery from the specific infectious process was obtained in two cases by the intraventricular administration of serum carried out from the beginning of the illness, in two cases by intraspinal application; whilst in two other cases that at first had also been treated by intraspinal injections, intraventricular application was carried out later. But only in one of these cases has it been proved that serum, injected at the usual lumbar site, appeared in the ventricles merely in insignificant quantity (0·003 per cent., twenty-four hours after injection). The child's life was saved by intraventricular injections, and it is evident that it was only by the practice of this method of injection that recovery in this case became possible.

Among ten infants, in four cases each in the first year of life (i.e., in 40 per cent. of the cases) serum treatment failed completely. This negative result cannot be explained either by low therapeutic value of the serum administered, or by its being present, in the ventricles, in too low a concentration. Nor does the addition of complement (fresh guinea-pig serum) seem to influence the situation in any way. We are, therefore, compelled to assume that the cause of failure, e.g., a lack of the necessary collaboration—lies in the infected patient. Indeed, we meet with these negative results particularly often in children in their first year, in whom we are justified in supposing a special susceptibility to, or a lack of resistance against, the infectious process; the same not infrequently happens in children in their second year (our only case), but only quite exceptionally (146*)[1] in older individuals.

This point must be borne in mind, in order that we may not expect—as Dopter does—very much from the intraventricular injection of serum in the case of infants in their first year. Indeed, the mortality of these patients from cerebro-spinal fever is about 90 per cent. in the absence of serum treatment. It diminishes, however, under the intraspinal injection of serum to about 50 per cent., and, under the influence of serum injected intraventricularly, it is further reduced by 10 per cent. (one case out of our ten cases), the total mortality-rate thus falling to 40 per cent. The remaining cases comprising this 40 per cent., however, do

[1] The observations marked with asterisked numbers are, in my papers, illustrated with charts, representing all their essential clinical symptoms as well as the results of investigations.

not seem to be influenced at all by serum treatment, at least, in the present state of our resources.

The situation is very different in older individuals, for, in these cases, serum—so long as it is well adapted to the infecting strain of meningococcus, and present, in the ventricles, in adequate concentration—appears invariably to overcome the infection.

Of twenty-three cases of our patients over 2 years of age, two died. Both these cases, however, should be set aside, as one (151*) was of the fulminating type, and the whole course lasted only forty-one hours. In the other (146*), that of a boy aged 7 years, admitted on the fiftieth day of illness, there was extreme emaciation and advanced hydrocephalus that finally resulted in death, in spite of the meningococci having twice disappeared under the influence of serum administered intraventricularly, while intraspinal injections had no effect.

All the other cases (21) recovered completely; thus the gross mortality was 8·7 per cent., the corrected mortality 0 per cent.

Of 21 cases, recovery in two was obtained by intraventricular administration practised from the outset of treatment. Nineteen cases were treated by intraspinal injection, and in fourteen of these the effect was quite positive, while in five others I felt inclined to substitute the intraventricular application for the spinal. But only in two of these cases (145*, 152*) was it proved that serum, injected intraspinally, did not reach the ventricles in adequate concentration. The same thing also happened in both fatal cases (146*, 151*) and in one infant in its first year that recovered (178) out of four treated by spinal injections. Therefore in five cases altogether, out of twenty-five thus treated, i.e., in 20 per cent. of these cases, the fact of the insufficiency of the intraspinal administration was made quite evident. In addition to this, in four of these cases, i.e., in sixteen per cent. (145*, 146*, 152*, 178) the superiority of ventricular treatment has been proved.

The figure of 16 *per cent. thus corresponds to the further reduction of the mortality of cerebro-spinal fever that may be obtained if those cases in which intraspinal serum injections have failed, after having been practised for several days, are subsequently treated by intraventricular serum injections, and if, in severe cases, this latter method of therapy is instituted from the outset of treatment.*

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INSTRUMENT FOR PUNCTURE OF THE VENTRICLES OF THE BRAIN.

Shown by DR. KSAWERY LEWKOWICZ.

(Professor of Children's Diseases, Jagellonian University, Cracow, Poland.)

PROFESSOR LEWKOWICZ demonstrated an instrument he had had specially made to puncture the ventricles of the brain, remarking that by its aid the puncture was easily carried out, and there was no need for either general or local anæsthesia. Often the patient declared that less pain was caused by this procedure than by lumbar puncture. The needle could then be quickly introduced into the bore-hole and the injection made. He added that he had demonstrated the method in Strassburg, in Paris, in Montpellier and Lyons, and all who had seen the operation performed agreed that it was quite simple.

Dr. WORSTER-DROUGHT said that hitherto it had been customary to consider the meninges as the site of election of the meningococcus and that after the organisms were absorbed from the nasopharynx they were carried by the blood-stream to these membranes. Professor Lewkowicz, however, in his interesting paper, contended that the primary lesion in the brain was an inflammation of the ependyma, and that the actual meningitis was merely a secondary event. Professor Lewkowicz had converted him (the speaker) out of his own mouth. He (Dr. Worster-Drought) had described certain fulminating cases of cerebro-spinal fever in which, ante mortem, the meningococcus had been isolated from the blood-stream, but on lumbar puncture the cerebro-spinal fluid had shown no evidence of meningitis; post mortem, however, there had been found a small amount of turbid fluid, also with inflammatory changes in the ventricles, although the meninges were free from purulent exudate. At the time, he (Dr. Worster-Drought) had not attributed the true significance to this observation. He, therefore, had no doubt the Professor was correct in his view that the leptomeninges were only secondarily affected.

With due regard to this view of the pathology of the disease, Professor Lewkowicz's main method of treatment was to attack the primary intracerebral lesion, i.e., to inject serum intraventricularly instead of intraspinally. The instrument shown by the Professor for drilling the skull was particularly interesting as he (the speaker) had done the operation only in cases of hydrocephalus by drilling a hole with an ordinary bone-drill at Keen's point—3 cm. above the external auditory meatus—and then puncturing the ventricle with a lumbar puncture needle. He agreed that there was practically no shock associated with this operation, and he considered that the instrument now shown was admirable for the purpose of ventricular puncture.

Section for the Study of Disease in Children.

President—Dr. H. C. Cameron.

DISCUSSION: IS THE MODIFICATION OF COW'S MILK NECESSARY IN INFANT FEEDING?

Dr. Leonard Findlay (Glasgow).

At the outset I should state that in my opinion undiluted cow's milk is not only a suitable food for the generality of infants but is perhaps the best substitute for human milk.

This does not of course mean that I believe in giving unmodified cow's milk. But I am not sure that the modification which I consider essential, is one that entered chiefly into the thoughts of those who suggested the subject for discussion. I feel very strongly that under no circumstances should raw cow's milk be given to the infant, or indeed to any child under five years of age. The milk should invariably be sterilized by boiling or pasteurization.

Cow's milk is more or less, and usually more, contaminated with pathogenic organisms, and in this way it differs very strikingly from human milk. Hence the only way to make it a safe diet for the infant is by sterilization.

I know it is argued—and indeed I have argued in a similar fashion myself—that to recommend such treatment is to divert attention from the ideal mode of attack, namely, the supply of a pure milk. Although the chance of contamination by the tubercle bacillus can be considerably diminished its complete eradication is, however, apparently impossible. Last summer, Dr. Park, of New York, informed me that in the case of the last six examples of bovine tuberculosis which he had investigated all the infants had been fed on Grade A milk obtained from a well-known model dairy.

Even were it possible to get an absolutely clean and tubercle-free milk from the cow, there are so many opportunities for contamination in its passage from the source to the consumer, that we cannot be sure that the child is ingesting a clean food. One has only to consider the question of the milkers and dairy workers suffering from ambulatory enteric fever, or dysentery or unrecognized scarlet fever, to appreciate the force of this argument. Then we must take into account the conditions which prevail in the ordinary household. Further, the economic aspect of the question must not be forgotten, for it may be so expensive to obtain what we consider the ideal as to make it prohibitive for any but the wealthiest classes. By all means let us have milk as clean as possible for our infants, but let us remember that a stage is reached along these lines when further refinements, at least, are economically unsound.

In addition to making the milk safe for human consumption, boiling has the further advantage that it makes it more digestible. Boiled milk curdles less readily and less coarsely than raw milk, and thus one of the undoubted disadvantages of cow's milk is removed. Dr. Janet Lane-Claypon, in a comparative study of feeding with raw and boiled cow's milk, showed that, if anything, the advantage lay with boiled milk. It is possible that boiling may somewhat diminish the nutritive value of milk, but the chief danger of sterilization lies in its tendency to destroy the anti-scorbutic substance; this, however, is a disadvantage which is easily overcome.

With this proviso, then, I believe that cow's milk need not be further modified for use by the average infant. There are of course certain pathological states in which modification is necessary, but these I consider are outside the present problem.

I propose to discuss the question of modification under three heads.

(1) *The Difference between Cow's Milk and Human Milk.*

There are, undoubtedly, radical differences between these two kinds of milk, and at first sight there might seem sufficient reason for the idea that modification of cow's milk is necessary before it can be suitable for the infant's needs. But what are the differences which make one so suitable for the infant's needs and the other liable to cause gastro-intestinal disturbances? Unless we know the causes of the different reactions to these kinds of milk intelligent modification is obviously out of the question.

All the various constituents of the milk have been blamed in turn; first the protein, then the fat, then the sugar, and finally the salts. Although the fact of the greater amount of protein

in cow's milk, and the fact of this protein being chiefly in the form of caseinogen, suggests that dilution is necessary, the difference is a more subtle one than merely one of quantity. It should be remembered that protein is really effective as such by virtue of the amino-acids of which it is composed, and that some of these, especially tryptophane and lysin, are absolutely essential. Now as the lactalbumin in 100 c.c. of cow's milk only amounts to 0·5 grm. compared with 1·0 grm. in an equal quantity of human milk, and since of the two, human lactalbumin is much richer in leucin, lysin and the absolutely essential tryptophane, any modification by dilution is a serious matter. The smaller amount of iron in cow's milk with the tendency to the development of anæmia on prolonged milk feeding must also be borne in mind.

There is definite proof that the practice of simple dilution of cow's milk in order to make the various constituents approximate in amount to that found in human milk is wrong. Many workers have shown how widely varied are the proportions of the different amino-acids in different proteins, and how this variation influences the minimal amount of protein upon which nitrogen equilibrium can be obtained. From such experiments tables have been compiled showing the relative values of different proteins: for example, if the amount of human protein necessary be represented by 100, caseinogen of cow's milk will be represented by 128 and vegetable protein by 163.

I do not know whether the same rule holds good for fat and carbohydrate but it seems to hold good for calcium, at least the calcium of human milk is better retained both proportionately and absolutely than that of cow's milk. This probably results, however, rather from the medium in which it is suspended than from any difference in chemical composition.

The whole question of the difference between cow's milk and human milk received considerable illumination from the whey-exchange experiments of Ludwig Meyer. After interchanging the whey and curd of the two milks and carrying out feeding experiments on normal children, and also on children recovering from enteritis, Meyer found that the increase in weight and the character of the motions were invariably better with mixtures containing human whey, whether this was combined with cow's milk curd or with human milk curd. He therefore concluded that the different effect of the milks was due to some difference in the whey. He obtained similar results even when the lactalbumin was removed from the whey, and thus the search for the difference between the two milks seemed to be narrowed down to the sugar and the salts. Now, since the different effect of the cow's milk is possibly due to irritation of the intestinal mucosa, it seemed most rational to incriminate the salts, as these are $3\frac{1}{2}$ times more abundant in cow's milk than in human milk. The work of Jacques Loeb lent considerable support to this hypothesis. It was on this principle that the idea of "Eiweiss Milch," with its diminished whey content, originated. However, an artificial whey with the salts in the proper concentration and obtained from ashed human whey was anything but satisfactory, so that, whatever the importance of the salts, there seemed to be necessary some organic factor which had been destroyed in the process of ashing.

But although we do not know the real cause of the difference between cow's milk and human milk, we know enough of the chemical composition of both to realize that the difference is so fundamental that the rational modification of cow's milk is impossible; in fact the humanization of cow's milk is a mere figment of the imagination, The protein itself defies transformation. It would require to be broken up into its constituent parts, as in digestion, and then to be rebuilt as it is by the human tissues before we could speak of modifying it so that it would simulate human milk; and even then the protein supplied would need to be perfect. Dudley and Woodman[1] have shown that after racemization of different proteins (sheep and cow caseinogen) optical activity reveals fundamental differences in the intramolecular structure of some of the amino-acids, especially in the case of lysin and glutaminic acid, and they conclude that each species of protein is specific.

Thus it would seem that any rational modification of cow's milk is impossible.

(2) *The Available Scientific Proof that the Child is Unable to Digest and Absorb Cow's Milk.*

The pathology of nutritional diseases in infancy is in a most unsatisfactory state. Many theories have been suggested to account for malnutrition, and the amount of chemical and metabolic investigation conducted to elucidate the problem is immense. From a study of the different systems of nutritional disease suggested I can but conclude either that diseases

[1] *Biochemical Journal*, 1915, lx, p. 98.

vary in their type in different parts of the world, or that the Glasgow babies are formed on a special plan. In short, the systems do not work. Clinically at least, they do not fit in with the reality as I see it in Glasgow.

Though I myself have not done any work on metabolism, I have had associated with me at the Royal Hospital for Sick Children, Glasgow, Dr. Fleming, Dr. Hutchison, and Dr. Muriel Brown, who have all made serious investigations in this field. The problem which we set ourselves to study was whether badly nourished infants could digest, absorb and metabolize the proximate principles of cow's milk as well as the normally thriving infant does. Balance experiments were undertaken and the respiratory exchange was studied. The study of the behaviour of protein and fat was straightforward, as either the substances themselves or their end products could be isolated and estimated; but in the case of carbohydrate the question was quite different, because owing to fermentation in the intestine its end products (CO_2 and H_2O) were imponderable. Dr. Muriel Brown, however, attempted a solution of this aspect of the subject by studying the fasting sugar level and the blood-sugar curve, after the ingestion of glucose in different diseases, and with different diets. With the exception of a low, fasting blood-sugar level in the presence of vomiting there was no evidence that the marantic or non-thriving infant was less well able than his well-nourished or normal brother to deal with these food-stuffs.

One point of significance, however, did emerge from the experiments and supported our clinical experience with undiluted milk feeding. Dr. Hutchison found that as the marantic infant improved while on a constant protein intake the daily retention of nitrogen diminished, a fact which seemed to indicate a previous state of starvation; this did not refer of course to children with vomiting and diarrhœa. The dietary histories of many of these children showed pretty conclusively that they had been starved, and recovery not infrequently resulted from administering food presenting the requisite or proper amounts of nourishment. In fact, so far as our hospital material is concerned, most cases of malnutrition could be divided into two classes, (1) those in which the malnutrition was due to inanition, and (2) those in which it was due to enteritis, possibly of an infective nature. It is true that occasionally in private practice one sees a child in whose case the underlying cause seems to be an inability to absorb fat, since diminution of the fat intake improves the state of nutrition, but I have never had an opportunity of undertaking a balance experiment to test this hypothesis.

The points in the problem requiring solution are of course numberless, and we do not pretend to have touched more than a mere fringe of the subject, so that any conclusions which I am inclined to draw are only tentative. Nevertheless there does not seem to be any evidence that the poorly-nourished child, in the absence of enteritis, is marantic because he is not able to deal efficiently with the various proximate principles as presented in cow's milk.

(3) *The Results of Clinical Experience.*

Finally, as against the need for modification, other than stated in my introductory remarks, is the fact that the majority of children digest and thrive quite well on undiluted cow's milk. I am not here speaking of cases of enteritis but only of children who suffer neither from vomiting nor from diarrhœa. It must be remembered, however, that vomiting and diarrhœa, or, at least, frequent and small loose stools with much mucus, may occur in cases of inanition, and may be mistaken for the result of gastro-intestinal irritation.

I have tried diluted milk with sugar, and with or without the addition of cream, milk acidified either by fermentation or by the addition of lactic acid, and peptonized milk, but I have always returned to the use of undiluted cow's milk as it has given me the best results. I have also employed extensively citrated milk but I cannot convince myself that superior results are obtained in this way.

Such, then, are the reasons for my faith in unmodified cow's milk as a suitable food for the generality of infants. I do not wish it to be understood for one moment that I would administer unmodified cow's milk no matter what the condition of the child might be. For instance, in the presence of enteritis or in the case of very young infants, that is, during the first three weeks of life, I am a firm believer in "Eiweiss Milch," which, I think, owes its beneficial effect to its small carbohydrate content, its high protein content, and probably, to a great extent, to its diminished salt content. In this type of milk the nutritive elements are apparently suspended in the most suitable medium, and there results, in consequence, the least irritation to the intestinal mucosa. But as the child ages these structures become more resistant to deleterious agents and modification of the milk is less and less called for.

Dr. ERIC PRITCHARD.

I should like, before I commence to advocate my particular thesis, to congratulate Dr. Findlay on the very excellent defence he has put up on his particular belief. Before I proceed to give you what I have to say in favour of modification, I would remark, however, that I think he has rather shelved the question, for, as far as I understand from him, he has regarded the modification of milk as a mere question of dilution. You will see, from what I have to say, that this is not my idea of the means we have of modifying milk so as to make it approximate more closely to the human standard.

The main argument, in my opinion, for the modification of cow's milk in infant-feeding is that breast-milk has proved itself on biological grounds to be, if not perfect, at least a highly suitable food for this purpose; a fact which is implicit in the survival of the human race through some very trying experiences in its history. Cow's milk, which has had the same success in preserving the bovine species, has an entirely different balance as far as its main proximate principles are concerned; a balance which, presumably, is designed to conform to the particular metabolic requirements of the calf. If balance has no nutritional significance, then all I can say is that the whole science of dietetics is in error.

The scientific knowledge which has accumulated so rapidly during recent years on matters relating to nutrition proves more and more conclusively that for every individual animal there is an optimum requirement for a very large number of food elements, and that this optimum is not a constant one, but one that varies with a large number of fluctuating conditions in the habits and environment of the individual concerned. The public is looking to the medical profession for light and leading on these difficult problems; are we to tell them that these difficulties have no existence and that as far as the infant is concerned the problem has been satisfactorily solved by the cow itself?

The defects of cow's milk as a substitute for breast-milk were recognized long ago and many crude attempts were made, even before the end of the last century, to adjust certain of these differences, but these efforts were mainly confined to adjustments of the balance between the three main food constituents—the proteins, the carbohydrates and the fats. The American, or more properly speaking, the Boston system of percentage feeding, with its meticulous refinements, reduced this method to an absurdity. To secure certain minute adjustments in the percentages of these three elements in milk-dilutions the quantitative representation of the remaining twenty-seven elements in milk, many of which are not a whit less important, was completely ignored and sacrificed.

In cow's milk we know that there is at least five times as much caseinogen as there is in breast-milk, a discrepancy which makes the former without modification most unsuitable as a diet for infants. In accordance with Liebig's "law of the minimum," the nutritive value of a food must be measured by its greatest defect, and not by the special properties of a few of its constituent elements. The value of a diet, otherwise perfect, may be entirely vitiated by the absence or deficiency of any one single essential element, such as iron, phosphorus, iodine, lecithin, cholesterol, vitamin, or possibly any one of some thirty other independent constituents of which milk is compounded. And so close is the interdependence of certain of these elements that mere excess of one may render an otherwise correct representation of other elements relatively subnormal. For instance, the optimum calcium balance is dependent, among other factors, on the phosphorus content of the food. Excess of carbohydrate or farinaceous food may create a demand for the water-soluble B factor; whilst acid qualities in any particular element in a dietary may create greater demands for alkalies and so on. In fact, the more we learn about nutritional requirements the more complicated becomes the problem of finding the optimum balance, not only of one or two of the more important constituents of a dietary, but of all.

The newer knowledge of nutrition teaches us that many of the time-honoured shibboleths and doctrinaire conceptions of the older dieteticists must be "scrapped," and that we must review the whole situation from an entirely new standpoint. The old, simple laws based on the conservation of energy, though still holding good as far as they go, do not cover the whole ground, even as far as quantity is concerned. We now recognize that there are a great number of biological factors—including the stimulating influences of hormones, vitamins and other imponderable molecular units—which must be taken into account in estimating the availability of any combination of food constituents and their optimum quantity under given circumstances. So far we have not yet discovered in the domain of dietetics a Charles Darwin or a Herbert Spencer to piece together in one comprehensive fabric the innumerable fragments of knowledge which are turned out in such profusion from the humming laboratories of science

in all parts of the world. But this is surely no time to fold our hands and delude ourselves with the comforting assurance, that one of the most complicated and kaleidoscopic problems in the whole realm of medicine can be solved by the simple formula of cow's milk undiluted and unmodified.

In undiluted cow's milk there is a great excess of caseinogen; on the other hand there is no excess of the whey proteins, which are probably of greater biological value; there is no excess of many of the most important of the mineral constituents including iron, indeed, there is usually a deficiency; there is no excess of any of the vitamins, nor of lecithin, nor of cholesterol, nor of many other proximate principles of equal importance. So that, although cow's milk stands in need of dilution from the one point of view, there are very urgent reasons why it cannot stand dilution from a number of others. These are not arguments in favour of the non-dilution of milk, but they call loudly for more scientific methods of modification than are usually employed; for instance, in the "split-protein" method of modification, the protein excess can be reduced to the standard of breast-milk without any dilution of the remaining elements.

Although the literature teems with controversial opinions as to the precise optimum representation of protein in the diet of adults, no one ventures to challenge the suitability of the protein ratio in breast-milk for infant feeding. In breast-milk the ratio of protein to the non-protein elements is 1'5 to 10'5, or 1 to 7. In cow's milk the ratio is 4 to 8, or 1 to 2; a huge discrepancy. It has been claimed, however, by the "whole-milk" school that the protein (caseinogen) of cow's milk is so much more indigestible than that of breast milk that only a small fraction is available for nutritional purposes, and therefore that a much larger quantity should be provided. This is a strange argument. There is something to be said for only giving a small quantity of an indigestible food or for not giving it at all, but nothing for giving it in excess. Emmett Holt and others have contended that the biological value of the caseinogen of cow's milk is so inferior to that of breast-milk that to secure an equivalent effect on growth there is need for four times as much of the former as would be necessary in the case of the latter. There is no clinical or experimental evidence with which I am acquainted in support of this view: splendid results can be obtained with modified cow's milk in which the protein representation is not higher than 1'7 per cent., but yet this statement of Holt has crept into our text-books and has been accepted by many as a dietetic axiom.

I cannot, on this occasion, refer to all the disastrous consequences on nutrition which result from excessive protein feeding in the case of the infant: I would refer only to some of the more important. In the first place, protein is, in a physiological sense, a very expensive source of energy, partly because it never yields up its full quota of energy value, and partly because its combustion in metabolism involves strains on the digestive organs, on the liver in the process of de-amination, on the kidneys in the elimination of its final products, and on the blood owing to the presence of amino-acids and other intermediate products of combustion. A certain amount of protein is necessary for: (1) growth, (2) repair of tissue, (3) elaboration of secretions, and (4) formation of antibodies; any quantity given over and above that required for these purposes is detrimental to the interests of nutrition. The degree of tolerance of the human organism to excess of all elements, including proteins, is so great that many of the disastrous consequences which follow excess escape notice. But the effects of protein excess are cumulative and usually show themselves finally in the breakdown of liver function, in ammoniacal urine and other symptoms of acidosis.

As a rule, the stomach is the first organ to rebel against excess of cow's milk caseinogen, and to avoid the symptoms connected with its clotting in this sensitive organ citrate of sodium is added frequently to the milk. The consequence is that peptic digestion is short-circuited and the onus of digestion falls on the intestinal tract. If the stomach is able to deal with the excess it means that its functional capacity to provide HCl is over-developed. Dr. Leslie J. Harris has estimated that five times as much of this acid must be secreted to digest the caseinogen in whole milk as is required for the digestion of an equivalent of breast-milk. The resulting hyperchlorhydria brings in its train many troubles which are easy to recognize after a milk diet has been relinquished.

The physician who pins his faith to the panacea of whole milk for all conditions of health and disease is manifestly at great disadvantage as compared with one who is constantly exercising his ingenuity to find the optimum modification. I admit the difficulties of scientifically adapting the food to the existing nutritional needs, either in the case of the sick or of the healthy child, but the only possible way to improve our results is to press into the

service of our everyday practice the rich contributions afforded by experiments in the biochemical laboratory. I maintain that those who constantly practise scientific modifications of cow's milk when dealing with healthy children are more resourceful in the modification of cow's milk in the more difficult cases of sick children; and it seems to me that in nearly all cases of ailing infants you find intolerance for some element or another, a condition which has usually been brought about by previous excess of the said element. If you confine yourself to the flat method of treating all these children with whole milk, you are not in the same position to deal with the situation as are those who are familiar with all the applications of modification.

Dr. HUGH THURSFIELD.

When I looked at the subject as laid down for discussion this evening I was puzzled, because I suppose there is nobody in this room who is prepared to advocate the use of entirely unmodified cow's milk for any infant. And it is a little difficult to know how to approach the matter. Dr. Findlay and Dr. Eric Pritchard have covered the ground so completely from their points of view that there is not very much left for me to say, except, if I can, to bring the matter back to the more clinical aspect.

Dr. Pritchard pointed out that the medical profession were constantly being asked for an answer to the question: which, in our present knowledge of the science of dietetics, is the proper food for an infant, that is to say, the optimum food? I suppose the only answer one can give at the present moment is, breast milk. And if you come to inquire what breast milk is, you are confronted by the most extraordinary diversity of opinion. Some of you will have seen an excellent article in a recent number of the *Practitioner*, reviewing the whole subject of the analysis of breast milks, and it will be obvious to you that breast milk is anything but uniform; there is no sort of standard by which you can possibly measure or attempt to prescribe the composition of a food as set out by the average breast milk. The average differs so enormously from the extremes, not merely in individual women, but in the same individual from day to day. The composition, merely in the broad elements of fats, proteins and carbohydrates, may differ by as much as from 60 per cent. to 70 per cent. from hour to hour and from day to day, and even, in some cases which have been carefully examined, during the course of the same feed. If that is so, if we have no standard, it becomes extremely difficult to answer the question which the public is asking us: what is the best substitute for breast milk? I agree with Dr. Findlay that to answer "We must try to produce humanized milk" is a failure, for the reason that we do not know what human milk is, for the particular individual case. And, as a matter of fact—it must be familiar to all of you—humanized milk, as manufactured first of all in hospitals and then by manufacturers, has not been a brilliant success.

When you come to inquire as to the next step, you may ask what are the foods on which healthy infants have been brought up. I went into the surgical wards of the Hospital for Sick Children, where a considerable number of children are admitted for minor operations—cosmetic operations, hernias, cleft palate, cleft lip, nævi, orthopædic operations, &c., and I asked the Sisters what was their rule as to feeds. I found that they had only one rule, that is, feed the child on the food that it has been having. That meant that the child was fed on all sorts of impossible foods from our medical point of view, and that, so far from being bad specimens of childhood, they were extremely healthy specimens. Mr. Higgins will bear me out in that statement, for he will not operate on a child who is not in decent "fettle"; he does not care to give a child an anæsthetic for cleft lip unless it is reasonably well fed.

And that brings one back to the fact which will be supported by general practitioners. We are rather apt to think it is necessary to feed children on certain definite lines—those laid down by biochemical theory. The general practitioner knows that the great British public does not do it, and so far from the children suffering, many of them thrive on it.

So we arrive at this position: that, provided you have a healthy infant, it does not matter very much what food you give it, so long as it gets enough of it, and that it will discard, in various ways, the excess, or the substances which it does not want for the building up of its body. And so, although I should prefer not to feed an infant on a patent food, if I find a child has been fed on that food and is healthy, and I cannot detect in that child any particular aberration from a normal healthy child's progress, I do not feel very much concerned to alter the food to something which I consider, on purely hypothetical grounds, to be an immense improvement. But if the question is put to me: "What am I to feed my child on?" if it cannot take breast milk, I answer, "cow's milk, boiled," for much the same reasons as Dr. Findlay has given.

On the whole, cow's milk boiled, protected by antiscorbutic, is usually enjoyed by healthy children, and it is digested far better than most of the other infant foods. A healthy child thrives extremely well on cow's milk. I think I should hesitate to give whole cow's milk to a three weeks' old infant, though I have done it with great success.

But the case is different when you are called in—as we London hospital physicians dealing with children usually are—to advise as to sick children whose digestion is already disturbed. Then if you ask me whether we are to modify cow's milk, I should say "Yes, you must." You have to try to discover the particular fault of digestion in that child, and modify your cow-milk feeding accordingly. That answer, however, will not satisfy the British public. It is not definite enough, for one thing, and it is not sufficiently routine. Apparently they think—I am not sure that Dr. Pritchard does not think—that there is an optimum, which can be ascertained and laid down in black and white. I do not share that view. There may be—there must be—an optimum for each individual; but in our present knowledge I should say "We cannot tell you." For the masses there is only a mean, and that mean is best obtained for a sick infant by modification of cow's milk, and for a healthy infant, by boiled cow's milk.

When you come to ask why the sick infant, or the infant who is not thriving, cannot digest whole cow's milk and does not thrive on it, the question becomes extremely complicated. From purely clinical experience, not from experimental work, I become every year more and more convinced that the great majority of digestive troubles of children are due to excess of fat, and that is true not only of the artificially-fed infant, but of the breast-fed infant. If you modify the breast-feeding of a woman whose infant is suffering from digestive disturbances so that the infant will get considerably less fat in the milk, it is striking to note that the troubles will largely disappear. And, in the same way, if a child is being artificially fed on a food which is rich in fat, by reducing the fat in the food most of the beginnings of digestive trouble will disappear.

The case of the marasmic infant is different, and causes difficulty. In feeding such children lately, I have practically been compelled to give up the use of any sort of cow's milk. When they come into hospital they are put on some kind of food, and then the time comes when you think they can digest some better food and they begin to put on weight. You put them on to some modification of cow's milk, and, almost at once, the temperature goes up; they are attacked with diarrhœa and vomiting, and we have to begin all over again. So long as you withhold fat from those marasmic infants, and give them a fair amount of protein and an excess amount of carbohydrate—judged scientifically—they do fairly well. But in the majority of cases they do not really make any progress until they reach the age of three or four months. At that age they appear to acquire the capacity of dealing with fat and absorbing it—a capacity which they did not possess earlier. If you can diet them over that early period, you can begin on whole cow's milk, unmodified otherwise than by boiling; and it will be found that they do very well.

This is not a scientific contribution to the discussion, but it is an attempt to set forth what has so far been my experience in infant feeding, though I admit that, on scientific grounds, you cannot justify a good deal of our practice at present. I can only re-state the fact mentioned at the outset, namely, that science has not yet provided us with anything like a standard capable of being laid down as a working basis in this matter.

Dr. R. C. JEWESBURY.

May I venture to criticize some of the remarks of previous speakers? Dr. Findlay discussed the question as to whether milk should be boiled, or not; I think most of us are agreed on that point. All cow's milk given to babies should be boiled, and the boiling is part of the necessary act of modification. He referred to "humanized milk" as a thoroughly unsatisfactory term, for there is no such thing as humanized milk. I agree with him and others that our attempt to modify cow's milk is not a perfect one, because we are very ignorant of the obscure chemistry of milk. But, at any rate, surely we can try to get somewhere near it. It is only reasonable, when we know the percentages of sugar, of fat and of protein—though there may be different kinds of sugar, and fat and protein—to modify the cow's milk in order to bring it nearer to human percentages. I agree with Dr. Findlay that there must be something strange about the constitution of Glasgow babies; they do not seem to respond to the dietetic treatment in the way that London babies would, so far as I know them. He surprised me very much when he said he found that the marasmic child is able to deal with and thrive on unmodified milk. I have found that the modification of milk is especially necessary in cases of infants under weight, and suffering from any digestive upset.

I am very much in agreement with Dr. Pritchard's views, but I confess to great ignorance

in not knowing what are all his thirty elements in breast milk. I agree particularly with what he said about protein in cow's milk.

Dr. Thursfield said we could take no standard for breast milk, and he pointed out that great variation occurred in the breast milk, not only of different women, but in the milk of the same woman. I would ask Dr. Thursfield how his samples of breast milk were taken. It is very difficult to get satisfactory samples of breast milk for analysis; it means that both breasts must be emptied completely—and it is difficult to do that artificially—for the whole twenty-four hours. My experience is that if the samples have been properly taken, there will be found to be very little variation in the composition of the breast milk, both in the same person, and in different individuals. The variation is greatest in regard to fat, and I have come across a certain number of mothers who have complained of their babies being upset. On examination of the breast milk it has been found that the fat content has been raised. The highest fat content in my experience has been 7·5 per cent. By adjustment of the mother's diet that difficulty has been overcome. Temporarily, in those cases, it has been necessary to take the baby off the breast and to express the breast milk and set it, the fat being removed from it before it is given to the baby.

Dr. H. C. Cameron (President)

said that the discussion had shown how divergent were the views of practitioners and teachers upon this subject. It was clear that in infant feeding there were two schools of thought. There were those who, starting with a study of the differences in composition of breast and cow's milk, were in hopes of constructing a diet so like that of the natural diet that it shared with it many or most of its advantages. To those observers the ill results of feeding with undiluted or diluted cow's milk, without approximation to the percentages found in breast milk, were manifest. Others regarded the results of feeding with whole milk, or with diluted milk and added sugar, as satisfactory, except only in a certain minority of infants who reacted badly. For these, special modifications were necessary, and those modifications might take a direction which led the resulting mixture far away from the standards of breast milk, provided only that they achieved the intended purpose of controlling the symptoms.

Personally, he did not find that "humanization" of milk diminished the small percentage in which feeding with a simple cow's milk mixture produced disturbance, nor did he think that "humanization" was often successful in allaying a disturbance of digestion produced, in the first instance, by such simple mixtures. Apparent success with any dietetic therapy was often due to the natural cessation of a disturbance secondary to some obscure but transient infection or to the quieting of the restless infant by good nursing in hospital or in the home. No one diet could confer health on all ailing babies and he was doubtful of the power of any physician to produce super-states of health. There were those who spoke and wrote as if they were able to recognize in after-life those children who had been reared according to their dietetic prescriptions, by reason of their superior physique and brighter intelligence.

In practice, complete recovery to a perfect state of nutrition in later childhood, after a most unsatisfactory infancy, was common enough, and the damage wrought by even profound malnutrition was often temporary. The causes of recovery seemed to him (the President) often to be as obscure as the causes of the original disturbance.

As to the diet for a healthy infant, he felt that the results given by the simpler formulæ of whole milk or diluted milk, with the addition of sugar, cod-liver oil and fruit juice were no worse than those given by more elaborate modifications to breast standard. He regarded such a diet as a "sighting shot," which, in a minority of cases, must be corrected, because, without correction, disturbance resulted. The definition of the normal child might be that of the child who had the power of thriving on a simple milk mixture. When that power was regained, then, and not till then, did the abnormal infant prove its recovery.

Dr. Bernard Myers

said he knew of one medical officer who had used whole cow's milk for some time, and he saw little harm result from it. It was interesting to hear that not only the immediate effect of a diet should be regarded, but also the future effect; the desire that the child should keep healthy and away from infection was paramount. Human milk was the standard; and children did best on it.

Many analyses of mothers' milk had been done at the Children's Clinic. The milk obtained was divided into three portions. The first sample usually contained excess of protein, and the last had more fat; but that was not invariably the case, nor was it always so with the

same mother. In one instance he had had the milk tested ten or twelve times. He would never have thought that the sugar content would vary even as much as it did.

He found that children did best on what was called a modified milk, and the question was as to how the milk could best be modified. One wanted to maintain weight, and allow for growth, while providing for the production of heat and energy. It must be remembered that some children could be brought up on, and could digest, almost anything, but in such a case we did not know whether harm was not actually being done to the child which might be observed only in the future. In many children the power of absorption of nourishment was disturbed, in some cases considerably, while the action of the liver would at times be interfered with. As was pointed out by the late Sir William Bayliss, the individual cell should contain the three enzymes responsible for oxidation, reduction, and hydrolysis, but these functions might be more or less interfered with. He always endeavoured to keep that fact in mind when treating marasmic children.

It was necessary to realize the optimum condition, and he suggested the general use of the term " optimum tolerance "; i.e., a condition in which the child was fed on protein, carbohydrate, fat, and salts and water, in the correct proportions and amounts to suit the particular case, so that while keeping in best health it made the greatest gain in weight. If the amount of food was increased beyond that optimum, the child gained less and less, and might even lose weight.

Recently he had seen in hospital a child aged 9 months, suffering from marasmus. It weighed 8½ lb. It had already received much treatment, but at the time there was diarrhœa and vomiting, and the temperature was 99°F. The infant had lost too much salts and nitrogen. The body had to be trained to digest, and the intestinal mucous membrane to be trained to absorb nourishment. Further, the cells had to resume their normal function. That child was first given water for six hours, then whey, which altered the percentage of salts from 0·7 to 0·5. Then the sugar was brought down to 4 per cent., the protein to 0·9, and the fats to 0·5. The baby was weighed daily, and the nutrition watched. That child lost weight for a day or two longer, then the weight curve straightened out, and a fresh start was made. The child was now 11½ months old, and its weight was over 16½ lb.

The effects of vitamins had also to be considered. Whey was known to contain vitamins B and C. He thought it would be agreed that cod-liver oil emulsion should be given, and Colonel McCarrison said that only a small quantity of this was necessary, half a drachm of emulsion a day being ample for a baby. The child was also given a teaspoonful of orange juice night and morning. He did not think any one factor was responsible for the gain in weight above mentioned and the improvement in this child's condition, but all the factors. When the fat in breast milk reached 6 per cent., his experience was that one must be on the look-out for diarrhœa. He had one case in which the fat in human milk went up to 11 per cent. That child was unfortunately sent to America, so that he lost touch with it.

Dr. LEONARD PARSONS (Birmingham)

said that during the last few years he had had considerable experience with wasting babies. His " sighting-shot " was a cow's milk boiled and diluted to the extent of not more than 2 to 1, to which dextri-maltose was added. The healthy child showed considerable tolerance and would thrive on almost any food. In Birmingham a considerable number of balance experiments had been done on about one hundred artificially fed, wasted babies, and, like Dr. Findlay, they could not find the kind of cases described by German writers on these subjects.

How did Dr. Jewesbury and Dr. Pritchard know that their children suffered from protein indigestion? He understood that they cited the presence of curds in the stools as evidence, but he thought it had been proved that the great majority of curds in the stools were not protein, but were neutral fat or soap curds. He also asked what was the evidence that protein was indigestible. In none of the cases to which he was referring was there any evidence of indigestion of protein[1] or of deficient splitting of fat, or of any deficiency in the alimentary enzymes. It was necessary to give an excess of protein to these children for the following reason: lactalbumin was the most efficient growth-protein known, but the amount of it in cow's milk was so small, and casein was such a poor substitute, that it was therefore essential to give an amount of protein which was excessive when compared with the amount

[1] In a few cases in which considerable amounts of protein, such as "Casec," were added to the milk mixture over long periods of time, protein curds have appeared in the stool.—L. G. P

of protein in breast milk. He also asked what was the evidence of the decomposition of protein in the bowel, mentioned by Dr. Pritchard. Also, what was the evidence of absorption of toxins; what were those toxins, and what was the evidence that they caused damage to the liver and kidneys? With regard to the statement that one-third of the protein only was broken down and absorbed, the results obtained in these investigations showed that 70 to 90 per cent. of protein was broken down in the alimentary tract and absorbed.

With regard to the acidity of cow's milk, he agreed that his "sighting-shot" was not always successful, and that sometimes it was advisable to acidify the cow's milk. Before three months of age the hydrogen-ion concentration of the gastric juice was low, and to acidify milks before that age was asking for trouble. What was Dr. Pritchard's evidence that this method of feeding produced excessive acid? It was not in accordance with his (the speaker's) findings in Birmingham. For the acidification of cow's milk he had used citric acid ½ gr. to the ounce, N/10 HCl and lemon juice.

He would like some evidence from Dr. Thursfield as to fat indigestion, as he (the speaker) had not found it unless the baby was suffering from diarrhœa. Dr. Thursfield also said that after they had attained the age of three months marasmic children did well, but what proportion of marasmic children did he (Dr. Thursfield) see under three months old? It was rare to come across many such children unless they were subjects of pyloric stenosis or pylorospasm.

He agreed with Dr. Findlay that starvation was the factor which needed combating in the large majority of wasted children. This might be due either to diarrhœa, or to some fault in the amount of food given. It was important therefore that a sufficient amount of protein and fat should be given, the milk being modified only by boiling and by the addition of water and dextri-maltose.

Dr. FINDLAY (in reply)

said that the whole question turned on clinical experience; the ultimate test was the child. Every year brought forth some new modification in infant-feeding; in nearly every hospital one visited the physician in charge had a special panacea, and he practically stated that his was the only method of successfully rearing children. But if the truth were known it would probably be found that he had run the whole gamut of the known modifications and that the lauded panacea was only his latest preference.

He (Dr. Findlay) did not think that the babies in Glasgow differed from those in London and Birmingham, and consequently he doubted much of what was said about the special reactions. Was there, he asked, any real proof that breast-fed babies were better in health than bottle-fed babies, unless among the very poorest classes? Who, in fact, were the mothers that fed their children mainly at the breast? Those of the poorer classes, certainly so far as Scotland was concerned. The average better-class mother was not so fit to suckle her infant as her poorer sister. Yet who had the bigger child at the end of a year? Was there, too, real evidence of damage to the liver or kidney, such as had been mentioned, or interference with development as the result of feeding infants on undiluted cow's milk? In this connexion he would mention the case of twins which he had the opportunity of observing during a visit to Pirquet's clinic. One of the twins was being fed on the principles laid down by the Lister Institute school of thought, i.e., on a diet rich in vitamins A and C, and the other on a diet very poor in fats and these vitamins, because Pirquet did not, at least at the time, consider them necessary. Both children developed equally and well. Both children contracted influenza at the same time, and it was interesting to record that it was the child which had been fed on the fat- and vitamin-poor diet that recovered and regained its weight most rapidly.

An interesting point was that of curds in the stools, often taken as evidence of inability on the part of the child to digest proteins. In his experience, everything pointed to their presence being due to some change in the milk rather than in the child, since it would happen that when the curds did appear, all the children in a ward would be affected.

He (Dr. Findlay) said he had little doubt that in Glasgow at least, many cases of wasting were due to insufficient nourishment. If there was slight vomiting or an attack of curds, the doctor or nurse in charge got the idea that the milk was too strong. The milk was therefore diluted, with consequent diminution in the amount of nourishment obtainable from it; the child continued to cry, in fact it cried worse, but from hunger, and the process of dilution was carried still further until the child was only getting one or two ounces of milk in the day. That was revealed by many of their case histories both in hospital and private

practice, and improvement only set in (and as a rule did so immediately) when the child was given its calorific requirements.

A remark had been made on the need for application of the different clinical pictures produced by different types of food intolerance, but at the present moment we did not know these pictures. At one time he (Dr. Findlay) had thought he could recognize "fat dyspepsia," of which so much had been made by various writers. It had been stated, for example, that by examination of the stools with Sudan III it was possible to tell whether or not they contained an excessive amount of fat. Neither he nor his co-workers had been successful in the use of this test, and when it was appreciated, from the work of Dr. Harry Hutchison, that the fat-content of a non-diarrhœal stool of a marantic infant when examined chemically contained the same amount of fat as that of the normal infant—some 30 per cent. of the dried matter—the futility of such a ready test would be apparent. Nevertheless, he was of opinion that some children who did not thrive on full-cream milk throve on half-cream milk with the addition of carbohydrate. Whether this was due to the diminution of the fat allowing of better absorption, or simply due to the additional amount of carbohydrate, was a point that at present remained undecided, as Rosenstein had shown that equally good results were obtained by the addition of carbohydrate, whether the fat intake was diminished or not.

In conclusion Dr. Findlay remarked on the fact that although Dr. Pritchard had enumerated the tremendous differences (affecting in all thirty substances) between cow's and human milk, he (Dr. Pritchard) nevertheless appeared to have paid all his attention to the proteins.

Dr. ERIC PRITCHARD (in reply)

said he had not thought it necessary in his address to enter upon the question of indigestion from excess of protein other than that due to motor disturbances of the stomach. Evidence of this was furnished every day by washing out the stomach and finding curd three hours after the ingestion of milk. What he had referred to, chiefly, were the disturbances of inner metabolism and those of liver and kidney function, symptoms more definitely seen in older children. In infants the clinical picture of protein decomposition was very clear: there were alkaline and offensive stools and a muddy complexion—some considered that by this grey-blue complexion they could at once recognize children so poisoned. There was also stupor and other evidence of poisoning by the toxins produced by the bowel and absorbed into the system. Dr. Parsons rightly said that the lactalbumin in the milk was *par excellence* the protein for growth, and he also asked, as there was only a small proportion of it in cow's milk, what chance was there of increasing the amount by modification? His (the speaker's) reply was, that the whole point of scientific modification was to secure any required percentage of composition. Dr. Parsons said it was asking for trouble to give acidified milk to children under three months of age; if that were so, he (the speaker) feared he was courting disaster every day of his life. But the results he got did not bear out Dr. Parsons' belief.

CLINICAL SECTION.

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Clinical Section.

President—Dr. G. Newton Pitt, O.B.E.

Two Cases of Persistent Paralysis of One Vocal Cord from a Tubercular Lesion involving the Recurrent Laryngeal Nerve.

By Sir StClair Thomson, M.D., F.R.C.S.

(ABSTRACT.)[1]

These two cases exhibited are of interest as the hoarseness resulting from this was the first symptom to attract attention, and the paralysis persists four and eight years after its first discovery, although in both cases the tuberculosis in the chest is quite arrested.

Case I.—Male, aged 40, lost his voice after influenza in April, 1916. No cough, no sputum, and tubercle bacilli were never found. But there were symptoms of diffused fibrosis and disease at both apices. Patient's larynx remains to-day in the same condition in which it was found seven years ago, i.e., the left vocal cord is quite immobile in the cadaveric position; its margin looks excavated (from atrophy); the right cord, in an effort of compensation, crosses the middle line, but fails to close the glottic space, so the voice remains hollow and toneless.

The patient carries on his occupation as a draper.

Case II.—Patient was on active service through the war, and was demobilized in February, 1919. His voice had been slightly hoarse for years, but he had taken little notice of it until, in January, 1920, he had a sudden hæmoptysis of half a pint of blood.

On admission to the sanatorium at Midhurst he was afebrile; there were tubercle bacilli in scanty sputum, but only at the right apex were there a few crepitations. Mobility of right vocal cord defective. Although this latter cord retained its tension and adducted well, it did not swing out in abduction beyond the cadaveric position.

He has kept on his work in a City bank since he left the sanatorium in June, 1920. The larynx remains *in statu quo*, i.e., both cords adduct well, and completely close the glottis, hence there is not the voice of phonative waste of the preceding case. But the right cord does not abduct beyond the cadaveric position.

The case illustrates Semon's law.

A Case of Cutaneous Neurofibromatosis (Recklinghausen's Disease) with a Left Lateral (Suprazygomatic) Meningocele, simulating a Soft Fibromatous Fold of Skin.

By F. Parkes Weber, M.D.

The patient, E. D., aged 38, unmarried, English, domestic servant, was admitted to hospital, complaining of dyspeptic pains, which were probably connected with

[1] These cases will be published in full elsewhere.

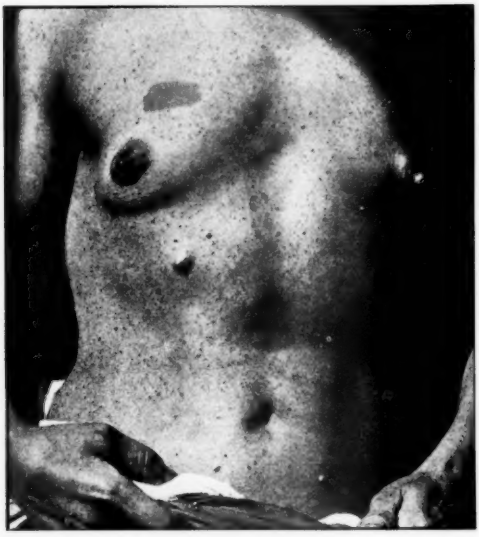


FIG. 1.—To show the small molluscous fibromata and the cutaneous pigmentation. (From a photograph by Dr. G. Welsch.)

FIG. 2.—To show the meningocele with the tail-like tuft of hair below it. (From a photograph by Dr. G. Welsch.)

gastroptosis and gastric achlorhydria. She is a thin, dark-complexioned woman with numerous small soft brownish molluscous fibromata scattered over her trunk, together with numerous brown or blackish spots and areas of cutaneous pigmentation of various shades and various sizes, resembling simple cutaneous pigment nævi. In fact, she has a moderate degree of Recklinghausen's disease or neurofibromatosis (*see* fig. 1).

On the left side of her face, in front of the ear, is a tail-like tuft of black hair, which she thinks she has had all her life, evidently a hairy nævus. Just above this, hanging over the zygomatic ridge, is a soft fold of skin (fig. 2), at first sight resembling one of the soft fibromatous folds (" chalastodermatocele " or " Lappen-Elephantiasis ") sometimes associated with Recklinghausen's disease. On further examination, however, the contents of this fold of skin appear to be fluid and can be easily pressed into the cranium, evidently through an aperture in the skull. A depression in the skull can be distinctly felt above the zygomatic ridge, in about the position of the " pterion " —the spot where the greater wing of the sphenoid bone meets, or nearly meets, the temporal, parietal and frontal bones.

The fold-like swelling is translucent to light, and free from pain or tenderness, even when pressed upon. It is not tense, and is not made definitely more tense even when the patient holds her breath and forces the blood into her head. Roentgen skiagrams, it may be noted, do not confirm the existence of any aperture in the skull. The swelling was apparently first noticed when she was about 20 years of age, and has slowly increased in size since then. A surgeon (Dr. H. Rast), who was consulted about it, recognized its connexion with the interior of the cranium, and refused to remove it. It causes her no trouble beyond a slight feeling of dragging.

That it must be a true meningocele can hardly be doubted. It is very unlikely to be a dermoid cyst, situated partly within and partly without the skull. There is no history of trauma to suggest that it might be a so-called " false meningocele " or " Billroth's disease." A false meningocele is due to a previous fracture, with injury to the brain substance, which has allowed the escape of cerebro-spinal fluid from the lateral ventricle, through an opening in the meninges and cranium, thus forming a fluid swelling under the scalp (probably between the skull and the periosteum). It has not been thought advisable to do a lumbar puncture to try whether the pressure of the escaping cerebro-spinal fluid can be increased by squeezing the (supposed) meningocele.

The tail-like tuft of hair (hairy nævus) on the side of the face near the meningocele reminds one of the tuft of hair (" myelopathic circumscribed hypertrichosis ") or " human tail," sometimes met with on the back in connexion with *spina bifida* or *spina bifida occulta*.

A cranial true meningocele in an adult is a rare condition. A few examples are referred to by John Ruhräh (" Meningoceles and Allied Malformations," *Archives of Pediatrics*, New York, 1902, xix, pp. 514-531) from the previously published literature on the subject. They may first show themselves many years after birth, but I have never heard of a case exactly like the present one.

Very little is known of the family history of the present patient. Her father and mother are supposed to be living, but she knows nothing of her father, and has not seen her mother for years. Her mother had nine children, of whom the patient herself and, she thinks, two brothers are living, but she has not seen these brothers for a long while. On the whole, she has enjoyed good health. She does not know how long she has had the little cutaneous tumours and pigmentation (Recklinghausen's disease), and she says that she has paid very little attention to the matter. She has not heard of anyone else in the family having anything similar.

It may perhaps be noted that the patient has what appears to be a supernumerary nipple below and to the inner side of the right mamma. Nothing else special has

been discovered. A blood-count shows that she is not anæmic, and her urine is free from albumin and sugar. Her brachial systolic blood-pressure is about 120 mm. Hg.

According to Professor G. Castronuovo (*Riforma Medica*, Napoli, 1920, xxxvi, p. 817) Recklinghausen's disease may be associated with congenital lipomata, plexiform neuromata, syringomyelia, and some other dystrophies, including bony malformations. The rare developmental condition termed tuberous sclerosis of the brain may perhaps be allied to Recklinghausen's disease, and is well known often to be associated with sebaceous adenomata of the face, sometimes with other superficial abnormalities (such as pigment nævi and peculiar fibroid thickenings of the cutis or subcutis), and with curious renal "tumours" of congenital developmental origin. Leontiasis ossea, or localized osteitis fibrosa of the face, has been occasionally associated with Recklinghausen's disease, and E. Pearce Gould (*Quarterly Journal of Medicine*, Oxford, 1918, xi, pp. 221-228) has described bone-changes, similar to those of osteomalacia, in Recklinghausen's disease.

Case of Aneurysm occurring eight years after Gunshot Wound.

By E. Laming Evans, C.B.E., F.R.C.S.

H. R. G. H., aged 33, painter's improver.

History: (1) *Past.*—In the Army for four years and three months (September 29, 1914—January 11, 1919). Wounded three times, (1) September 12, 1916, in right calf; (2) August 4, 1917, in right knee (superficial shrapnel wound); (3) July 18, 1918 (left thigh). His first wound in September, 1916, was a penetrating one, the bullet entering the outer side of the calf in its upper third, passing through the tibia to a point over the subcutaneous surface of the tibia, and was the probable cause of the present aneurysm. An X-ray photograph taken October, 1924, shows a well rounded exostosis corresponding with the line of bullet flight, and a detached spicule of bone suggestive of myositis ossificans.

(2) *Recent.*—On September 4, 1924, whilst at work, he noticed a sudden pain in the right calf, and felt a lump of which he was previously unaware: it was painful on walking to such an extent that he had difficulty in getting home. He was subsequently transferred to Shepherd's Bush and examined by me on September 30.

Present condition.—Half an inch below the fold of the right knee and occupying the outer half of the calf under the triceps suræ, there is a sessile swelling, measuring 4 in. by 2½ in., which exhibits expansile pulsation, and a systolic bruit which ceases on compression of the femoral artery. The anterior and posterior tibial pulsations at the ankle are equal in the two sides. Blood-presssure appears normal. No arterial thickening. Wassermann negative. Soft systolic murmur audible at apex of the heart and limited to that area. No evidence of lead poisoning.

Special point of interest: Delayed origin of the aneurysm, or delayed manifestation of its symptoms.

I propose to excise the aneurysmal sac, ligaturing the arteries and veins above and below.

It is not possible to say, before dissection, whether the aneurysm is in connexion with the anterior or posterior tibial arteries.

Dr. Parkes Weber, in view of the long interval between the injury and the occurrence of the aneurysm, suggested that the latter might be a so-called "mycotic aneurysm." Most mycotic aneurysms were connected with chronic malignant endocarditis, but microbes gaining access to the blood-stream during some infectious disease, such as gonorrhœa or so-called influenza, though insufficient to set up malignant endocarditis or metastatic abscesses, might possibly, nevertheless, lodge at a *locus minoris resistentiæ* in the arterial wall (caused by a previous injury), so as to set up non-suppurative inflammation with the formation of a "mycotic aneurysm."

Case for Diagnosis.

By B. T. PARSONS-SMITH, M.D.

F. W. D. W., aged 48.

Symptoms.—Giddiness, sleeplessness, fainting sensations and breathlessness during past two years.

Past History.—Scarlet fever at 8; rheumatism at 25; active service in France 1914-1917: shell concussion 1915: invalided (valvular disease of the heart) 1917, 40 per cent. disability; exercise tolerance normal until onset of present illness; tobacco and alcohol always moderate.

Physical Signs.—Heart enlarged downwards and to the left; apex impulse well localized in the sixth space 4½ in. from the mid-line; right border of heart normal (confirmed by X-ray examination); rhythm regular except for an occasional premature contraction; harsh systolic murmur and systolic thrill both of maximum intensity in the fourth left space at the sternal margin, the murmur being conducted upwards to the left infraclavicular region; pulse 90, volume regular but small; systolic blood-pressure 140 mm.; pulse-pressure 50 mm.; no venous congestion. Urine examination negative. Wassermann test negative.

Electrocardiogram.—Partial heart block (P. R. interval 0·28 sec.); T-wave flattened in Lead II; no preponderance of either ventricle.

X-ray Examination.—Heart enlarged downwards and to the left; transverse measurement 6 in.: great vessel shadow, normal in shape and contour, transverse measurement 2 in.; normal cardiac movement with respiratory phases; posterior mediastinum clear; diaphragmatic excursion diminished on the left, normal on the right side; root shadows bilateral.

DISCUSSION.

Dr. PARSONS-SMITH said he was of opinion that the systolic murmur and thrill were indicative of obstruction at the base of the stem of the aorta, and by reason of the fact that the murmur was atypical in its seat of maximum intensity (fourth left space), and of the normal distribution usually observed in aortic stenosis, he suggested that the obstruction was situated below the level of the aortic valve; he considered that the condition was one of sub-aortic stenosis and that the associated signs—cardiac enlargement to the left and downwards, heaving and well-sustained apex impulse, small pulse—were confirmatory of this diagnosis. The question of congenital defect could not be entirely shelved in spite of the fact that the patient had been passed fit for military service during the war, and that there were no obvious indications of the syndrome of congenital heart disease. He (the speaker) was inclined to regard the case as one of aortic stenosis and he thought that there was insufficient evidence to determine whether the lesion was acquired or congenital.

Dr. DOUGLAS FIRTH agreed with the diagnosis of aortic stenosis and was of opinion that congenital disease could probably be excluded; he did not agree with Dr. Parsons-Smith that the murmur was not conducted into the cervical vessels; he could hear the murmur in the carotids on both sides, also in the femoral vessels; consequently he felt disposed to name the condition aortic stenosis without qualification.

Dr. PARKES WEBER agreed with Dr. Parsons-Smith that the cause of the abnormal physical signs was probably some obstructive lesion in the neighbourhood of the aortic valves, that did not prevent the valves from closing during the cardiac diastole. In view of the definite history of traumatism (the patient said he was "blown up" and was unconscious for a considerable time afterwards), Dr. Weber suggested that the obstructive lesion was probably of traumatic origin, possibly resulting from rupture of the endocardium below the aortic valves, or from a sub-endocardial hæmatoma, with resulting cicatrix. In regard to the possibility of the occurrence of such traumatic lesions, Dr. Weber referred to a work published in Stockholm in 1923, relating to tears in the internal elastic lamina of cerebral arteries.[1]

[1] O. P. Reuterwall, "Ueber bindegewebig geheilte Risse der Elastica interna der Arteria basilaris," Stockholm, 1923.

Case of True Congenital Thyroglossal Fistula.

By HAMILTON BAILEY, F.R.C.S.

THE patient, a girl, has not had an operation upon the neck. Her mother is confident that the condition was present at birth.

As Raymond Johnson (*Lancet*, May 10, 1890) first pointed out, *congenital* thyroglossal fistula is most exceptional; it is usually due to the rupture of a thyroglossal cyst.

In thirty-one consecutive cases of thyroglossal fistula admitted into the London Hospital only one was present at birth. The remaining thirty were acquired at some time between the third week and thirty-first year of life.

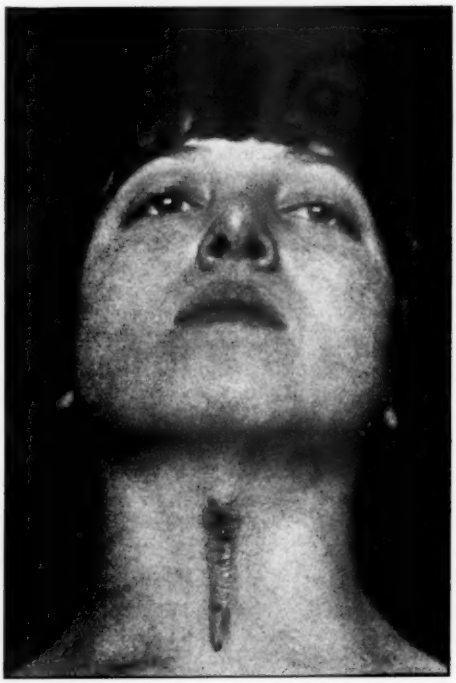


FIG. 1.—True congenital thyroglossal fistula.

Whilst in a few of these thirty cases the cyst ruptured spontaneously during an attack of acute inflammation, the vast majority were the direct result of some form of surgical intervention, which includes: (1) *Incision of the cyst* (usually performed under the erroneous impression that an inflamed thyroglossal cyst was an abscess). (2) *Excision of a thyroglossal cyst without enucleation of the entire thyroglossal tract.* This case is a good illustration of the fact that the thyroglossal tract on occasions reaches far below the level of the isthmus of the thyroid gland and thus some so-called " dermoids " in the suprasternal recess are in reality thyroglossal cysts.

Patient under care of Sir Hugh Rigby.

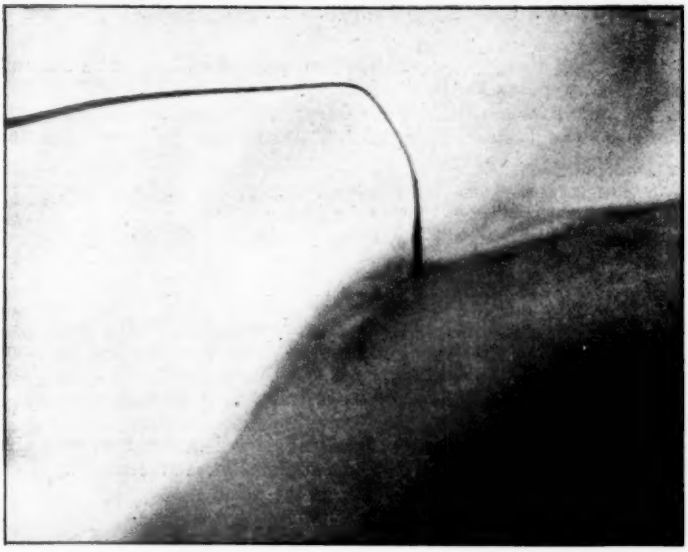


FIG. 2.—True congenital thyroglossal fistula. A probe passed into the inferior fistula passes down just behind the manubrium.

Case of Sub-hyoid Thyroglossal Cyst.

By HAMILTON BAILEY, F.R.C.S.

Sub-hyoid thyroglossal cyst.

PATIENT, a young man. The cyst has been present as long as he can remember, but has been rapidly increasing in size latterly.

This is the commonest situation for a thyroglossal cyst. In ninety-two cases in the records of the London Hospital the site of the cyst has been accurately described:—

In tongue beneath foramen cæcum	3 per cent.
In floor of mouth protuding beneath chin	6 „
Suprahyoid	12 „
Subhyoid	37 „
On thyroid cartilage	23 „
At level of cricoid	7 „
In suprasternal recess	4 „

The operative treatment of this condition, in the past, left much to be desired. At least 26 per cent. of thyroglossal cysts (up to 1922) were found to be recurrent or the thyroglossal cyst was converted into a fistula. Sistrunk's operation (which eradicates every vestige of the thyroglossal tract) now ensures success in these cases.

The patient in this case has advanced mitral stenosis and therefore if the cyst is to be removed the operation will be carried out under local anæsthesia.

Clinical Section.

President—Dr. G. Newton Pitt, O.B.E.

Case showing an Unusual Variety of Repeated Seizures.

Shown by Macdonald Critchley, M.D. (for Aldren Turner, C.B., M.D., F.R.C.P.).

E. B., male, aged 16, pupil teacher, was admitted to the National Hospital for the Paralysed and Epileptic, Queen Square, under Dr. Aldren Turner, with the following history: Patient was well until Easter, 1924, when he contracted a febrile disorder which confined him to bed for three weeks. He had nocturnal delirium and restlessness for one week, and was extremely lethargic during the day-time (? acute epidemic encephalitis). Patient never made a satisfactory recovery from this illness and was sent away to convalesce. One month before admission a hacking cough appeared which continued almost incessantly throughout the day and kept him awake at night. One week before admission he suddenly fell down in what was called a "faint." He recovered in about 30 seconds and was very soon overcome by another "fit." These turns now came on every 30 seconds or so, continuing throughout the day but ceasing during sleep. This was his condition when admitted to hospital. Previous health showed *nil ad rem.*

Family History.—Maternal grandparent in an asylum for senile dementia.

Condition on Admission.—The "turn" was occurring every 30 seconds, only slowing up under the stimulus of a meal, or during examination. The fits were ushered in by several noisy expirations; the patient then compressed his lips tightly together and voluntarily held his breath. The tongue was protruded between his inverted lips. The face became deeply congested and the pupils dilated. The limbs were held in a state of tonic extension. After about 10 seconds the breath was released and the cyanosis was succeeded by intense pallor. At the same time the mouth was widely opened, and the eyes were deviated to the right. Breathing returned in deep pants. During this cycle, consciousness was only occasionally lost. Corneal and plantar reflexes remained unaltered.

Physical examination revealed three points of abnormality: (1) There was rapid, ill-sustained, horizontal, rhythmic nystagmus on lateral, ocular deviation. (2) Pupillary reaction was sluggish on accommodation. (3) Ocular convergence could not be sustained when the fixation-point was nearer than 4 in. to the eyes.

Under the influence of sedatives (luminal, bromides, hyoscine) the attacks became somewhat less frequent and less violent.

Chloroform anæsthesia and suggestion failed to bring about any lasting relief.

[By the courtesy of the Kodak Co. a cinematograph film of the patient was exhibited].

Dr. F. Parkes Weber regarded the remarkable attacks in Dr. Aldren Turner's case as probably of organic nervous origin—a sequela of encephalitis lethargica. Though much more complicated, they might yet be compared with the respiratory disturbances that had occasionally been observed after undoubted epidemic encephalitis. In a few such cases tachypnœa had been accompanied by myoclonic or cramp-like movements synchronous with respiration (for instance, movements about one or both shoulders). It would be of the utmost interest in Dr. Turner's case to have a chart made on which the respiratory movements, in their amplitude and frequency, could be registered before, during, and after an attack. Such a graphic chart should be contrasted with one, taken in the same way, showing the frequency and amplitude of the respiratory movements in patients with typical grouped or periodic respiration of Cheyne-Stokes type.

Case of Aortic Aneurysm (Intrapericardial).

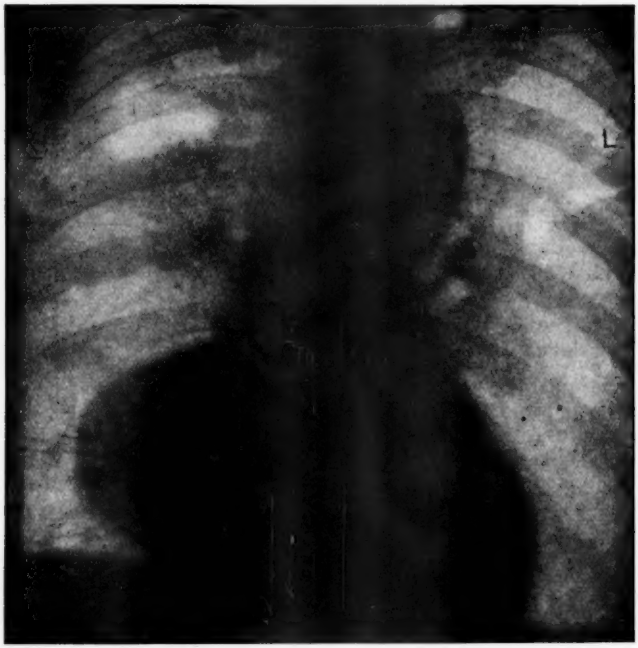
By B. T. PARSONS-SMITH, M.D.

S. J. C., AGED 51.

Onset of present illness January, 1923; pain in chest (chiefly on right side) constantly present but intensified by exercise, palpitation (usually felt on the right side), breathlessness with effort, and cough spasmodic in type.

Past History.—Rheumatic fever at 30; influenza 1921.

Physical Signs.—Area of cardiac dullness (normal to the left) continuous, with dull area in third and fourth right spaces, which extends to region of forcible and expansile heave in fourth space, 4 in. from mid-line; rhythm regular; systolic and



diastolic murmurs over whole præcordium and over dull area to right of the sternum; vessels thickened; blood-pressure (arms) 120 mm. systolic, 70 mm. diastolic; jugular vessels (particularly the right) engorged in recumbent position; electrocardiogram shows left ventricular preponderance; X-ray examination (see fig.) shows large pulsating mass in the third and fourth spaces on the right side, which merges into, and is apparently continuous with, the cardio-vascular shadow; edge of shadow convex rounded and regular; density of shadow similar in depth to that of heart; posterior cardiac space invaded in middle third.

Wassermann reaction positive.

Dr. PARSONS-SMITH, having reviewed the chief diagnostic features of the case, referred to the rarity of the condition; he had observed three cases of aortic aneurysm intrapericardial in type, in all of which the progress of the disease was noted to be abnormally slow compared with the more frequently occurring aneurysms of the extrapericardial aorta; he suggested that with dense layers of pericardium reinforcing an aneurysmal sac the resistance of the latter would be materially increased, and that one might reasonably anticipate the capacity for exercise to be moderately well maintained in such cases; further, that the life outlook was likely to be distinctly more favourable.

[*December* 12, 1924.

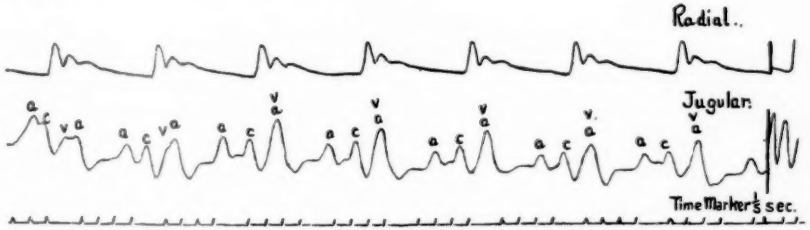
Case of Complete Heart-block.

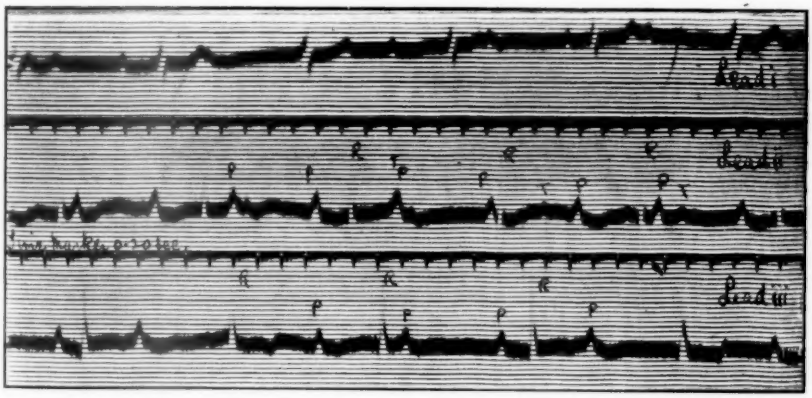
By B. T. PARSONS-SMITH, M.D.

L. F. P., FEMALE, aged 22, civil servant.

Symptoms.—Breathlessness, giddiness and exhaustion during past three years.

Past History.—Childish ailments—measles, tonsillitis (several attacks), whooping cough, bronchitis, chicken-pox and diphtheria; slow heart action since the latter illness (diphtheria) seven years ago.





Physical Signs.—Heart enlarged: increased area of relative dullness to the left to the region of the apex impulse in the fifth space, 4 in. from the mid-line; rhythm a regular bradycardia at 52; soft systolic murmur at apex and base; diastolic interval pure; vessels normal for age; blood-pressure 130 mm. systolic, 85 mm. diastolic; no venous congestion; no œdema; urine analysis negative; Wassermann reaction negative. Electrocardiogram (see fig.) shows complete auriculo-ventricular heart-block with some sinus arrhythmia.

Diagnosis.—A case of cardiac enlargement with complete heart-block.

Note.—In order to estimate the degree of the block and the functional elasticity of the ventricle in this case certain tests were performed as follows:—

(1) *Vagal release*: $\frac{1}{60}$ gr. atropine by hypodermic injection increased the rates of the auricular and ventricular contractions, the latter from 48 to 75, the former from 84 to 120.

(2) *Adrenalin test.*—Adrenalin hydrochloride (7 min. in 1 c.c. saline), subcutaneously, accelerated moderately the auricular and ventricular rates, the former from 84 to 105, the latter from 48 to 75 in eight minutes.

(3) *Digitalis test.*—The prescription of digitalis (Nativelle's granules, $\frac{1}{240}$ gr.) had no appreciable effect on the auricular or ventricular rates during three weeks' medication, and the electrocardiographic records were uninfluenced by the drug.

(4) *Exercise test.*—A simple exercise, walking round the ward, the ascent of a short staircase and such like, induced a slight ventricular acceleration averaging four to six beats per minute; effective tolerance for muscular effort very poorly sustained.

A Case for Diagnosis.

By Harold A. Moody, M.D.

S. B., male, aged 67. Bricklayer (but not for the last two years).

Past history.—Two previous attacks of jaundice, (?) 1911 and 1914. Severely ill in 1914 for thirty to forty weeks. Liver then enlarged, for his doctor said: "He must have been abroad to get such a liver." Short of breath for several years (four or five). Very deaf for years. No other illness. Never been abroad. Denies venereal disease, also alcohol. Married 37 years. No children. No history of miscarriage. Wife died two years ago.

History of present illness.—First seen by me on July 30, 1924, when he gave the following history: Not well since loss of wife; worse this year. Slight jaundice in March. Now has cold shivers down the back. Urine offensive. Loss of flesh. Irritable skin, general weakness and shortness of breath.

On examination.—Slight icteric tinge, flattening of upper part of right chest with bulging in axillary line towards the base. Right chest, 16½ in.; left, 17½ in. At the right base, which is dull, there is no movement on deep inspiration, although the breath sounds are heard there. Says dyspnœa is less marked than two years ago. Cardiac dullness was normal. Heart-sounds also normal, with the exception of a few pulmonary bruits. Blood-pressure, 170/80. Weight, 10 st. 4½ lb. Double inguinal hernia. Urine, 1015; no albumin; no sugar. Pigmented scar of varicose ulcer on left leg. Liver enlarged.

Patient went away for a short holiday and when I next examined him, on October 17, I found that the signs in the chest had increased so that there was now only a small resonant area at the apex. He said he had a fainting attack when away at Westcliff; also he felt a little faint during the examination. Weight, 10 st. 3⅛ lb. On November 30 vocal resonance was increased in the front of the chest and diminished behind.

Present condition (November 5, 1924).—Right common carotid seems pulled over to the mid-line. *Heart:* Superficial cardiac dullness extends from the upper border of the third rib to the fifth space, N L.; right border ill-defined, sounds normal, with the exception of increased aortic second sound and some premature beats. Heart-sounds well heard in the third right interspace for about 2 in. from the right sternal margin. *Lungs:* Left chest emphysematous. No adventitious sounds. Right chest: resonance diminished from above down, dull at base and also right axillary region. Breath-sounds increased at apex, but diminished from above downwards. Point of marked

change in resonance and breath-sounds seems to commence about the lower border of third rib. Vocal resonance increased behind, diminished in front. *Liver:* Upper limit, lower border of fifth rib. Lower limit, within two finger-breadths of umbilicus. Surface has a ridge across, otherwise smooth. Spleen not felt. No ascites. No œdema. Deep reflexes normal. Temperature and pulse normal.

PATHOLOGICAL REPORT BY E. ff. CREED, M.D., AND R. D. LAWRENCE, M.D.

Wassermann reaction.—Clearly negative.

Van den Bergh's test.—Direct positive; indirect 1 unit (normal 0·2 to 0·6). This suggests a slight obstructive jaundice.

Blood-count.—Red blood corpuscles, 4,720,000 per c.mm.; hæmoglobin, 60 per cent.; white blood corpuscles, 5,600 per c.mm.; colour index, 0·84.

Differential count.—Polynuclear leucocytes, 61·6 per cent.; eosinophils, 0·8 per cent.; lymphocytes, 34·0 per cent.; large mononuclears, 2·4 per cent.; mast cells, 1·2 per cent.; no abnormal forms seen.

X-RAY REPORT BY ROBERT KNOX, M.D.

Screening report.—Hilar shadow on right side increased in breadth. Aorta (arch and descending) generally dilated. Right dome of diaphragm moves less than left. Left costo-phrenic angle does not light up on inspiration. A triangular opaque area with base at periphery and apex towards hilum extends across the lower half of the right lung field.

Film report.—The X-ray in the anterior-posterior line shows diminished translucency throughout the right lung field with a triangular shadow extending towards the hilum, but not reaching to this structure. The base of the opacity is at the periphery and has a sharply defined curved upper and inner border which extends to the apex. The lower border runs into the diaphragm shadow. The tile-roof ribs over the collapsed lung are seen in the oblique view. General dilatation of the thoracic aorta is seen both in the anterior, posterior and the oblique positions. The appearance would suggest an extra-pulmonary lesion, probably encysted empyema. The diminished translucency of the lung field is due to pressure of the lesion and the accompanying fibrosis. ? Mediastinal tumour.

REPORT BY ARTHUR CHEATLE, C.B.E., F.R.C.S.

Nose, pharynx, naso-pharynx, larynx, *nil.* Both membranes clear.

Lesion: Middle-ear deafness. No nerve lesion. Nothing suggestive of syphilis.

REPORT ON COMPLETE EXAMINATION OF URINE BY E. ff. CREED, M.D., December 9, 1924.

Specific gravity, 1015; reaction to litmus, acid; albumin, present; sugar, *nil*; acetone, *nil:* indican, *nil*; iodides, present; bile, *nil.*

Deposit: A little pus, a few epithelial cells, mucus, one or two oxalate crystals, coliform organisms and granular urates.

Gram-negative bacilli in direct smears.

Dr. F. PARKES WEBER regarded the case as one of chronic polyserositis, probably of tuberculous origin, with an adherent thickened pericardium, and with a chronically congested liver bound down by a thick perihepatitic covering—the German so-called "Zuckergussleber"—a pericardial pseudo-cirrhosis of the liver (Friedel-Pick). The lower part of the right lung was probably partially collapsed, and the pleura over it, originally containing a serous effusion with much fibrinous deposit, was now probably adherent and much thickened. There was possibly also some chronic tuberculous indurative mediastinitis present; and some opaque patches in the upper part of the right lung, seen in the Roentgen skiagram, were apparently calcified remnants of old tuberculous foci. The most important treatment doubtless consisted in good nutrition and the avoidance of over-exertion. Death would most likely ultimately be due to gradual cardiac insufficiency or disseminated tuberculosis.[1]

[1] Some cases of the same class apparently showed what the French called "tuberculous cirrhosis" of the liver at the post-mortem examination, and fresh miliary tubercles were likely to be found in the liver of such cases on microscopic examination.

Case of Abscess of Lung treated by Bronchoscopic Aspiration and Lavage.

By MAURICE A. CASSIDY, M.D.

W. W., AGED 38, was sent to St. Thomas's Hospital by Dr. Watson Reid, of Woking, in June, 1924, with the following history:—In October, 1923, he had four teeth extracted under nitrous oxide; an attempt was made to extract a fifth, which was broken during the process. Five days later there was severe pain on breathing over the left side of the chest, followed by pyrexia and the appearance of copious, fœtid sputum. One week, and two weeks, after the onset of these symptoms he had hæmoptysis. He remained in bed for four months and then went to the seaside to convalesce. No improvement resulting, he was sent to St. Thomas's Hospital. On admission he looked very ill; his weight was 7 st. 7 lb., and for six weeks there was constant evening pyrexia from 99 to 101° F. He expectorated from 12 to 15 oz. of very offensive, dark brown, purulent sputum in the twenty-four hours. This sputum contained no tubercle bacilli, the predominant organism being a hæmolytic streptococcus. There was a pleural rub in the left axilla. Over the apex of the left lower lobe breath-sounds were diminished and the percussion note was modified. X-ray examination showed a dense opacity over the left lower lobe; in some of the films it was thought that the outline of a small cavity containing fluid could be seen near the apex of the left lower lobe.

Bronchoscopy was performed by Mr. W. G. Howarth on May 13, 1924, when an abscess cavity was found opening into the left upper bronchus. This cavity was about the size of a walnut. It contained extraordinarily offensive blood-stained pus. After aspiration of this cavity it was found to be lined by granulation tissue, but no foreign body was seen or felt. It was swabbed out with four per cent. or ten per cent. silver nitrate, and this was repeated after aspiration on six subsequent occasions. At the same time thiocol was administered by the mouth. The results of this treatment have been most gratifying. The patient is now free from sputum and his general health is much improved. He has returned to his occupation in a gas-works.

Case of Incessant Paroxysmal Tachycardia in a Child.

By MAURICE A. CASSIDY, M.D.

C. T., BOY, aged 8, was brought to the Tuberculosis Department of St. Thomas's Hospital in December, 1922, because his father had died of pulmonary tuberculosis, and the mother thought the child was looking pale and thin. There was no history of rheumatism or sore throats, or of anything suggestive of cardiac disease. No evidence of tuberculosis was discovered, but the heart being found to beat irregularly, he was transferred to the Cardiographic Department, where he has been under observation ever since. During this period he has occasionally complained of vague pains in the limbs, but with this exception he declares himself to be perfectly well. He runs about with other boys, and recently he broke his leg in the course of a game of football. He is unable to say whether a paroxysm is in progress or not unless he feels his own pulse, when he can always decide the point.

On examination: A little pale and thin, otherwise no physical signs of disease. No evidence of cardiac enlargement; sounds normal. Subject to short paroxysms of tachycardia every few minutes; as a rule these paroxysms last about thirty seconds, rarely more than a minute. Sometimes they follow one another incessantly, successive paroxysms being separated by an interval of only three to five seconds; at other times there may be periods of normal rhythm lasting five or ten minutes.

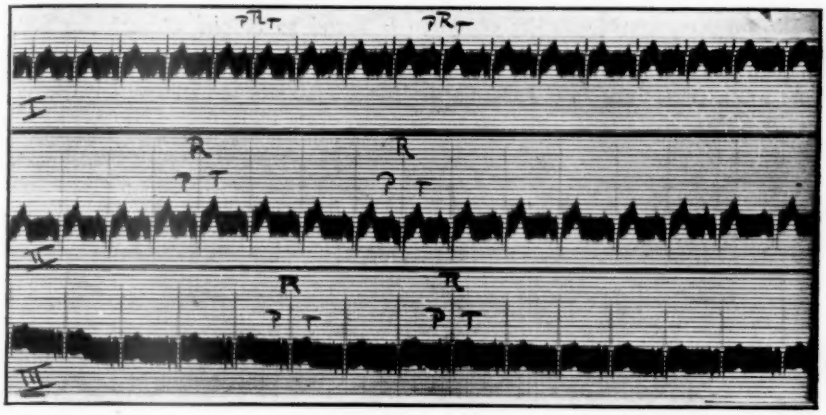


FIG.1.—Normal rhythm.

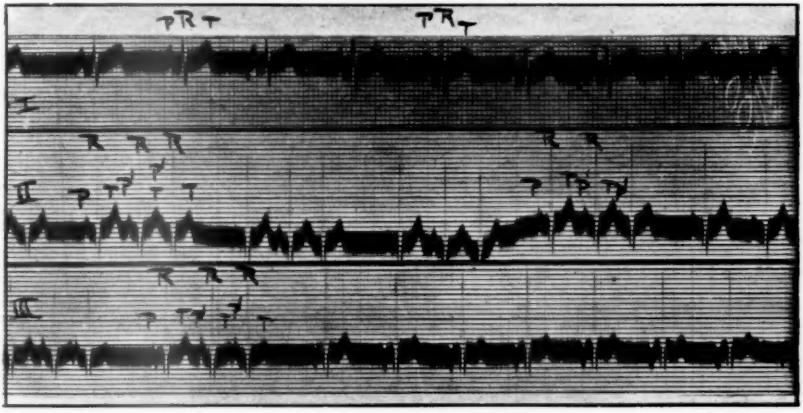


FIG. 2.—Triple rhythm.

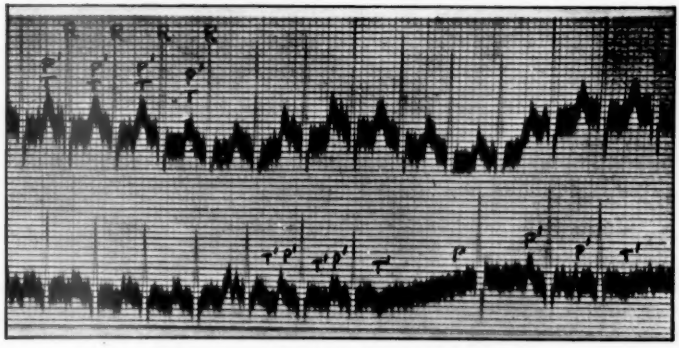


FIG. 3.—Paroxysm of auricular tachycardia.

Electro-cardiographically the condition is found to be one of auricular tachycardia as a rule, though in some of the paroxysms auricular flutter has been demonstrated. A prolonged course of parathyroid treatment was without effect. From April, 1924, until July, 1924, he was given one grain of quinidin sulphate once

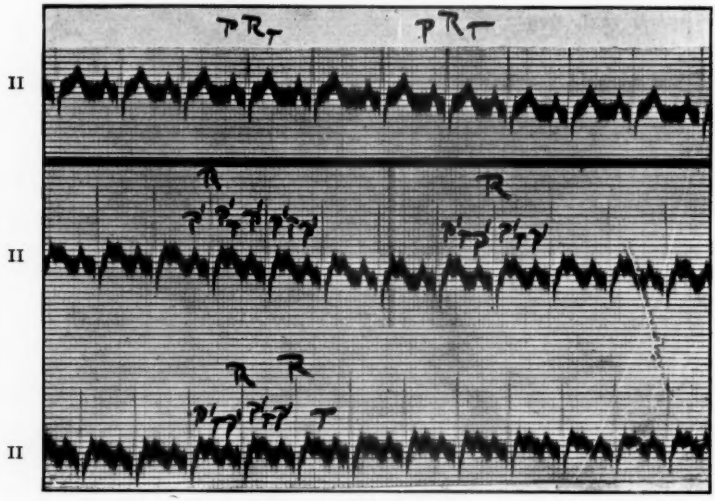


FIG. 4.—End of a paroxysm.

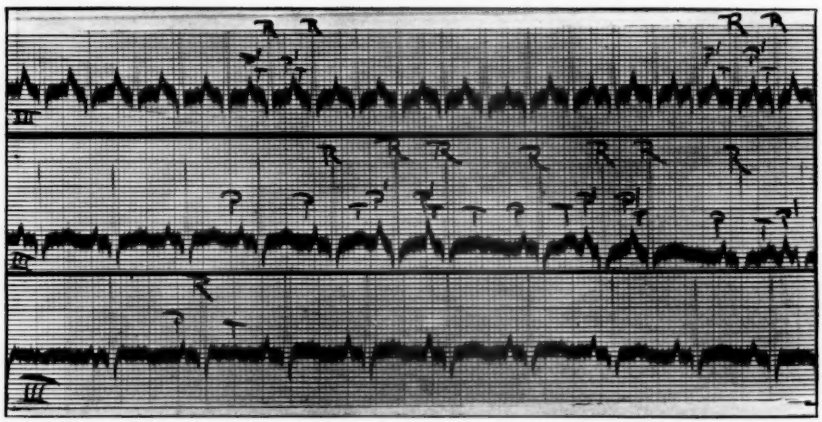


FIG. 5.—Auricular flutter.

or twice a day; while under the influence of this drug the attacks appeared to be a little less frequent. During the last month rheumatic pains have been more definite. It is proposed to enucleate his tonsils.

A Case of Cirsoid Aneurysm.

By C. F. T. East, B.M.

Patient, female, aged 48, married; has had nine children. When she was fifteen years of age she first noticed a noise in the right upper arm when she was in bed. This was evidently the systolic bruit over a dilated brachial artery. Since this time a gradually increasing enlargement of the brachial artery and more especially of the arteries of the lower arm has taken place. Four years ago dilated vessels appeared on the ring finger and since then the little finger has become involved. No history of trauma.

Present Condition.—Considerable pulsation is to be seen behind the right sterno-clavicular joint and a whizzing systolic bruit can be heard here. It is possible to feel a much enlarged brachial artery and hear a bruit over it; fat prevents it being seen. Its course is tortuous. Around the wrist the vessels stand out in knots, tense and bluish. A marked thrill is to be felt over the front of the wrist. The palm is a mass of tortuous, tense vessels. On the back of the hand large blue vein-like vessels form a mass like a cavernous nævus. They are not tense and no thrill is to be felt over them, neither do they pulsate. She complains only of a feeling of tension in the hand, which aches if she lets it hang down long or if she uses it much. She has also another congenital vascular abnormality in the form of a nævus, about 2 in. by 3 in., on the left cheek just in front of the ear. High blood-pressure and cardiac hypertrophy. She has, therefore, a cirsoid enlargement of the innominate artery probably, and of the further branches of this artery in the right arm. An X-ray photograph does not show any enlargement of the aorta.

The condition is due to some congenital defect, no doubt, in the vessels in question. It is true that it did not make its appearance till puberty, but that is sometimes the case with cirsoid aneurysms. In respect of this late appearance one may compare the time of onset with another congenital vascular lesion—multiple familial telangiectasis. The age of onset in a family, a member of which I showed last year, was about 20. No doubt the dependent position of the arm would tend to accentuate the condition in the lower part of that limb when once it was established.

In this case the enlargement of the vessels is not confined to the arteries and their smaller branches only. It would appear to have extended right down to their smallest twigs and further to the capillaries and even to the veins. It would appear to be an abnormality affecting all sizes of the various vessels. It is interesting to note the presence of a nævus in this patient as well. This is a local dilatation of capillaries forming an angeioma, but it is confined to the capillaries. In the arm all types of vessel are involved, but one would suggest that the change is in the nature of an angeioma, and strictly comparable to a nævus. Possibly multiple familial telangiectases are the same sort of condition with a family incidence; these, as has been pointed out, do not necessarily appear at birth, but the tendency must be congenital, as would seem to have been the case in this patient.

Dr. Parkes Weber said he regarded "cirsoid aneurysm" as a developmental condition, potentially present at birth, and intimately allied to vascular nævi. It was therefore interesting to note that the present patient had a fairly extensive congenital vascular nævus on the left side of her face in addition to the cirsoid aneurysm of her right upper extremity.

[*January* 9, 1925.

Case of Exophthalmic Goitre.

By BERNARD MYERS, C.M.G., M.D.

C.B., GIRL, aged 13, was admitted to the Royal Waterloo Hospital under my care on November 25 last, complaining of nervousness, restlessness at night, disturbing dreams, prominence of eyes, a larger neck than formerly and shaky hands. These symptoms were of several months' duration. She had attended school up to October last year, where she was in Standard VI.

History.—Previous illnesses: measles, impetigo of face, and ringworm of the scalp. About two years ago she had a knock on the head. Patient was born at Kingston-on-Thames. No similar case in the family. When questioned she stated that she was not worried in any way, that she liked school and did not find her lessons too much for her.

Upon admission she had very definite exophthalmos, enlargement of the thyroid, tachycardia (pulse 124), was nervous and restless, and there were fine tremors of each hand. So far she has not menstruated. She stated that the onset of the symptoms had been gradual, that she felt very happy and bright, that she got easily excited, slept poorly, was not weak nor had she become thinner. There had been no vomiting nor diarrhœa. The urine did not contain any albumin or sugar.

On examination.—Found to be a well-developed girl of the nervous type; subject to flushings. With regard to the eyes, Stellwag's, von Graefe's and Joffroy's signs are present, but Moebius's sign is absent. Movements of eyes, normal—blinking infrequent. Pupils react to light. Uniform enlargement of thyroid; measurement around neck twelve inches. A systolic bruit is heard over the swelling. No forcible pulsation observed in the neck. Patient flushes readily and her hands feel moist. Moderately marked fine tremors of the hands, especially the right. No murmurs are heard in the heart but the action is a little rapid. Lungs normal. Tongue was noted to be slightly furred, the teeth good; tonsils injected. Knee-jerks present on reinforcement; plantar response flexor. Abdominal reflexes obtained. Temperature has practically remained at normal level since admission; pulse has varied from 105 to 80. Bowels have moved once or twice daily.

Patient is on ordinary diet. Treatment adopted has been rest in bed, quietness and the exhibition first of mistura potassii bromidi and then quinine hydrobromide. At the same time she has taken colloidal iodine three times daily.

Dr. Mackenzie Wallis kindly estimated the basal metabolic rate and found it to be + 45 per cent. He adds: "This is a high basal metabolic rate compared to the normal, which ranges between + 10 and − 10."

Exophthalmic goitre is not often seen in children under 15 years of age, and the majority of the cases recorded have occurred between the ages of 8 to 15. Still there are occasional cases under the age of 8, and one or two have been reported during infancy. Heredity is not infrequently noted. My house physician, Mr. Lorenzen, has not been able to trace any history of heredity in this case.

I take it that this girl is likely to recover from her symptoms, but the prognosis as to the future is not so easy to formulate.

Case of Pericarditis with Slight Adhesions.

By BERNARD MYERS, C.M.G., M.D.

F. A., GIRL, aged 9, was admitted to the Royal Waterloo Hospital on May 20 last year, suffering from pericarditis with effusion.

Previous History.—No special points, except that she had had an attack of rheumatic fever two and a half years ago. When the distress, dyspnœa and precordial

pain were at their worst immediate relief was obtained from the application of leeches placed in usual positions. She remained in bed for five months.

This child has done well, and now suffers no discomfort whatever, but the left ventricle is half an inch outside the left nipple line, and is distinctly forcible. There is a mitral regurgitant murmur propagated to the axilla, and the pulmonary second sound is accentuated. This may have occurred two and a half years previously. As there is a certain, although not at all pronounced, amount of systolic retraction of the ribs, I would like the opinion of the Section as to whether it would be advisable now for small excisions to be made from some of the ribs, or should this operation be delayed until more urgent symptoms appear?

Dr. NEWTON PITT (President) expressed the opinion that nothing surgical should be done for the present.

Case of Purpura Hæmorrhagica.

By BERNARD MYERS, C.M.G., M.D.

E. D., FEMALE, aged 28, married, one child, was shown before this Section eighteen months ago as a case of purpura hæmorrhagica of severe degree which was cured by intravenous injections of citrated human blood after all the usual calcium salts, styptics and sera had failed to arrest or even to lessen the bleeding. During this period she has kept well except for occasional reminders of purpura simplex on the arms and legs and sometimes a purple spot on the lips. For these slight attacks calcium lactate has so far always been successful. The spleen has never been felt. Small knocks cause rather big bruises on her limbs.

It may be remembered that previously no platelets had been found in her blood. A blood-count by Dr. Robert Donaldson, pathologist to the Royal Waterloo Hospital, lately gave the following result:—

Red blood corpuscles, 5,000,000; hæmoglobin, 80 per cent.; colour-index = 0·8; leucocytes, 6,250 per c.mm. Differential: polymorphs, 54 per cent.; lymphocytes, 41 per cent.; eosinophils, 3 per cent.; transitionals, 2 per cent. No abnormal red cells found. Platelets appear to be almost completely absent. Blood coagulation time = 2 min. 20 sec.

Dr. F. PARKES WEBER remarked that the case seemed to be a genuine one of thrombocytopenic purpura hæmorrhagica in a woman who had suffered from the same disease during childhood and who had then apparently recovered; the present relapse appeared to be connected in some way with the recent pregnancy (or puerperal condition). The prognosis was uncertain. Further differential counts of the blood-cells and the platelets (thrombocytes) should be made, and the possibility of the presence of atypical leukæmia should as far as possible be excluded. The subcutaneous hæmatoma on the right leg had a rather hard "core," as if some kind of organization were taking place in it. An apparently similar condition was present in a grave purpuric case he (Dr. Weber) once saw, and the patient (a woman, aged 30) subsequently died with symptoms resembling those of acute aplastic anæmia.

Case of Aneurysm of Transverse Aorta, Innominate and Subclavian Arteries.

By BERNARD MYERS, C.M.G., M.D.

J. B., FEMALE, aged 62, married, was shown before this Section about two years ago. The aneurysm has increased a little in size since then and has exerted pressure on the brachial plexus, causing pain and some wasting, chiefly of the shoulder muscles of the right arm. There is now a distinct boring pain just internal to the inferior angle of the left scapula. The most useful drug for the pain appears to be phenazonum. The Wassermann reaction is positive, and she has been receiving anti-syphilitic treatment.

Clinical Section.

President—Dr. G. Newton Pitt, O.B.E.

New Growth of Lung (?).

By F. E. Saxby-Willis, M.D.

Patient, a male, aged 38, upholsterer.

September 11, 1924: Admitted to the Royal Chest Hospital under the care of Dr. Saxby-Willis, complaining of cough, blood-stained sputum, dyspnœa, loss of weight.

H. P. C.—March, 1924, first began to cough and bring up blood-stained sputum. Pain in left side, in bed fourteen days. Pain ceased and has never returned; cough and blood-stained sputum continue. No night sweats, feels tired, appetite fair. Lost 1 st. since March.

Past History.—Age 18, enlisted, three years with colours, never abroad. 1914: Called up with Reserve, sent to France. 1916: Discharged army A1, time-expired.

Family History.—No history of chest trouble.

On Admission.—September 11, 1924. Temperature 101·3° F., pulse 104, respiration 32. Thin, anæmic, marked clubbing of fingers; tongue clean, teeth carious, pyorrhœa: fauces normal; neck, no enlarged glands palpable: pupils react to light and accommodation. *Chest, left side:* Movement impaired. Vocal fremitus absent. In anterior axillary line from fifth to eighth rib, percussion note impaired; breath-sounds weak; vocal resonance diminished. In posterior scapular line, from first to fifth dorsal vertebræ percussion note impaired. From fifth to tenth dorsal vertebræ percussion note dull. Breath-sounds absent. Vocal resonance absent. *Right side:* Breath-sounds increased. *Heart, urine, abdomen, nervous system* all normal.

October 20, 1924: Sputum less, but still clots of blood. Heart drawn over to left, upwards and outwards. Apex beat visible in fourth space, 4 in. from mid-sternal line. Temperature range last four weeks 98° to 99° F., pulse 80 to 100, respirations 24.

October 30, 1924: Absolute dullness in posterior scapular line to third dorsal vertebra.

X-ray Examinations.—September 12, 1924. Marked left-sided fibrosis, opacity lower half of chest left side, suggestive of consolidation, possibly tuberculosis. Right side of chest unaffected. Left diaphragm motionless. October 4, 1924: Hila glands enlarged. Some opacity left base, might be due to congestion. October 30: X-ray suggestive of new growth on left side. (See figs. 1, 2, p. 22.)

Wassermann Reaction.—Negative on two occasions after provocative dose of novarseno-billon.

Sputum.—Bacillus tuberculosis negative on seven occasions.

Paracentesis thoracis.—No fluid found.

Weight.—On admission 8 st. 10 lb. October 25, 1924, 8 st. 13 lb.

Postscript, April 23, 1925.—Physical signs still limited to left lung. Difficulty in swallowing solid food. X-ray barium examination of œsophagus shows constriction at level of arch of aorta.

FIG. 2.—Oct. 31, 1924. Very suggestive of new growth *left* side.

FIG. 1.—Sept. 12, 1924. Opacity lower half of chest; left side suggestive of consolidation; possibly tuberculosis, but rest of chest unaffected.

[*February* 13, 1925.

Case of Acromegaly.

By Sir PERCY BASSETT-SMITH, K.C.B., C.M.G., R.N.

PATIENT, a male, before going out to the war was in perfect health. While at Salonica, about 1917, he contracted dysentery and was invalided to England. The acromegaly symptoms have dated from that time, when he was under the treatment of Sir Thomas Horder at St. Bartholomew's Hospital. He came under my observation at the Ministry of Pensions for sequelæ of dysentery. The enlargement of hands, head and feet was typical but not increasing. As the condition of the face pointed to some suggested nodular leprosy I have brought him up for demonstration. There are no indications of leprosy; there is no anæsthesia, the ears are not affected, and no acid-fast bacilli can be found in the serum taken from the nodular elevations on the face. I presume that the skin condition is due to metabolic irregularities from deficient hormones.

Case of Fröhlich's Disease (Dystrophia Adiposo-Genitalis).

By Sir PERCY BASSETT-SMITH, K.C.B., C.M.G., R.N.

PATIENT, a female, aged 23, was shown here some months ago. She weighs 16 st., and has been under the care of Dr. Langdon Brown. At that time the presence of calcareous changes in the pituitary body was demonstrated by X-rays,

FIG. 1.

and she was treated by thyroid administration. She first menstruated at age of 9. There is no infantilism; the face, hands and feet are normal, but the arms, thighs, chest, and breasts, are enormously enlarged (figs. 1, 2). There is a slight decrease in

sugar tolerance and low blood-pressure. There are no ocular changes, and the X-rays now show no abnormality at the base of the skull. The condition is associated with asthma, but there are no pains. Treatment with various gland

FIG. 2.

extracts, and the diet she has received both as an out- and an in-patient, have not led to any successful result. Her mother is quite normal in appearance.

Case of Hyperpituitarism.

Shown by R. COVE-SMITH, L.R.C.P., M.R.C.S., for DOUGLAS FIRTH, M.D.

F. S., MALE, aged 43. In August, 1923, this patient suffered from pain in the head and legs, loss of power in the legs, and failing sight.

The legs showed spastic weakness and exaggerated knee-jerks with double extensor plantar response. There was diminution of the temporal fields of vision, and the optic discs were somewhat blurred.

X-ray of the skull, October 13, 1923, showed nothing very definite, but the outline of the pituitary fossa is not so clear as the normal.

Blood-Wassermann reaction negative. Cerebro-spinal fluid showed increase of lymphocytes and of protein, presence of globulin and a positive Wassermann reaction. Lange test gave a result of paretic type 3555555543. The patient improved on mercury and iodides.

Until May, 1924, patient felt better generally—except for frequency of micturition, especially at night (five to seven times). Vision much improved, can now read newspaper. Slight weakness of right side of tongue and left side of face. Pupils react sluggishly to light and weakly to accommodation. Double extensor responses. Knee-jerks exaggerated. In May, 1924, passing 70 to 110 oz. of pale

urine of low specific gravity (1005). 1 c.c. of pituitrin did not raise the specific gravity. Much thirst. Sugar tolerance fairly normal.

January, 1925: still passing large quantities of pale urine. Memory not so good, and his wife states that he seems to have been getting more irritable during the last month or so.

February, 1925: patient shows some slight exophthalmos and slight weakness of left side of face. Pupils react to accommodation, but not to light. Optic discs, pale; but no definite irregularity of margins. Arm-jerks exaggerated; abdominal reflexes absent; knee-jerks exaggerated; ankle-jerks exaggerated; plantars double extensor. No loss of joint sense. No incoordination—finger-nose test good. No astereognosis.

Urine is passed normally during the day, though the quantity is rather large; during the night the patient passes it under him, although he is not conscious of doing so. This occurs once or twice during the night when asleep.

Is passing 60 to 90 oz. (incomplete) of pale urine, specific gravity 1001 to 1002. No albumin, no sugar, no blood. White deposit of phosphates. Blood-sugar curve shows hyperglycæmia. X-ray shows some thickening of floor of pituitary fossa and of bone below this, around sella turcica. The alteration in the blood-sugar curve and in the X-ray appearance of the pituitary fossa in the last eight months, pointing to a prolonged hyperglycæmia of the type associated with hyper-pituitarism, suggests an active irritative lesion (probably syphilitic), which is now involving the anterior as well as the posterior lobe of the pituitary body.

Case of Dystrophia Adiposo-genitalis.

Shown by R. COVE-SMITH, L.R.C.P., M.R.C.S., for DOUGLAS FIRTH, M.D.

E. S., MALE, aged 17. Patient is very obese and big for his age. Fat is most abundant in abdomen and buttocks. Hair is absent in the axillæ and very little pubic hair is present. Testicles small. Frontal headache and giddiness since age of 14. Memory is deficient. Not intelligent—patient has never been to school.

Has also had weakness of left side of body with occasional fits since age of 8 months. The left arm and leg are slightly spastic, and left leg is dragged when walking. Pupils react to light and accommodation. Rotary nystagmus. Left internal strabismus. Wassermann negative. Sugar tolerance increased, but normal blood-sugar curve consistent with hypopituitarism. X-ray examination shows no definite abnormality of the pituitary fossa (? small).

Polyglandin tried with no result. The condition is one of dystrophia adiposo-genitalis, and the possibility of traumatic origin seems to be indicated by the left-sided hemiplegia. The birth was instrumental.

A Developmental Deformity of the Right Forearm, similar to that sometimes met with in cases of Multiple Exostoses ("Diaphysial Aclasis" of Keith).

By F. PARKES WEBER, M.D.

THE patient, E. M., aged 18 years, an English warehouseman, complains of a defect in shape and movement of his right forearm, which, he says, he first noticed at sixteen years of age. Since that age it has gradually become more pronounced. The right forearm (fig. 1) is about one and a half inches shorter than the left one, which appears to be normal. There is ulnar deviation of the right hand, together

with imperfect power of supination of the forearm. Roentgen ray examination (fig. 2) of the right forearm shows that there is a deficiency (agenesis) of the distal end of the ulna; the proximal ends of the ulna and radius appear abnormally separated from each other, the shape of the head of the radius is somewhat peculiar, and there is an abnormal curve of the distal part of the radius, with its concavity towards the ulna.

Otherwise the patient seems to be normally developed, excepting that he is very tall (about 6½ ft. in height). His lower jaw is rather large. The pituitary fossa (which was examined by X-rays on account of the question of there being slight "pituitary gigantism") appears to be of average size.

The deformity of the right forearm in the present case is similar to that sometimes met with in cases of multiple exostoses (the "diaphysial aclasis" of Sir Arthur Keith[1]): for instance, in the case which I showed at this Section not long ago,[2]; but

FIG. 1.—Right forearm showing ulnar deviation of the hand.

there are no exostoses or chondromata in the present patient, unless at the right elbow, where the peculiar shape and position of the proximal end of the radius may possibly be of that nature (i.e., due to a small exostosis or chondroma not definitely shown in the skiagram). Is not the present case a minor localized form of the same developmental dysplasia, although no obvious exostoses or chondromata can be detected?

May there not be localized varieties of what I would call "periosteo-osteo-dysplasia" without any exostoses or chondromata, although the dysplasia in question is usually manifested by their presence? Cases of periosteo-osteo-dysplasia may

[1] Sir Arthur Keith, "The Nature of the Structural Alterations in the Disorder known as Multiple Exostoses," *Journ. Anat.*, Cambridge, 1920, liv, pp. 101-115.

[2] F. Parkes Weber, "Diaphysial Aclasis or Periosteo-osteo-dysplasia (Multiple Exostoses) with Shortness of Forearms," *Proc. Roy. Soc. Med.*, 1924, xvii (Clinical Section), p. 40.

perhaps be divided into three groups: (1) the ordinary cases, with multiple exostoses, without agenesis of the distal end of either or both ulnas; (2) rare cases, with

FIG. 2.—Skiagram of the right forearm, showing deficiency of the distal end of the ulna and abnormal separation of the proximal ends of the ulna and radius.

agenesis of the distal end of either or both ulnas, but without exostoses; (3) cases with both exostoses and agenesis of the distal end of either or both ulnas.

The condition known as "multiple chondromata" should probably be regarded as the result of an allied developmental dysplasia.

Case of Hypertrophy of the Breasts.

By PHILIP TURNER, M.S.

PATIENT, a girl aged 14 years, first noticed an enlargement of the right breast eight months ago. Two months later the left breast began to enlarge. Since then both have grown to an enormous size. There has been no pain, but a good deal of dragging owing to the weight of the enlarged organs. She has had one menstrual period, and with the exception of the present trouble her health has been good.

Case of "The Hairy, Black Tongue" (Nigrities-Hyperkeratosis Linguæ).

By ST. J. D. BUXTON, F.R.C.S.

PATIENT, a male, aged 64, came to hospital, as his doctor had found his tongue was abnormal. The patient did not know anything was the matter with it, and the condition was found on routine examination of the tongue. He thinks it has been "rough" for six months.

The size of the tongue is normal. The borders show no abnormality, but thin, hair-like processes, about one-third of an inch long, cover the centre of the dorsum over an area, "V" shaped, with the point of the "V" at the foramen cæcum. This area is blackish in colour, feels soft, and there is no induration. With a lens it can be seen that there are many of these processes, and each one is stained. Gentle rubbing will cause a few to be detached from the tongue.

It is recognized that the condition tends to disappear naturally, but suggestions with regard to quicker cure are asked for.

Case of Diabetes Insipidus and Acquired Infantilism.

By C. F. T. EAST, B.M.

PATIENT, a man aged 30, who had an attack of malaria of cerebral type in East Africa in 1917. Since then he has suffered from typical diabetes insipidus. At the same time he lost his pubic and axillary hair and most of the hair on the face, the testes became small, and sexual power disappeared. He had been normal at 18 years of age.

The points of special interest for which the case is exhibited are: incidence of diabetes insipidus after malaria—a posterior lobe deficiency; incidence of puerile condition at the same time—an anterior lobe deficiency. Pituitrin controls the polyuria fairly well.

Dr. PARKES WEBER remarked that in certain families a kind of hereditary diabetes insipidus occurred, sometimes in individuals who were otherwise apparently healthy and strong. Data of such a family from the seventeenth century to the present time had recently been collected and investigated by M. Gänsslen.[1] A modern member of the family was strong enough to serve as a soldier during the Great War, and his distressing thirst in the trenches at the front, owing to occasional difficulty in obtaining water, was referred to. A healthy woman, married to one of the affected members, suffered from extreme thirst towards the end of her first pregnancy. This disappeared as soon as the child was born, and Gänsslen suggested that the mother's abnormal thirst was due to a diuretic hormone formed by the fœtus in her uterus. In fact, her eldest child after birth was found to have the familial complaint of diabetes insipidus; whereas her second child was not so affected, and during her pregnancy (with the second child) she had no excessive thirst and polyuria.

[1] M. Gänsslen, *Med. Wochenschr.*, Berlin, 1924, iii, p. 22.

Clinical Section.

President—Dr. G. Newton Pitt, O.B.E.

Case of Raynaud's Disease.

By Sir Percy Bassett-Smith, K.C.B., C.M.G., R.N.

Patient, Miss E. K., employed as a nurse, 1917 to 1922. Overworked; enlarged glands and tender appendix. Operation for appendicitis, by Mr. Joyce, April, 1923. Did not make good recovery. Noticed weakness of right leg, followed by marked anæmia. February, 1924, developed Raynaud's disease; most marked left hand, but also affected right, and occasionally right leg and nose; tinglings, pallor, cyanosis, then œdema.

Treatment: Thyroid and parathyroid, calcium lactate, &c. Under treatment all 1924. Periods of unconsciousness, sickness, loss of power in left leg and general debility.

Admitted to Victoria Park Hospital, January 12, 1925. Vaso-motor changes left hand, œdema up to wrist, right hand less affected, trophic changes in bones of left hand, difficulty of walking with dragging of right foot. Blood-count: red cells, 4,800,000; hæmoglobin, 75 per cent.; white cells, 8,000. Reduction of polymorphs, slight increase of mononuclears. Very marked improvement under administration of parathyroid and artificial sunlight treatment.

Case of Chorea.

By Bernard Myers, C.M.G., M.D.

Patient, J. P., aged 10, admitted to the Royal Waterloo Hospital on January 30 last with a history of "twitchings and dropping things" since the middle of December. A sister has had five attacks of chorea.

On Admission.—Marked choreiform movements. Patient unable to feed herself. Knee-jerks greatly increased. Tonsils injected. Apex beat in left nipple line. Diffuse pulsation over præcordium. First sound at apex loud and booming.

Patient given light diet, kept quiet in bed and treated with sodium salicylate and bicarbonate of soda for four days, then with tinct. valerian amm. for a week, and afterwards with colloidal iron. She has made good progress. A systolic murmur was heard at apex and base a week ago, but seems now to have disappeared. Patient lost all choreic movements a month ago.

Dr. E. Stolkind said that hitherto the treatment of chorea in children had varied. More than fifteen years ago he had treated all cases of chorea in children with absolute rest only, without giving any drugs, but during the last few years, in addition to the rest treatment, he had also prescribed arsenic, iron and quinine hydrobromide, and bromide in severe cases. In his opinion, alcohol was contra-indicated in all cases of chorea in children.

Absence of Anus: Rectum Opening in Posterior Vaginal Wall.

By Bernard Myers, C.M.G., M.D.

Patient, V. H., aged 12, was admitted to the Royal Waterloo Hospital on February 27 with the above diagnosis. The opening was in the lowest part of the posterior vaginal wall.

Patient is well-built and healthy, notwithstanding this malformation, but just a little below the normal height for her age.

Bowels open daily and she has perfect control.

No anus or external sphincter can be seen or felt (evidently total absence of proctodeum).

On account of her nearing the menstrual age opinions are asked as to the treatment, as it would appear not unlikely that infection of the uterus might occur during a menstrual period.

The points suggested for consideration are the following: (1) Should nothing be done? (2) Should a plastic operation be performed, retaining the present efficient sphincter? (3) Should radium or X-rays be used to sterilize the ovaries? If this were done would mental symptoms follow? (4) Should extirpation of the body of the uterus be performed and the ovaries left intact?

It is interesting to note that Clark Yeomans states that approximately one out of every 5,000 babies shows an ano-rectal defect of development, and that these defects are fairly evenly divided between the sexes.

A Case of Bronchiectasis.

By L. S. T. Burrell, M.D.

Patient, a boy aged 19, developed bronchiectasis following pneumonia, in March, 1923. When I first saw him in November, 1924, there were bronchiectatic râles at the left base and a few fine crepitations at the right base. Owing to the signs at the right base and the fact that the left seemed to be entirely adherent to

Fig. 1.—Erect position.

the diaphragm I was not anxious to induce a left pneumothorax. Treatment with creosote did not produce any improvement nor any diminution in the quantity of sputum, about 10 or 12 oz. of which was expectorated each day, and the odour of

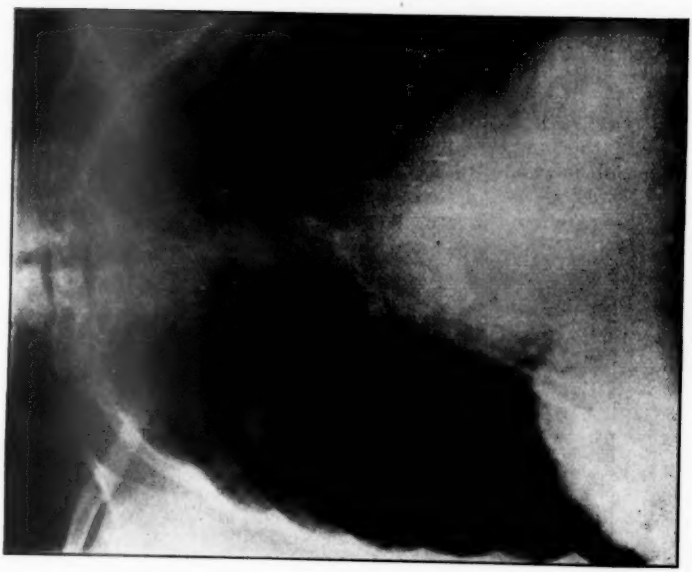


FIG. 3.—February 23, 1925.

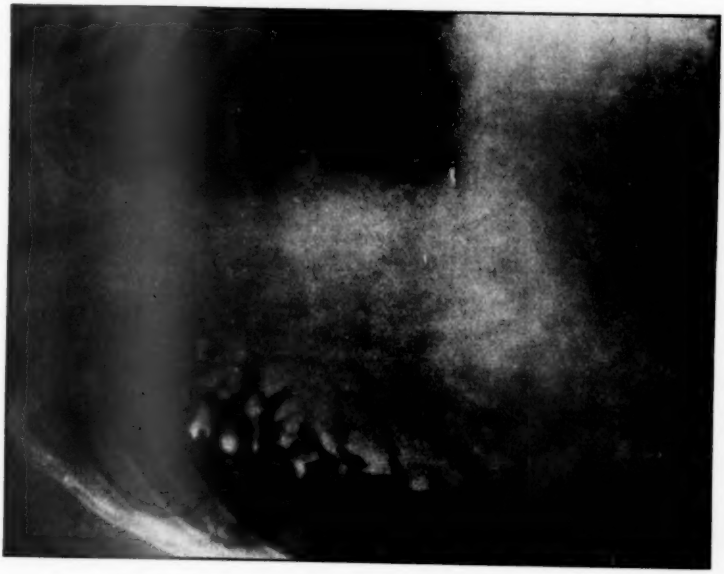


FIG. 2.—Recumbent position.

which was offensive. I therefore had the left phrenic nerve cut, but no improvement followed. I injected some lipiodol into the trachea on February 9, 1925.

Figs. 1 and 2 show the degree of dilatation of the bronchial tubes in the erect and recumbent positions respectively immediately after this injection.

On February 12, 1925, I induced a left pneumothorax, and fig. 3 shows the condition eleven days later. The sputum was then reduced to 4 oz.

On March 9 the lung was further collapsed, as shown in fig. 4. The sputum was no longer offensive and the amount expectorated was reduced to 1 or 2 oz. a day.

FIG. 4.—March 9, 1925.

It will be seen that complete collapse is impossible owing to the adhesions to the diaphragm, and further improvement is not likely to occur. I think it will probably be best to allow a little re-expansion of the lung and to keep it partially collapsed in the hope that fibrosis at the base will eventually obliterate the cavities. An interesting point to note is that the bronchial cavities and dilatations are very much smaller after the pneumothorax, which indicates that the tubes are compressed as well as the lung tissue

My thanks are due to Dr. Stanley Melville for taking the X-ray photographs.

Large Ulcerating Carcinoma of the Breast—Diagnosed as Sarcoma.

By DUNCAN C. L. FITZWILLIAMS, C.M.G., F.R.C.S.

PATIENT, Mrs. W. B., aged 38, noticed that her left nipple was retracted some four and a half months ago and that a red swelling had appeared in the breast. For two months it did not seem to grow and she took little notice of it. Then quite suddenly small spots or "pustules" sprang up round it, broke down, began to discharge, and very rapidly the whole area was turned into a fungating mass which rapidly increased in size until it occupied the whole surface of the breast. The edges of the ulcer were thick, everted and turned back just like the petals of an opening flower. No nipple was visible, but it was present on the under surface of the inner

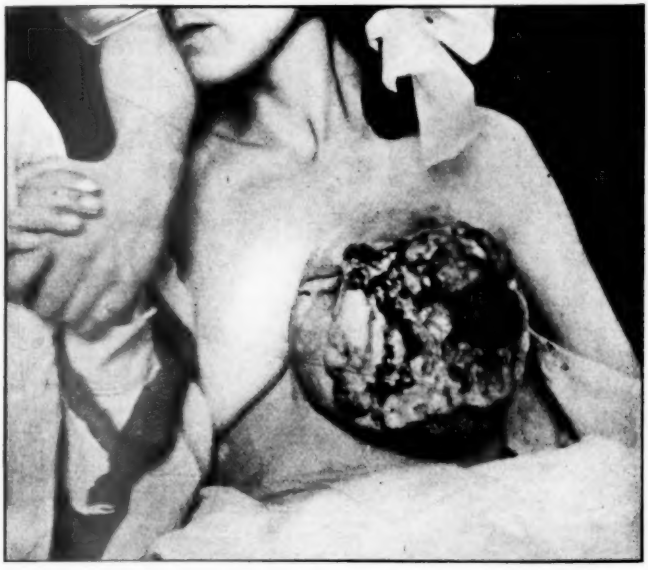


FIG. 1.

turned-back edge. The ulcerous mass was about 8 in. across, with the base yellow and sloughing, and emitting a stale odour. Many tuberous masses were invading the skin around. Large glands could be felt in the axilla (figs. 1, 2).

She had hidden this horrible condition sedulously from everyone. No one knew of it in her own home, though when I saw her the breast protruded strangely, this protrusion being occasioned both by the size of the tumour and the quantity of absorbent dressings she had to wear over it. Eventually her mother became suspicious of the state she was in. She nevertheless refused to show the growth to her mother, but the latter induced her to visit Dr. Cokkinis, who sent her straight on to me.

On admission: She was thin, sallow, and septic-looking, and had evidently been losing weight. Her pulse was 126 and her temperature 101·6° F.; this increase in pulse and rise in temperature I put down to the constant absorption from the septic area.

Now in these cases the operation is always a cause of anxiety. Whatever is done it is impossible to get the surface of the ulcer aseptic, and the wound is very likely to become infected.

A towel soaked in strong lysol was sewed on to the skin all round the ulcer; then by refraining from handling the tissues the whole mass was removed, together with the two pectoral muscles and the contents of the axilla. The skin had to be so widely removed that it would not meet, and no great attempt was made to make it do so. The parts of the wound that would come together easily were sewn up,

Fig 2.

and the remainder was covered with gauze dipped in paraffin. No infection of the wound took place, and when healthy granulation tissue had spread over this raw area, it was covered with skin grafts which have taken readily.

When I first saw this case I had no doubt that the condition was a sarcoma, though these growths of the breast are extremely rare. I thought that this was indicated by the rapidity of the growth, the quickly spreading extensive ulceration, and the freedom of the breast from the deeper structures. Large glands were certainly felt in the axilla but these have often been reported as enlarged in sarcoma. At the operation a few small hard glands were found at the apex of the axilla on the costo-coracoid membrane, which were very suspicious of carcinoma.

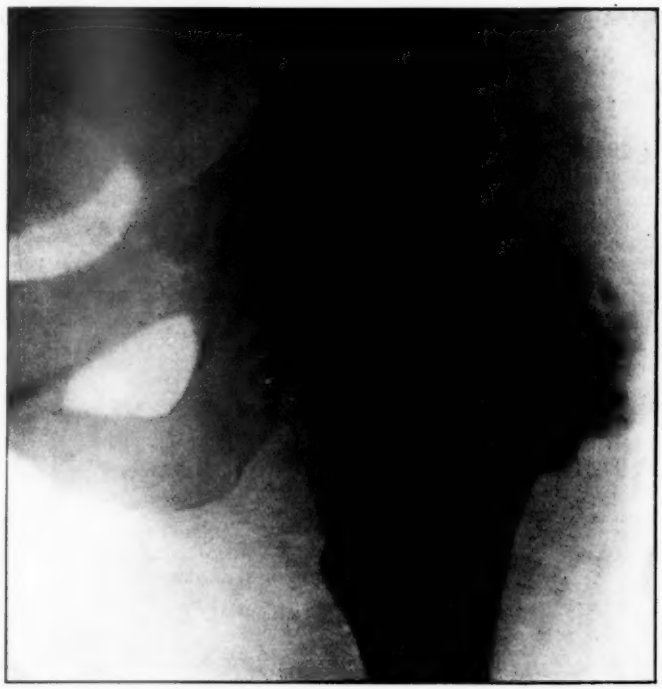
The pathologist reported that the condition was a carcinoma of the most active description; that the glands from the upper part of the axilla were infected but that the large ones from the lower part were merely inflamed.

Case of Multiple Exostoses and Osteochondromata.

By DUNCAN C. L. FITZWILLIAMS, C.M.G., F.R.C.S.

PATIENT, Miss A. R., aged 38, complained of nothing until six years ago. Then a small lump appeared at the top of the front of the thigh. She says it was quite hard, not painful, and did not interfere with the movement of the limb. Last year, however, it increased considerably in size and became soft and painful.

At the present time she has a large swelling in the thigh just below Poupart's ligament, which moves with the femur. The swelling fluctuates and is due to a bursa covering an exostosis of the upper end of the femur. On careful palpation melon-seed-like bodies can be made out.



X-ray examination shows a large exostosis on the upper and front part of the right femur.

Patient has osteo-chondromata also in other parts of the body: right femur, tibia, left ulna, and other long bones. As usual, they grow chiefly from near the end of the diaphysis and then in the line of the least resistance.

In this particular case the condition is hereditary. It is known that the grandfather, the patient's mother, aunts and uncles were affected, and two of the four sons are also affected.

X-ray photographs have been taken of many members of the family but these persons refuse to be exhibited. I operated upon the mother for a cancer in an outlying lobe of the breast; she died from secondary growths some years later.

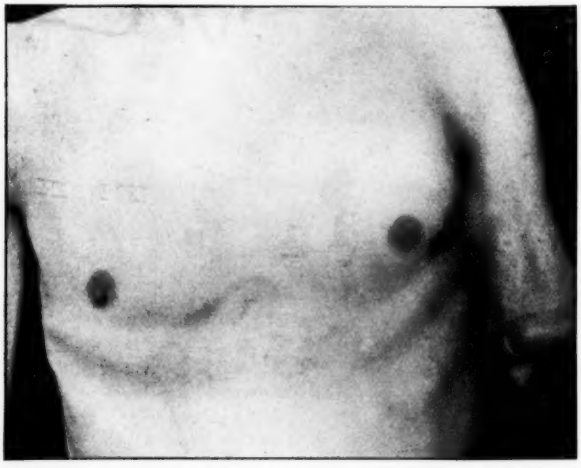
Dr. F. PARKES WEBER remarked that, in addition to the exostoses, Mr. Fitzwilliams's patient showed a developmental forearm abnormality similar to that present in the cases of multiple exostoses which he (Dr. Weber) had demonstrated at the meeting of the Section on February 13, 1925, and at a previous meeting in 1924 (*Proceedings*, 1924, xvii, p. 40).

In these cases the distal end of the ulna was imperfect, and did not reach the carpus, as it normally should do. The whole developmental disorder, which could be termed "diaphysial aclasis" (after Sir A. Keith), or by some name such as "periosteo-osteo-dysplasia," might be divided into three groups: (1) Cases in which multiple exostoses were associated with the above-mentioned forearm abnormality; (2) cases in which there were multiple exostoses without any forearm abnormality; (3) cases, such as that shown by himself (Dr. Weber)[1], on February 13, in which the forearm abnormality was present on one or both sides, without any definite exostoses or chondromata anywhere.

Secondary Carcinoma of the Fourth Rib.

By DUNCAN C. L. FITZWILLIAMS, C.M.G., F.R.C.S.

History: H. F., patient, age 54. Six weeks ago his doctor, when examining him, noticed a small lump of the fourth rib, left side, just below the nipple; it was then about the size of a hazel nut, but it has been steadily increasing in size since. It has been quite painless. He complained of no other trouble except that he has lost a great deal of weight recently.



On examination there was seen to be a flat, rounded elastic swelling involving about 3 in. of the left fourth rib; it was raised about 1½ in. above the rib level; its position was partly deep to, and external to, the nipple, which was consequently displaced forward. It was continuous with the rib substance, but the superficial structures were freely movable over it. Lateral pressure on the fourth rib gave slightly

[1] See *Proceedings*, 1925, xviii (Clin. Sect.), p. 25.

increased " spring " to the rib. The swelling was hard and elastic; fluctuation was doubtful.

Diagnosis : Cold abscess under considerable pressure, with caries of the fourth rib.

X-ray examination showed apparent absorption of bone. Probably tuberculosis with cold abscess. The possibility of sarcoma was seriously considered but excluded on account of its rarity.

At the operation the growth was found to be solid and adherent to the pleura. About 5 in. of the third, fourth, and fifth ribs were removed together with the pleura, exposing the lung and pericardium. The gap in the chest wall was closed by transplanting the pectoralis major over the opening.

Pathological Report : A secondary columnar-celled adeno-carcinoma. There is nothing to point to the situation of the primary growth.

[*May* 8, 1925.

Case of " Polycythæmia Hypertonica."

By F. Parkes Weber, M.D.

The so-called " polycythæmia hypertonica " or " hypertonia polycythæmica," to which F. Geisböck drew special attention in 1904 and 1905 (see " Die Bedeutung der Blutdruckmessung," *Deut. Arch. f. klin. Med.*, Leipzig, 1905, lxxxiii, p. 363), seems to me probably a secondary or symptomatic polycythæmia, connected in some way with the high blood-pressure of early stages of granular kidney, or with primary arterial hyperpiesia without definite signs of organic renal disease. In other words, the condition is one of renal or primary arteriosclerotic high blood-pressure with secondary polycythæmia rubra. It is not a very rare syndrome, occurring chiefly in middle-aged persons and probably more frequently in males; perhaps Hebrews are somewhat more often affected than others. From true erythræmia (the Vaquez-Osler disease) it is distinguished by the absence of splenomegaly, by the presence of high blood-pressure, and by the absence of such marked facial cyanosis as is seen in old-standing cases of erythræmia. Moreover, the syndrome may, I believe, represent merely a phase or episode in the course of a case of high blood-pressure, the polycythæmia rubra disappearing after a time, though the high blood-pressure persists or increases.

The present case, J. L., aged 57, a nervously-active Hebrew man, born in Austria, was first seen by me in 1920 (when he was aged 52). In that year he complained of lumbago-like and bilateral sciatica-like pains. I shortly described his case in my book on " Polycythæmia, Erythrocytosis and Erythræmia " (London, 1921, p. 106, Case 62). He is thin rather than fat, and has a red face, with a little, generalized acne rosacea, without cyanosis. Sometimes there is slight conjunctivitis, probably of rosaceous origin. His complaints have been chiefly of pains (probably of fibrositic nature) in one part or another. The brachial systolic blood-pressure has been generally 180-210 mm. Hg. The urine has been free from albumin and sugar. Wassermann reaction, negative. The blood-count in June, 1920, gave 8,000,000 erythrocytes and 12,000 white cells to the c.mm. of blood; hæmoglobin, 130 per cent. Since then it has varied considerably, the case apparently tending to become one of arterial hyperpiesia without polycythæmia rubra. His present blood-count (May, 1925) gives: erythrocytes, 5,780,000 to the c.mm. of blood; white cells, 8,000 to the c.mm. of blood; hæmoglobin, 96 per cent. The patient is inclined to be over-anxious about himself, and he needs reassurance.

A son (F. L.), who died in 1922 at the age of 26 years, suffered from sclerodactylia, which had commenced with "Raynaud-like" symptoms in 1918. There was also a remarkable telangiectatic condition of his face, as is sometimes seen in sclerodermatous cases of the symmetrical "sclerodactylia" type. His Wassermann reaction was negative, like that of his father. His urine contained albumin and hyaline and granular tube-casts.

A Case of Thrombo-angiitis Obliterans of Twenty-two Years' Duration.

By F. Parkes Weber, M.D.

The patient, M. M., a Russian Jew, aged 60, who came to England at the age of 25, was shown by me at the Clinical Section of the Royal Society of Medicine on December 13, 1907,[1] and was described in detail in the *Lancet* for January 18, 1908, under the heading, "Arteritis Obliterans of the Lower Extremity, with Intermittent Claudication" ("Angina Cruris").[2] It was not till later in 1908, that Leo Buerger, of New York,[3] introduced the term "thrombo-angiitis obliterans"—the term that is now generally used for this kind of arterial disease.

I first saw the patient in May, 1906, but the symptoms had commenced gradually in 1902 or 1903, at the age of 38, with pain in the left foot on walking, of the nature of intermittent claudication. The disease progressed by exacerbations from time to time, but there were prolonged periods of quiescence, with remission or absence of pain. In 1916[4] both lower extremities showed typical erythromelalgia-like symptoms as well as intermittent claudication and absence of pulsation in the dorsalis pedis arteries. No amputation had been performed, although the patient had passed through periods of very painful ischæmic ulceration, when gangrene seemed imminent. During one of these periods the pain, especially at night, which had commenced with slight ischæmic ulceration in one foot, lasted so long in spite of treatment and rest in bed that my house-physician could not understand why the patient did not demand amputation as the only possible remedy. The case furthermore had been complicated from time to time (1909, 1910, 1914, 1915, 1916) with multiple foci of superficial (sometimes "nodular") phlebitis, such as has been often observed in other cases, notably in many of those described by Leo Buerger.[5]

In 1915 the patient developed a tender localized swelling in the position of the radial artery at the right wrist. Subsidence of this swelling was accompanied by apparent obliteration of the arterial channel, as evidenced by loss of pulsation. The swelling in question was seemingly due to thrombo-arteritis and clinically analogous to the swellings which occur in nodular thrombo-phlebitis of superficial veins.

Of late, the thrombo-angiitis in this case has been non-progressive, but in May, 1924, the patient was temporarily in hospital for what I regarded as a mild attack of gout at the base of both big toes. I then found to my astonishment that there was again fairly good pulsation in the left radial artery at the wrist.[6] With small

[1] F. Parkes Weber, *Proc. Roy. Soc. Med.*, 1907-1908, i (Clin. Sect.), p. 44.

[2] F. Parkes Weber, *Lancet*, London, 1908, i, p. 152.

[3] Leo Buerger, "Thrombo-angiitis Obliterans—a Study of the Vascular Lesions leading to Pre-senile Spontaneous Gangrene," *Amer. Journ. Med. Sci.*, Philadelphia, 1908, cxxxvi, p. 567.

[4] See the article by F. Parkes Weber on "Thrombo-angiitis Obliterans or Non-syphilitic Arteritis Obliterans of Hebrews," *Quart. Journ. Med.*, Oxford, 1916, ix, p. 289.

[5] Cf. Leo Buerger, "The Circulatory Disturbances of the Extremities," 1924, p. 317, and elsewhere.

[6] Cf. F. Parkes Weber, "Return of Pulsation in Thrombo-angiitis Obliterans," *Brit. Med. Journ.*, London, 1924, ii, p. 62.

doses of sodium salicylate and sodium bicarbonate the gout soon subsided, but there were also signs of chronic interstitial nephritis with high blood-pressure. The urine contained a little albumin and a few granular tube-casts. The brachial systolic blood-pressure was, on the left (apparently sound) side, 192 mm. Hg; and on the right side it was 185 mm. Hg. The painful intermittent claudication had compelled the patient to follow a sedentary mode of life which was doubtless associated with relative excess of food, and this combination had probably favoured the onset of gout and chronic interstitial nephritis. When I saw him again in March, 1925, he complained of vertigo. The urine contained a little albumin and a few hyaline and granular tube-casts. The brachial systolic blood-pressure on the left side was 210 mm. Hg. It should be stated that the blood-pressure in each arm had formerly (1907) been 135 mm. Hg. He still has typical intermittent claudication of both lower extremities. There is a curious network of dilated superficial venules on the back of both feet.

The family history in the present case is remarkable. A son, aged 20, and a daughter, aged 18, both died in 1920 with diabetes mellitus. Another son formerly suffered from bronchitis and asthma. Another son, S. M., now aged 32 years, has been known to me since July, 1911. He formerly had a slightly enlarged spleen and used to suffer from recurrent attacks of headache and vomiting. A daughter (Netta M.), of that son (S. M.), when she was 12 months old (1922), suffered from typical recurrent attacks of Raynaud's syndrome.[1] Her Wassermann reaction and the Wassermann reaction of her father (S. M.) were negative.

The Wassermann reaction of the patient (the grandfather, M. M.) was examined on various occasions and was always found to be negative. Sugar was never found in his urine. There has been no evidence of saturnism. He used to be a rather free smoker of cigarettes. Like nearly all patients affected with thrombo-arteritis obliterans, the disease in London, as well as in New York, is found to be almost exclusively confined to Hebrews of the male sex, who themselves or whose parents have immigrated from Central Europe. Only four out of 500 cases collected by Leo Buerger did not belong to the Semitic race.[2]

I would add that at the German Hospital during the last two years a leg has been amputated in two bad cases of thrombo-angiitis obliterans (both Hebrew men). In one of these cases, M. G. L., aged 43 years,[3] microscopic examination of the anterior tibial artery and the surrounding tissue in the amputated lower portion of the (left) leg showed a very remarkable perivascular and perineural inflammatory cell-infiltration. For this finding I am indebted to Sir Frederick Andrewes, who very kindly sent me a report of the microscopic examination. He agreed with me that this cell-infiltration might perhaps represent an ascending perivascular lymphatic inflammation, which had travelled up from a septic inflammatory focus in the foot. It certainly reminds me of the fibrotic "matting" around the vessels (due to old chronic, perivascular changes), described by Leo Buerger as having been found in some of his cases.[4]

[1] F. Parkes Weber, "Raynaud's Syndrome in a Non-syphilitic Infant, with a remarkable Family History," *Brit. Journ. Child. Dis.*, London, 1923, xx, p. 25. Since 1922 the child has been quite free from Raynaud's syndrome.

[2] Leo Buerger, *op. cit.*, p. 277. See, however, E. D. Telford and J. S. B. Stopford, "Thrombo-angiitis Obliterans," *Brit. Med. Journ.*, 1924, ii, p. 1035.

[3] The history of this patient has been previously recorded by Mr. Geoffrey Evans, *Proc. Roy. Soc. Med.*, 1923, xvi (Clin. Sect.), p. 12.

[4] Cf. Leo Buerger, *op. cit.*, p. 320. He writes that during the stage of organization or healing there is a development of fibrotic tissue in the adventitia, which binds together the artery, vein and nerves.

A Case of Arterio-venous Aneurysm of the Neck, with Homolateral Paralysis of the Cervical Sympathetic Nerve and Contralateral Hemiplegia with Aphasia.

By C. WORSTER-DROUGHT, M.D.

H. J. M., AGED 32. In April, 1916, while serving in France, he received a bullet wound in the left side of the neck. While in hospital a few days later, he suddenly lost consciousness, and on coming round was completely paralysed in the right arm and leg and was unable to speak. During the next two years the paralysed limbs gradually regained some degree of power until he was able to use the arm and to walk fairly well. His speech, however, has shown very little improvement. Also "fits" have occurred at irregular intervals, averaging about one a month. The attack is sudden and is not preceded by any aura; he becomes cyanosed, stertorous, and occasionally bites his tongue.

At the present time he complains of pain in the region of the wound, occasional choking sensations in the throat, weakness of the right arm and leg, and inability to speak properly.

Physical examination: He is quite intelligent and understands all that is said to him, but is himself only able to say "yes," "no," give his name and utter a few monosyllables. He is able to read and there is no true agraphia, although the weakness of the right hand limits his writing capacity.

The scar of the bullet entry is seen 1½ in. below the tip of the left mastoid process immediately in front of the anterior border of the sterno-mastoid. No exit scar can be discovered and X-ray examination fails to reveal any bullet in the head, neck or thorax. There is some fullness with visible pulsation of the anterior triangle of the neck on the left side, and over its entire area—from the clavicle below to the pinna above—can be felt a pronounced thrill, with maximum intensity over the line of the carotid artery. Also, a loud and continuous murmur—humming in character—is heard over this area of the neck.

The heart, lungs and abdominal organs do not reveal any abnormality.

Nervous system: The right pupil is larger than left; both are regular in outline and react to light directly and consensually. Dilatation to shade of the left pupil is considerably reduced; it does not dilate to painful stimulation of the skin of the neck (absent cilio-spinal reflex). Also the dilatation produced in the left pupil by the instillation of cocaine solution is very slight and considerably less than that seen in the right pupil. There is some ptosis of the left eyelid and a slight degree of exophthalmos. (The above signs indicate the lesion of the left cervical sympathetic nerve.)

The optic discs and visual fields are normal. There is weakness of voluntary movement in the lower half of the face on the right side (upper neurone type). The tongue is protruded centrally and all other cranial nerves are normal.

There is considerable weakness of the right arm and leg, with spasticity, but no actual paralysis. The right hand-grip is feeble as compared with the left. All arm-jerks (supinator, biceps and triceps) are brisker than the left. The right abdominal and epigastric reflexes are absent; left brisk. The right leg also shows muscular weakness and spasticity; and in walking, the limb is thrown outwards in the manner characteristic of organic hemiplegia. The right knee- and ankle-jerks are greater than those on the left. The right plantar reflex is usually extensor, occasionally indefinite, and the left flexor.

(Formerly the degree of hemiplegia was much more profound, there being scarcely any voluntary movement in the right arm; there was also present a complete right-sided hemi-anæsthesia. The hemiplegia became less and the hemi-anæsthesia

disappeared immediately after a lumbar puncture, from which it follows that there was considerable hysterical disorder superimposed upon the organic disability —a not infrequent phenomenon in organic nervous disease.)

The blood yields a positive Wassermann reaction, but the cerebro-spinal fluid is normal in all respects (normal cell-count, negative globulin, Wassermann and colloidal gold reactions).

Commentary.—The case presents a series of interesting problems. The probable sequence of events is that the bullet entering the neck, damaged during its passage the left carotid artery and jugular vein, thus producing the arterio-venous aneurysm. What, however, has become of the bullet? It must have penetrated deeply in order to injure the cervical sympathetic nerve, as this lesion is much more likely to be due to trauma than to pressure by the arterio-venous aneurysm. The only possible explanation appears to be that the bullet made an exit by way of the patient's mouth. Following the arterial trauma, thrombus formation occurred, and, a few days after the injury, a fragment of clot became detached, producing embolism of the left middle cerebral artery with consequent aphasia and contralateral hemiplegia. The presence of a positive Wassermann reaction raises the question as to how far the syphilitic subject is predisposed to aneurysm by arterial trauma as compared with the normal individual—or, in other words—is the arterio-venous aneurysm the combined effect of arterial syphilis and trauma? With the presence of a definite scar—although neither the exit scar nor bullet can be found—and the development of hemiplegia within a few days of the injury, it can scarcely be maintained that the aneurysm is purely the result of vascular syphilis and that the cerebral lesion is one of specific cerebral thrombosis and not a direct sequel of the aneurysm.

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Section of Comparative Medicine.

President—Mr. Frederick T. G. Hobday, C.M.G., F.R.C.V.S.

President's Address on "The Comparative Section."

By Frederick Hobday, C.M.G., F.R.C.V.S.

In the first place I wish to be allowed to express my appreciation of the great honour you have done me in electing me as the first veterinary surgeon to occupy the Presidential Chair; and I promise that no effort on my part shall be spared to help forward the study of the comparative aspect of disease and to keep together the "entente cordiale" spirit which at present exists between the latest offspring of the Royal Society of Medicine—the Section of Comparative Medicine—and the other branches of its distinguished family.

I do not look upon it so much as a personal honour as a courteous acknowledgment to the veterinary profession; for, through the idea of the Comparative Section, the way has been paved whereby Members and Fellows of the Royal College of Veterinary Surgeons can belong to (and obtain the full privileges of) this great Society, which is one of the largest and most broad-minded in its views of all the medical societies in the world — the access to whose library of over 100,000 volumes alone will save more to a reading man in one month than the cost of his subscription.

I think that the beginning which we have made here in this magnificent and well equipped building, of forming a Section in which those who make their life's study the diseases of the animal kingdom can meet and discuss disease problems with those whose lives are equally busily occupied with human disease problems—and vice versa —will have such far-reaching effects that in the future we shall wonder how we existed so long apart.

That this idea of an "entente cordiale" between the two branches of medicine has "caught on" is already proved by the successful meetings which we have held during the past session—and by the meetings which have already begun to be held in other parts of the country—dealing with analogies between the diseases of animals and those of man.

So far as I am aware the notion of the formation of a Comparative Section of the Royal Society of Medicine, which would take within its embraces all who desire to study the relationship existing between the diseases of animals and man, really originated in 1921, when I approached the courteous and indefatigable Secretary of the Society, Sir John MacAlister, with the support of those two great friends of the

veterinary profession and former lecturers in the Royal Veterinary College, Sir John Bland-Sutton (who was then President of the Society) and Sir D'Arcy Power. I was not then aware that Sir Clifford Allbutt had had a somewhat similar scheme in hand some years before, but as soon as I knew I approached him, and he at once accorded it his unstinted support, as also did the late President of the Society, Sir William Hale-White.

I personally wrote to some 300 members of the medical and veterinary professions, and the replies I received were most enthusiastic and encouraging in support of the proposal. A preliminary meeting was held, with the ultimate result that a scheme, putting forward the proposal for the formation of such a Section, was adopted at a General Meeting of Fellows on June 19, 1923, the first inaugural meeting being held on July 6. At the present moment we have a total of 191 members, of whom 127 practise the human branch of medicine, and 64 are veterinarians.

In order to try to find a suitable name for the Section a *plébiscite* was taken of all those who had displayed interest in its formation and welfare. The votes received in response wavered between the adoption of "Comparative Medicine" and "Comparative Pathology," whilst some correspondents favoured a combination of the two names under the compound title of "Comparative Medicine and Pathology."

Eventually the title of "Comparative Medicine" was selected, and personally I was glad, for this heading gives a latitude which covers everything pertaining to the diseases of the human and animal worlds, whilst it also embraces those of fish, insects and plants. The word "veterinary" had been suggested, but this term was carefully avoided, as it would have savoured too much of the "water-tight compartment," and for anyone who wishes to discuss the medicine and pathology of animal diseases *per se*, there is ample field in the twenty-two branches of the National Veterinary Medical Association, whose ramifications extend throughout Great Britain, Ireland and the Colonies.

We have made a good beginning, and the past session has been quite a successful one. In addition to the stirring Presidential Address of the Rt. Hon. Sir Clifford Allbutt, we have had papers from the following gentlemen:—

Mr. Hobday: Cryptorchidism in animals and man.

Dr. S. G. Billington: Some unusual methods of culturing the blood, with special reference to distemper.

Professor E. Mellanby: Diseases of nutrition in animals.

Dr. T. W. M. Cameron: The pig and human diseases.

Dr. N. S. Lucas: A comparison of the pathology of captive animals with that of man.

Dr. F. E. Crew: Achondroplasia in man and animals with special reference to the "bulldog" calf.

All have been enthusiastically discussed; while for the forthcoming session the offers already made show that we shall have no lack of essayists and interesting subjects.

By the rules of the Section the President must be alternately a member of the medical profession on one occasion and a member of the veterinary profession on the next, and the President can hold office for two consecutive years. Half the Vice-Presidents hold medical, and the other half veterinary, qualifications; and the Council is built up in the same way: whilst the Secretaries are chosen similarly. You will thus see that the intention is to give the utmost facilities for a meeting on common ground of students of the two branches of medicine, the human and the veterinary. The ground to be covered is enormous and it is hoped that the benefit will be mutual, for there are numerous diseases intercommunicable between animals and man, many which are analogous, and many which show totally different methods of manifestation; the contrasts of the latter being not only between man and animals but between the different varieties of animals themselves.

As illustration of the intercommunicable diseases in this country alone, one has only to mention amongst the common maladies: Glanders, tuberculosis, anthrax, rabies, mange, ringworm and trichinosis; while amongst important matter which can be studied on analogous lines we have, as illustrations, such diseases as influenza, pneumonia, tetanus, internal parasitic infections, diabetes, cystitis, pyorrhœa, dental caries, very many surgical operations, the formation of cancerous and other tumours, and a host of other subjects too numerous to mention. Abroad we have the study of trypanosomiasis, piroplasmosis, and other diseases of which we shall hear from Mr. Montgomery to-night. As an instance of one pathogenic micro-organism showing a startling difference in its power of infecting some species and failing to infect others, we have the great and fatal virulence of the ravages of the *Bacillus mallei* for man and the horse tribe, and the immunity possessed by the ox. *Per contra*, there is the extreme sensitiveness of the ruminant and pig tribes to foot and mouth disease and the comparative insensitiveness to it of the horse and ass. The comparative immunity of the dog to anthrax, too, as compared with the extreme fatality of this disease when attacking other animals, is another instance which requires explanation.

That there must be reasons for all these things is very obvious, for Nature never does anything without a reason, and it is by a more complete knowledge of the comparative aspects, such as we hope to obtain by a combined and mutual study, that we shall make further and substantial progress in the fight against disease.

It now remains for us, the Members of the Section of Comparative Medicine, to prove our value, and to show that the trust put in us by the Council of the Royal Society of Medicine, by adding our name to their already specialized and lengthy list, has not been misplaced. This we can certainly do if only we will take up our responsibility individually and seriously, by contributing original work to the papers and discussions both of our own and other Sections, and thus "do our bit" to prove that the two branches of medicine, human and veterinary, can mutually assist one another. These two branches have the same original stock and origin and must absolutely pull together; and not be selfish or work in separate water-tight compartments.

There is comparative work to be done in every Section, and in each much can be mutually learned by comparison between the method of working which leads to the desired successful result respectively in the man or the animal, and between the symptoms caused by the same disease in each variety of patient. Flesh is flesh, whether of the high or the low animal, and disease is a common enemy to be attacked by all of us in our own special way according to the opportunities which experience presents to us individually and collectively. The foundation of our present position in the administration of anæsthetics, the splendid safety of our present day surgery, the study of therapeutics and pathology, even medicine itself, all had the principal part of their pioneer work done upon animal patients before being applied to humans.

Epidemiology, tropical and war diseases, the study of clinical cases and of dermatology; of neurology, laryngology, ophthalmology, odontology and otology; of obstetrics, urology and in fact every sub-division or Section of the Royal Society of Medicine—not even omitting psychiatry—can learn something from the comparative side—and who is so competent to put this forward as the trained and observant veterinarian? My experience has always been that one's views are always listened to with the utmost courtesy, and the presence of a Member of another Section is always welcomed. I mention this to encourage members of my own profession to go and attend the meetings of other Sections whenever there is anything to interest them, and because I know that in the past they have been unnecessarily diffident about so doing.

With the request that every Member of the Section will do his utmost to add something to the discussions at the meetings during the forthcoming Session, I bring my address to a close—for, as already mentioned, we are fortunate to-night in having with us a very distinguished essayist (Mr. Eustace Montgomery, the chief veterinary adviser to the Governments of East Africa), who will tell us of the diseases in that part of the world which need a closer union of medical and veterinary science in order to bring about their prevention and extinction.

Section of Comparative Medicine.

President—Mr. Frederick T. G. Hobday, C.M.G., F.R.C.V.S.

On Some Diseases Intercommunicable between Man and Animals in East Africa.

By Eustace Montgomery, M.R.C.V.S.

(Veterinary Adviser for Eastern Africa).

It is not only "disease" clearly and definitely communicable from animals to man, and from man to animals, that should be considered to represent comparative medicine; there are many other important conditions in which medical men and veterinarians are as intimately concerned as they are in pathogenic bacteria and protozoa. The few examples now to be outlined do not exhaust the comparative subjects available from Eastern Africa, nor can the treatment of each be more than a summary of some of the outstanding features.

Rabies.

Classical hydrophobia, as known in man and animals of Europe, is not recognized in Eastern Africa, though it occurs in the Sudan to the North and in Northern Rhodesia on the South. The disease of Eastern Africa, though fatal to animals, is apparently harmless to man.

Rabies depends for its diagnosis upon one or more of the following methods: clinical, histological, biological, the two latter being the more critical and dependable.

The presence of bodies or inclusions, known after their finder, Negri, within certain nerve cells is practically diagnostic. Similar inclusions have been described as very occasionally occurring in other forms of nervous disease both in man and animals; and their specific character as the causal agent of rabies has been called in question mainly on account of the ability of the rabies virus to pass through a filter candle, and of the apparent absence of these bodies from animals dead of the virus fixed by repeated passage. The presence, however, of unmistakeable Negri bodies in the hippocampi of over 95 cent. of dogs dead of the naturally acquired disease, as well as in many cases in man, and their practical absence from other diseases, leaves little doubt that whatever be their true nature, they possess a real diagnostic significance.

The first observation in East Africa was made in 1912 on a dog which had been bitten a few weeks previously by an unusually bold jackal; the dog manifested many symptoms of dumb rabies. Bodies resembling those of Negri were present; dogs and sheep, inoculated with brain emulsion obtained at autopsy, developed nervous symptoms and died within three weeks. From these animals a strain of virus was obtained which was perpetuated for forty-three generations by subdural inoculation of rabbits and guinea-pigs, a virus of seven to eight days being obtained after the twelfth passage. The disease was rabies, from every usual standard of diagnosis.

During the period of these experiments, neighbours, European and native, around Nairobi, obtained tangible evidence of a disease in their dogs and in wild jackals which was new to them, and which presented symptoms of rabies. Seventeen cadavers, including those of three jackals, were sent to the veterinary laboratory, and from nine dogs and two jackals histological and biological diagnoses were positive. The natives asserted that this disease was new to the country, and considered it due to the arrival of a jackal more intrepid, sometimes foolish, sometimes savage, which from its habit they designated by a new name.

Men were bitten: I have been unable to trace any injury to a European, and my personal experience is limited to two adult male negroes, both of whom were mauled, one on the leg, the other on the hand and forearm, by a dog proved to be infected, but rumour was current of others also being bitten without injury. Although there appears no reference to this in the annual reports of the Medical Department, that of the Chief Veterinary Officer for 1913-14 states: "Of the number of natives bitten by a rabid jackal or dog, and kept under medical supervision for considerable periods, not one died from this disease."

Since 1914 the disease would seem to have disappeared from notoriety in Kenya, but occasional reference is made in the reports of the Uganda Veterinary Department to a condition sometimes occurring there in dogs, the symptoms of which simulate those of rabies.

In Nyasaland there exists in dogs and cats a fatal disease, from one case of which the biological test incidental to true rabies was positive. There is no suspicion of any case occurring in man, though natives are known to have been bitten.

Hornby, from the adjacent territory of Northern Rhodesia—where, however, Europeans have contracted fatal rabies—reports a case in which two bitten natives certainly did not develop any infection.

A disease of carnivora, similar to that noted in Eastern Africa, has been observed in West Africa. No mention of it is made in the reports from British Colonies, but in adjacent French Guinea, Senegal and the French Sudan, numerous writers have furnished clinical, histological and biological evidence that the disease is rabies, but that the indigenous native is insusceptible.

There does occur in French West Africa a form of rabies to which man succumbs. Such cases are rare, but their existence necessitates a different vision in regard to the treatment of patients bitten by rabid dogs, the type of which cannot be differentiated except in man.

Speculation as to the identity or relationship of these two strains will avail little; French writers assume the individuality of each, and blame a more recent introduction by way of Morocco of the ordinary virulent form to explain the occurrence in man. With the problems of natural immunity and environmental resistance so much before them, veterinarians would be tempted to suggest that the susceptibility of the indigenous and the exogenous population be explored.

Obviously it were better for precision to replace suggestion. The immunity towards pseudo-rabies of dogs immunized to the European form, and the effect of pseudo-virus in the treatment of rabies in animals, are lines of inquiry which would clarify doubt.

Cysticercosis and Tæniasis.

Reports of the Principal Medical Officers of the several East African territories show that tæniasis is not uncommon. Until very recently *Tænia solium* was credited with being the dominant form; in the four years 1917 to 1920, of 769 cases treated in Kenya, no less than 663 are recorded as due to this parasite and only 106 to *Tænia saginata*. This is peculiar, since pork is not commonly consumed by Europeans and practically never by natives in whom by far the greatest number of cases is recorded. Again, while cysticercosis of cattle is known to occur in some localities,

neither the veterinary nor the medical organizations have yet found cysticerci in the flesh of East African pigs.

Subsequent to 1920 this statistical preponderance of *Tænia solium* over *Tænia saginata* has been reversed, but there are still recorded a larger number of the former than would be expected if the pig were the sole intermediary. It is probable that the diagnosis is in error; on the other hand, it may be that *Tænia solium* can pass its cysticercal stage in beef or in goat-flesh.

Opportunity for the infestation of cattle is greater among the animals of agricultural people with fixed villages than in those of the more nomadic pastoralist. Near these villages some rank vegetation is left in which all the inhabitants customarily defæcate. Such an area becomes grossly contaminated and, being conveniently situated, the cattle often lazily graze in it, the risk of infection being increased by the corn-eating habits of some natives and the grain-containing character of some of their fæces.

Meat inspection in Eastern Africa is erratic; in some slaughtering centres a veterinarian inspects when he is in the station, but the greater part of his time is spent on tour, and in one or two places this duty devolves on the M.O.H.

The detection of cysticerci in beef is not always easy. It is rare for a gross somatic and generalized infestation to be met with; and not always are the scattered bladder-worms to be found in the pterygoids, the masseter, heart, diaphragm or tongue. There do not appear to be any situations clearly more favourable than others, and the danger of not finding isolated parasites is very considerable, for it is obviously contra-indicated practically to mince the carcase. Serological diagnosis is equally impracticable as yet.

Meat exposed in markets which are liable to inspection is bought in small quantities by town dwellers, both European and native, and by them it is customarily well cooked before being eaten. The danger of human infection here is much less than it is among natives who in their own surroundings gorge upon half-cooked or raw meat from an animal slaughtered to commemorate some event, or dying of disease.

As regards control, I have suggested sufficient to indicate how speculative may be meat inspection alone. This certainly detects the most glaring examples, and as competent inspectors become more available a still higher efficiency may be attained; but it is village hygiene that in the long run is more likely to achieve success.

Although the native is opposed to reform I do not consider that an administration working where available, in conjunction with a medical and veterinary organization, would experience any insuperable difficulty in initiating a simple system of trench latrines in each village; or in making effective a prohibition that animals should graze within a defined area to be alienated for sanitary purposes.

Anthrax.

In Eastern Africa it is still largely a question of chance that enables a positive diagnosis of anthrax in cattle to be made. Its existence is known: some pastoral tribes recognize it as capable of infecting man, but the time has not yet arrived when even the worst of the enzoötic areas can be outlined.

To many natives the sickness or death of a bovine presents a heaven-sent opportunity for a feast on beef, and rarely is such a chance missed. A recent instance in Uganda is illuminating. During the movement of thirty-eight cattle for trade purposes, one animal died and was eaten, with the result that seven natives died. A few days later, and but a short distance on the road, six more deaths occurred among the cattle. There was another feast, and this time ten natives succumbed to anthrax. Only then did the occurrence come under notice, and as the result of action taken, further mortality in both man and stock was averted.

Lack of information may be the reason why in Africa there seems to be so large a proportion of cases of cutaneous anthrax in man, fatality from which is rare.

Taylor has recorded several cases in Uganda, six of which were pustular, and the patients recovered. In the Kenya records between 1919 and 1922, 135 cases were diagnosed in man, with only six deaths. The problem is to learn how many fatal and unreported cases of septicæmic anthrax occur in association with the mild and detected pustular cases.

Anthrax does not manifest itself as an epizoötic like rinderpest or plague; relatively it is responsible for a slight mortality only, and this tends to blind those in authority to the danger to themselves and to those who in the course of trade and manufacture handle such products as hides and wool of animals infected with this disease.

It is difficult to see how any serious progress can be made until a much more intimate contact is possible between veterinary and medical departments on the one hand, and animals and their owners on the other. This entails not only an adequate skilled personnel, but also an extensive semi-trained inspectorate, to obtain information of occurrences within the villages and native areas. One has only to consider the difficulties in England, where every farmer and farm-labourer is a factor in the official intelligence system, and every policeman is an inspector, and where medical men and veterinarians are within comparatively easy reach, to realize how far behindhand are the facilities in our tropical possessions.

Until then, and until the application of suitable measures, bulk sterilization of suspected produce is the best palliative measure now available for the protection of industrial workers: but its limitations are apparent.

Malta Fever.

The observation by Alice Evans, in 1918, that the *Micrococcus melitensis* and the bacillus of bovine contagious abortion are each capable of reaction by sera containing the agglutinins of the other, and are indeed bacteriologically indistinguishable, has opened up a wide field for speculation.

Malta fever in Europeans appears to be rare in Eastern Africa, but it was in the natives and goats of a district in Uganda that Bruce, in 1910, identified its existence.

It certainly does not seem, as for example in England, that the presence of contagious abortion bears any relationship to that of Malta fever: and in other countries there are but few data that incriminate the bacillus of Bang as a factor in human pathology. *Micrococcus melitensis*, however, in one direct experiment has been blamed for producing abortion in a cow; and it is stated that when Malta fever first attacks a herd of goats, abortion is a common symptom.

It is rather in prophylaxis that greater difficulties are now presented, since an agglutinating serum is not necessarily obtained from a carrier of Malta fever.

Trypanosomiasis.

Domestic animals have not been seriously considered as reservoirs of a trypanosome pathogenic to man; but as critical inquiry becomes more possible their influence in this disease grows more apparent.

Let us, in the first place, consider the influence of stock upon the distribution of the tsetse flies mainly responsible for the transmission of *Trypanosoma rhodesiense* to man. There are also other "bush" species, requiring vegetation and shade for their normal existence; they do not naturally occur in open pastures. Large areas of Africa that are now under "fly" were even thirty years ago free from the pest, and were tenanted by a healthy human and cattle population, who, by cultivation and grazing, controlled the growth of bush, and by hunting kept away game with the attendant risk of disease. In 1893 began the first great epizoötic of rinderpest in Central Africa: the cattle died and the grass and the bush grew up: game and fly crept in and the scientifically weak and unorganized man receded, leaving an

unproductive and a potential *rhodesiense* area where but a short time before cattle kept away the menace.

It is clear that *Trypanosoma brucei* and *Trypanosoma pecaudi*, forms with which many of these "fly" areas are infected, are not usually pathogenic to man, for he survives when equines are quickly destroyed—and Taute even failed to infect man by inoculation—but morphologically they very closely resemble, respectively, *Trypanosoma gambiense* and *Trypanosoma rhodesiense*, to which man is susceptible. Duke has propounded an alluring hypothesis of mutation to explain the recent occurrence of the human disease in what was until lately a country healthy for man.

All four of the trypanosomes named are capable of living for long periods in cattle without producing symptoms in them, and the prospect of an infection being so introduced must be faced, however rare such an origin may have been in the past.

Perhaps no subject affords so good an example of comparative medicine as does trypanosomiasis, for whether approached mainly from a human or from an animal standpoint, or primarily as a protozoological or an entomological study, the unity of these several branches is obvious.

Bovine Tuberculosis.

This is so rare in Eastern Africa as to be practically non-existent. Importations of stock which take place from Great Britain, South Africa and Australia, are most carefully supervised in order that this freedom, unique in stock-producing territories, may be maintained.

Glanders.

The infection was introduced with equines during the stress of war, but the disease has been eradicated, and no case has occurred during the past eight years.

Foot and Mouth Disease.

This occasionally occurs in animals, but fortunately in a very mild form, and I cannot recollect any occasion on which man contracted infection.

Rinderpest and Small-pox.

Attention was drawn some years ago, by Sir William Simpson, to the affinity between rinderpest and small-pox in India. The similarity of lesions on the skin of recovering and mild cases of rinderpest to those of man with small-pox is noted by the natives of Africa, who in many dialects designate both by a common name. Certain pastoral tribes in particular, hold to a view that both occur in cycles of seven years, the one following the other, and either may initiate the cycle. We have not accumulated any data of precision to support this belief, which may have been originated by Indian immigrants, for it is doubtful if the primitive African could have himself evolved it with a limited acquaintance of rinderpest of less than forty years since its introduction to the continent; indeed epidemiological evidence tends to negative any intimate affinity.

DISCUSSION.

Sir William Leishman said that among other points in Mr. Montgomery's paper he had been greatly interested in what had been said in connexion with the apparent diminution of virulence in the case of three diseases, differing so widely in their nature as anthrax, rabies and foot and mouth disease, as contrasted with the virulence they usually exhibited in other countries. He wondered if any climatic or other conditions peculiar to East Africa had any bearing upon this, and considered that further observations and research on this subject were much to be desired.

Major Rabagliati said that the diseases communicable between animal and man in East Africa appeared to be very similar to those met with in Egypt. He said that rabies was a most important disease, and was very prevalent in Egypt, where any number of persons varying from 50 to 100 were treated daily by the Pasteur method. The form of the disease

met with appeared to be typical, and the symptoms seen were more or less of the classical variety. He had never heard of any cases occurring in Egypt of the peculiar form of rabies described by Mr. Montgomery. He stated two instances in which the veterinary profession had helped the medical in the elucidation of outbreaks of anthrax: this showed the great benefit of collaboration between the two professions.

He also mentioned how camel-mange and camel-pox had been transmitted to attendants during the war, and he said that diseases such as glanders, tuberculosis, Malta fever, foot and mouth disease, cysticercosis, trichinosis and trypanosomiasis were further examples of intercommunicable conditions. He stated that tuberculosis was a very serious disease amongst the natives, many cases being of the bovine variety, but he said he had never heard of a case of foot and mouth disease or of trypanosomiasis being transmitted to a human being in Egypt. He said that Malta fever had been fairly common in goats some years ago, especially in Port Said, where there were many Maltese goats. Cases were also met with in man in the country from time to time. No case of contagious bovine abortion had been diagnosed in Egypt, though he admitted it had never been carefully looked for.

Mr. HENRY GRAY said it would be interesting to know if that rabiform disease, not transmissible to man, which occurred in Kenya Colony, could be distinguished from contagious pseudo-rabies or infectious bulbar paralysis, described by various Hungarian, Austrian and Roumanian authorities as occurring in Hungary, Austria, Roumania and adjoining countries, and as not being pathogenic to man. It was generally thought to be due to rats.

Dr. T. W. M. CAMERON said that *Tænia solium* had become very uncommon in this country—so uncommon that great difficulty was experienced in obtaining specimens. *Tænia saginata* was, however, by no means rare, and many hastily diagnosed specimens of *Tænia solium* had been found to be *Tænia saginata*. In connexion with the apparent immunity of man in East Africa to foot and mouth disease, he pointed out that Continental workers had suggested that two distinct strains of the virus existed and that possibly only one of these was infective to human beings, although the symptoms caused by both strains in cattle were indistinguishable. This might furnish an explanation of the East African situation.

Section of Comparative Medicine.

President—Mr. FREDERICK T. G. HOBDAY, C.M.G., F.R.C.V.S.

Puerperal Sepsis in Cattle.

By LESLIE PUGH, B.Sc., F.R.C.V.S.

THE subject of puerperal sepsis in cattle is so wide that no more than a small section can conveniently be dealt with in a short paper. It seems better to exclude altogether the disease known as contagious abortion and associated with the *Bacillus abortus* of Bang, although this may appear in the guise of a puerperal fever following parturition at full time. There is, however, another syndrome in which many of the characteristic features of human puerperal fever seem to be reproduced, and which is of very great interest to the comparative pathologist. It is with this syndrome that this paper deals, and it is proposed to set out briefly the salient points in the clinical picture and then to direct attention to a few of the points on which comparative pathology may perhaps assist both obstetricians and veterinarians to a better understanding of puerperal fevers.

This syndrome has not yet acquired a distinctive name, nor has it attracted much separate notice in veterinary literature. The discovery of Bang's bacillus gave a new and very distinct orientation to veterinary work, and there was a great tendency to include under the title "contagious abortion" syndromes which have little in common with the results of infection with the *Bacillus abortus*. We are only now beginning to separate out those clinical entities which were thus included. In due course they may be expected to arrive at the dignity of distinctive nomenclature.

Of the particular syndrome with which we are dealing the first sign is a delay in parturition. This is not always observed, and, in any case, it is soon followed by a very important symptom, namely, retention of the placenta after the birth of a full-time, apparently normal calf. As it is the habit of cattle to devour the placenta, this retention may be overlooked, especially if parturition has taken place at night and unobserved; and this is particularly easy, since the cow shows no signs of ill-health at this stage. Within a period of a few hours to a day or two the tip of the placenta may be seen, or a reddish-brown vaginal discharge appears, and the onset of the discharge is often sufficient cause for summoning expert advice. If this discharge also remains unobserved, very serious developments take place in a number of cases. With the same acute onset as is seen in human puerperal fever, there are found all the signs of profound toxæmia. There is a foul uterine discharge, and death may follow in spite of treatment.

The intervention of the veterinary surgeon may take place at one of three different points, and it is interesting to note the physical and bacteriological findings at each stage. If he be called within an hour or two of labour, as may be the case on farms in which the disease is endemic and carefully looked for, he will separate the placenta manually. The placenta is composed of a large number of separate cotyledons, each interdigitating with maternal cotyledons. He will find that many of these separate easily, but that a certain number are closely held together by swelling of the maternal part. This is so much the case that they may be torn apart during removal.

Having removed the placenta (which often means at least an hour's work), the surgeon will thoroughly wash out the uterus, syphon out the remnant of the washings which lodge in the dependent horns, and insert an antiseptic pessary in each horn. The course of the case will then, generally speaking, be one of uneventful recovery. If the placenta be examined at this early stage it will be found that there is a pure

infection with an organism of the coli group. In a number of cases that have been followed out by cultural methods, *Bacillus coli communis* has been obtained in pure culture.

Should the veterinary surgeon be called when the placenta has already been retained twenty-four hours or more, his procedure will be that already indicated, but the bacteriological findings will differ. The coli-infection is still present, but is often overgrown by crowds of saprophytes. Unless the case is shortly to take a turn for the worse, there are not many streptococci.

If acute constitutional symptoms have set in before skilled attention is called for, the treatment may be the same but it will have relatively little likelihood of success. Streptococci are now prominent and may dominate the field. The various findings reported by bacteriologists in human puerperal fever may be almost exactly paralleled from cases of this third stage in cattle.

Such, briefly, is the course and character of this particular syndrome in cattle: and it has only to be added that the disease persists for long periods on individual farms, and that streptococcal puerperal fever seldom follows in cases in which the placenta is not retained, or, if retained, is infected with Bang's bacillus. The primary coli-infection seems essential to the development of the much more serious streptococcal disorder.

The mere recital of the essentials of this syndrome seems to raise certain questions of its relation to the human form of puerperal fever, and this brings us to the field of activity which this Section was created to explore. Of the many possible questions which arise I should like to direct particular attention to two.

There is, first, the question of a primary coli-infection. Does this by any chance occur in human cases? Has it, perhaps, been overlooked owing to the inherent difficulty of early bacteriological examination? It is for obstetricians to advance any decided opinion that may be justified, but all are aware that there is at least one form in which coli-infection is peculiarly associated with pregnancy, namely, coli-bacilluria. Nor are there wanting records of coli-infection of the uterus, and the pathology of retained placenta in the human being is far from being fully elucidated.

Secondly, I would direct attention to the possible sources of streptococcal infection. In the case of cattle there can be no question of certain sources to which much blame is attached in current obstetrical literature, the dirty hands or instruments of midwife or doctor, self-examination, coitus during pregnancy, &c. In cattle, labour is unattended, and operative interference or examination is very rare and reserved for difficult cases. The coli-infection is apparently present even before labour commences, and is presumably carried by the blood or lymph: we have to explain the addition of saprophytes and streptococci. It is difficult to suppose that they arrive otherwise than *per vaginam*. What is the mechanism which conducts these organisms into the uterus? Experiments and experience seem to show that, during the irregular contractions of the uterus which are particularly associated with attempts to expel the placenta, a definite suction action is in force. Where contraction of the uterus has protruded a long fragment of membrane, it has been observed that this fragment may be withdrawn as much as 4 in. back into the uterus. Doubtless it would be contaminated with vaginal organisms. Is this the route and mechanism of infection? If so, how far is it true also of the human being? Is it not possible that the alternate contractions and relaxation of the puerperal uterus exert a similar suction action which results in contaminated mucus being drawn in from the vagina.

The veterinarian assumes that the cow is not very susceptible to septic infections and he does not scruple to invade the cavity of the uterus with his hands, with

douches, medicated paraffin and pessaries, and the results of cases in which streptococci have not yet obtained a hold are excellent. It is not presumption but pure interest that makes one ask if some antiseptic agents suspended in liquid paraffin might not have a similar beneficial effect if introduced into the uterus very soon after the birth of a child.

The sole object of this short paper has been to bring before you certain factors that may help to view the baffling subject of puerperal sepsis in better perspective, and, if it succeeds in so doing, it will be yet one more proof of the value of the study of comparative medicine.

Discussion.—Professor LOUISE MCILROY said that the subject of puerperal sepsis was so vast that in a discussion it was impossible to do more than touch upon one or two points. It was of the greatest value to the obstetrician to study the parallel manifestations in cattle that had been described by Mr. Pugh, and to learn how much they resembled those of human patients. In the case of the human species normal labour in its true meaning was seldom allowed to take place. Under supervision there was always some manipulative interference, more especially in the management of the third stage with the artificial stimulation of the uterus by manual massage. It was very interesting to learn that coli-infection of the placenta was a cause of delay in its expulsion. In the human subject, on the other hand, retention was said to lead to sepsis, but this condition had not been satisfactorily cleared up. She (Professor McIlroy) had been very much impressed with Mr. Pugh's description of infective processes in cattle and in her experience they ran on very close parallel lines in the human patient. She would refer to the coli-infection later on. Putrefactive organisms found a culture medium in the débris and blood-clot in the puerperal uterus, but the normal contractions adequately dealt with this condition. If the uterus was fatigued from prolonged labour, post-partum hæmorrhage, or toxæmia, it could not efficiently expel its contents and it became infected with putrefactive organisms. Drainage in these cases was always defective. The uterus might later become infected with streptococci and then general sepsis with blood infection was the result. In the early stages treatment by postural or local drainage and by medicated intra-uterine pessaries of iodine was of value. Lactic acid pessaries in the vagina were of use. The uterus was induced to contract by the administration of quinine, ergot, pituitrin, &c. The suction action of the uterus in cases of retained placenta was of interest. She felt that the manual manipulation of the uterus during the third stage of labour was undesirable, as it might bring about this very suction action which seemed to lead to infection in cattle. The ideal method of treatment of the uterus in a normal placental stage was to refrain from touching it until expulsion of the placenta was imminent. When membranes had been retained it was her practice not to explore the uterine cavity and the results were no worse than when energetic measures were employed. If the uterus contracted the membranes were expelled spontaneously during the puerperium. If a tag of membrane partially protruded from the cervix it was much more a source of danger from infection than if entirely within the uterine cavity.

The study of infective organisms and their treatment had been well carried out, but the source and mode of infection were still open to research. As the result of experience she was becoming more and more convinced that in the majority of cases of sepsis the organisms were not introduced by the attendant, except in those obvious cases in which there had been direct contact with septic patients or their surroundings. Cases of puerperal sepsis occurred in the practice of expert obstetricians and in the best maternity hospitals. They had all had their experience of them and until it was realized that blame should not in every case be attached to the medical attendant there would be no progress in elucidating the prevalence of the disease. Was there any proof in support of that statement?

(1) Maternal mortality from sepsis had not diminished during the last twenty years in spite of all precautions. Sterile gowns, gloves and masks were worn, and every method possible had been devised to render the process of labour and delivery as aseptic as possible. If sepsis were due to direct introduction of organisms these measures would, before now, have relegated puerperal sepsis to the list of obsolete diseases.

(2) Local manipulations could not be held to be responsible in every case. Some of the worst cases of sepsis occurred in patients who had had a precipitate or unattended labour. There was no manipulative interference in cattle. Rectal examinations alone had been carried out in the Obstetrical Unit for eighteen months and the results had been less satisfactory than in the case of vaginal examinations. But she (Professor McIlroy) considered

that for many reasons it was ideal to have no examination of the vagina. A large number of cases in which operations had been performed showed a perfectly normal puerperium.

In her opinion the main predisposing causes of sepsis were: (1) Lowered immunity; (2) lacerations of the genital tract; (3) latent infection.

(1) Lowered immunity or resistance was a factor in puerperal as in all other forms of sepsis. If it was induced by ante-partum or post-partum hæmorrhage as the result of toxæmia, sepsis was more likely to occur. Fatigue of the uterus took place in toxæmia, which frequently originated in intestinal stasis and coli-intoxication.

The presence of the coli-bacilli in pregnancy was demonstrated in cases of bacilluria, pyelitis and appendicitis, although in their experience cases of coli-infection in pregnancy did not always result in puerperal sepsis. This might be due to immunity acquired by ante-natal treatment. Prophylactic vaccines were of great benefit in these cases when given during pregnancy. Apart from pregnancy, patients who had a low resistance were susceptible to even slight infection, for example the cases of fatal sepsis from the stings of insects.

(2) Lacerations of the genital tract were by far the greatest source of sepsis. She (Professor McIlroy) could not believe however that the placental site was in the nature of an open wound. Surely there was in most cases a natural defence against the invasion of infection. Nature left little to chance, but no doubt if under misguided treatment the protective plugging of the mouths of the vessels, or the barrier of round-cell tissue in cases of putrefactive infection were scraped away, the placental site would be turned into an open wound.

Lacerations of the vagina and cervix were the most frequent source of infection. Intact epithelium was a safeguard against the entrance of organisms. Lacerations of the cervix led to infection of the pelvic cellular tissue and localized sepsis. In looking over the records of cases of pyrexia in the puerperium she (the speaker) almost invariably found that lacerations had taken place. The prevention of lacerations was essential and the prompt stitching of even small tears was necessary. Perineal lacerations were so obvious that as a rule they were ably dealt with.

Operations, however necessary, always increased the risk of sepsis because of the possibility of lacerations. If hæmorrhage occurred, the resistance of the patient was lowered at the same time. Coli-infection was found in some of the cases.

(3) Latent infection might occur by way of the upper route or blood-stream. Previous puerperal sepsis was found to repeat itself in a second labour, showing a latent infection of the uterus. A septic finger during pregnancy had caused death from sepsis in the puerperium.

Prophylactic vaccines or sera could be used, and this treatment of acute streptococcal infection, involving the blood, by antistreptococcal serum and by quinine injections was giving excellent results. No local treatment was considered necessary in these cases. She (Professor McIlroy) sometimes wished she had the courage in a teaching hospital to allow normal labour to take its course without any sterilization of the vulva or vagina, for she felt such measures were misdirected, if not useless. The natural protector of the vagina—the *Bacillus vaginæ*—shared the fate of its pathological brethren in being swept away by antiseptic applications.

For two years a former assistant in the Unit, Dr. Charlotte Houlton, had carried out some very careful researches into the bacteria of the cervix and vagina. She had confirmed the work of Döderlein and others. She had examined ninety-eight cases from the Ante-natal Clinic and had found normal, clear, vaginal secretion in sixty-seven cases. In thirty of these the *Bacillus vaginæ* was present. Upon the puerperium being followed up, only one showed a febrile temperature—1·5 per cent.

Of the remaining thirty-one cases with purulent discharge, the *Bacillus vaginæ* was present in only one case, but streptococci, staphylococci, and diphtheroid bacilli were present in all. In four of these there was a rise of temperature in the puerperium—13 per cent. If Gram-positive cocci were found in the discharge of pregnant women Dr. Houlton had advised the use of a vaccine during the last few weeks of pregnancy. As the *Bacillus vaginæ* generated lactic acid—an important antiseptic—they had found the use of lactic acid pessaries in the case of purulent discharge in pregnancy to be most efficacious.

It would be interesting to know whether lacerations occurred among cattle and whether serum was employed in the case of blood-infection. She (Professor McIlroy) suggested the ante-natal supervision of cattle and their surroundings, for it might give as encouraging results as it was yielding in the case of human individuals.

Dr. REMINGTON HOBBS said that he had seen many cases of puerperal sepsis which had been due to *Bacillus coli* infection. He mentioned that staphylococci and streptococci were

also present in uterine and cervical cultures. A short time ago he had investigated 100 cases of pregnancy, and in 23 per cent. the urine was found to contain *Bacillus coli*. In practically all these cases *Bacillus coli* was also found in the vagina and cervix. After injections of gomenol oil into the bladder had been made, and after intracervical and vaginal treatment with sterilized glycerine, no coliform bacilli were found. After delivery there were no cases of puerperal sepsis.

He quoted a case of a patient who was seven months pregnant, admitted with a high temperature, rigors, tenderness over the right kidney, and urine loaded with *Bacillus coli*. There was a profuse vaginal discharge; the cervical canal and vaginal secretions contained *Bacillus coli*, streptococci and staphylococci. After a month's treatment the urine had become sterile, and the vaginal secretions were free from *Bacillus coli*. Two months later the patient was confined. Forty-eight hours afterwards cultures from the cervix contained streptococci and staphylococci, but no *Bacillus coli*. The urine was sterile. There was no rise of temperature during the puerperium, but on the fourteenth day there was a slight secondary hæmorrhage which cleared up after intra-uterine applications of sterilized glycerine, which in his (Dr. Hobbs') opinion was a powerful stimulant to the uterine muscle, as well as an agent producing osmosis, thereby aiding Nature in draining its tissues without any damage resulting. Therefore, in spite of the presence of *Bacillus coli* and of streptococci in pregnancy the patients were not more liable to a streptococcal infection.

He also quoted another interesting case in which the temperature suddenly rose during labour to 103° F., the pulse to 136, with shivering; visual examination of the cervix showed a teaspoonful of pus actually coming from the lower uterine canal. This pus was loaded with *Bacillus coli*, which was also found in the urine. At that time and immediately after labour, injections of pure glycerine were begun, and the temperature and pulse fell to normal in a few days. For a long time he had believed that infection of the genital tract was caused by *Bacillus coli* or by some other virulent organism. In cases of fever following a confinement, with the presence of *Bacillus coli* in the urine, practitioners should be very careful not to miss a uterine infection. He (Dr. Hobbs) had also seen many cases of this type when the uterus had been pronounced normal, yet the patients afterwards showed marked symptoms and signs of subinvolution.

Mr. ALFRED C. WILD said he was of opinion that one of the causes of retention of the after-birth in the cow was to be found in the manner of connexion of the fœtal and maternal membranes of the placenta. The number of cases of septic infection varied considerably in different classes of farms. Infection was most prevalent on farms where grooming of the cows was not practised and where the cows were allowed to remain plastered with fæces which often harboured *Bacillus coli*. In such instances he considered septic infection would be much more common were it not for the protective action of the vaginal secretions. He had observed that few cases occurred in the model dairy farms, where pregnant cows were kept clean, always provided that they were not in fat or show conditions. His (the speaker's) usual practice was to remove the after-birth on the third day but he did not think much danger would accrue from leaving small pieces behind. Most cases of septic infection were caused by quantities of purulent fluid in the uterus and in such cases he usually douched the uterus with saline or very dilute antiseptics and then rotated his arm in the uterus and thus induced the contraction of this organ. He had also observed that on farms where contagious abortion was present, retention of the after-birth was very common.

Mr. L. LITTLE said that puerperal sepsis was one of the most common conditions met with in veterinary practice, and considering the conditions under which the majority of cows lived preceding and during parturition, and the difficulty experienced in obtaining satisfactory drainage from the uterus, it was extraordinary that such cases were not more frequent. He thought it was frequently associated with retention of the fœtal membranes, this condition being often caused by an infection with the bacillus of contagious abortion. In his opinion the manual removal of the retained membranes was a difficult and tedious operation and it was his practice not to attempt this if force were necessary as it frequently caused the death of the animal. He preferred attempting to induce contraction of the uterus by means of simple douches of normal saline having a temperature slightly above that of the body. He found that usually the membranes could be removed easily on the seventh or the ninth day, if they had not been ejected before, and he advocated the excision of all visible parts of the retained membranes. He deprecated the use of strong antiseptics and he considered the cause of many cases of sterility could be attributed to this method of procedure. He was much

interested in the method of injection of glycerine, and although he thought it was a much too expensive procedure for the larger animals it could very easily be used in the case of the smaller patients.

Dr. J. KINGSTON BARTON referred to his practice of midwifery in the years 1880-1895 in a good-class neighbourhood in South London, mentioning that he had two classes of patients: A poorer class of women, such as the wives of coachmen living in the mews adjacent to their wealthy employers. These women had usually well recovered from their confinements by the ninth day; and would be found washing and cooking for their families after that date. In spite of living in overcrowded rooms (generally permeated by odours from the stables beneath) and of having had no aseptic preparation for the coming puerperium, they never developed any morbidity (i.e., slight febrility) or any sepsis. Their old-fashioned nurses would merely wash them freely with soap and water, never using any douches or antiseptics of any kind. Within twelve hours they would be using the ordinary chamber, and by the second day would be sitting up in bed for feeding, &c., which would encourage good natural drainage from the vagina. In contrast to these patients there was the second class, namely, the wives of their well-to-do employers. These ladies would have a first-class special obstetric nurse, trained by the great obstetricians of that time, who kept the patients flat on their backs, tied up in stiff abdominal binders. All this kind of treatment quite prevented any natural drainage from the uterus and vagina. Strong antiseptic douches were used night and morning, yet a large number of these patients would suffer from a slight morbidity and occasionally from threatened or actual sepsis, and were poor creatures for four or five weeks after delivery. Fortunately, with the coming to London of the great obstetrician, Matthews Duncan, the practice of all these unnatural processes was swept away, and full reliance was placed on the greatest attention to perfect cleanliness, all natural drainage was attended to, and the patient was encouraged to be more active in her movements. In the whole field of surgery success seemed certainly due to the greatest attention to sterility and asepsis, rather than to the use of or reliance on antiseptics. And to reduce puerperal sepsis to a minimum, reliance must be placed on similar methods. The experience of one speaker was that puerperal sepsis was absent in a large model dairy of 200 cows, this being entirely due to simple but thorough ablutions of the hind-quarters of these animals. Another speaker from Australia also described the absence of sepsis in cattle that lived entirely in the open air and sunlight and had the run of large clean open spaces during their daily life. So that at the present day veterinarians concurred with obstetricians in the practice of extreme simplicity, combined with the strictest cleanliness, as the factors for reducing puerperal sepsis to a minimum.

Both Mr. Leslie Pugh and Professor Louise McIlroy had said a good deal on the subject of coli-bacilluria. He (Dr. Kingston Barton) all through his professional life had taken the greatest care to prevent nurses passing catheters, especially during the puerperium, as almost invariably a bacilluria would ensue, no doubt from the introduction of bacteria from the immediate neighbourhood of the meatus. Soap and water and the natural local immuniz ng powers would keep these bacilli at bay, but once introduced into the bladder or into the cervical canal, sepsis would ensue.

Professor McIlroy had expressed a desire to throw aside all the unnatural aids that were so much recommended and practised. He (Dr. Kingston Barton) would advise her to take courage and follow her great predecessor, Dr. Matthews Duncan, and rely entirely on simple methods and the strictest cleanliness.

Mr. THOS. DALLING said he wondered if any information were forthcoming on the question of "scours" in calves bred from cows affected with puerperal fever, pointing out that in all young animals, calves, foals, pigs and lambs, deaths on the day of birth or in the immediately succeeding days from *Bacillus coli* infections and preceded by "scours" was common.

Section of Comparative Medicine.

President—Mr. FREDERICK T. G. HOBDAY, C.M.G., F.R.C.V.S.

DISCUSSION ON CANINE JAUNDICE: WITH SPECIAL REFERENCE TO LEPTOSPIRAL INFECTION.

Dr. C. C. OKELL: I do not propose to discuss in detail the work carried out by Dalling, Pugh and myself on this disease. In the *Veterinary Journal* of January, 1925, we have given a full account of our attempt to ascribe enzootic jaundice in dogs to leptospiral infection. In brief, our hypothesis was developed along the following lines: we compared enzootic jaundice clinically and anatomically with leptospiral infection in man and other animals.

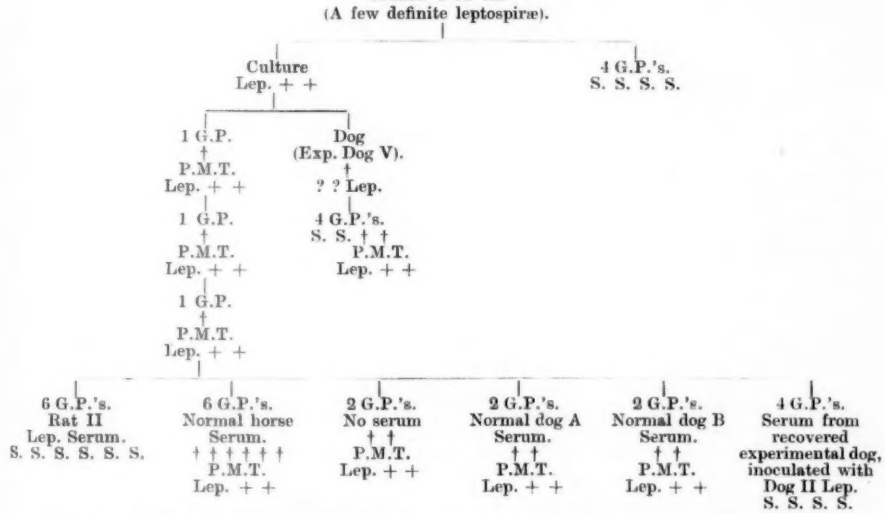
From the *clinical standpoint* we found in common such a characteristic group of symptoms as jaundice, hæmorrhages, albuminuria and a definite type of temperature curve. We found, moreover, such exceptional symptoms as acute muscular tenderness, hæmorrhages, herpes and keratitis, all of which occasionally occur in canine jaundice and are characteristic of leptospiral infections in general. From the standpoint of *morbid anatomy* we met with almost identical lesions in canine jaundice as in leptospiral infection in man and other animals—enteritis, nephritis, parenchymatous degeneration of the hepatic cells, and the hæmorrhagic condition of the lungs and other organs so typical of leptospiral infection.

To make a still closer comparison, we inoculated dogs with leptospiræ from the rat and reproduced a disease which, so far as we were able to see, was indistinguishable from the naturally occurring enzootic jaundice.

We examined in detail the material from ten dogs which had suffered from enzootic jaundice, and from the tissues of three of these we were able to isolate a leptospira and to submit it to further experimental study. In one further case we saw undoubted leptospiræ by dark-ground examination of ground-up liver, but we failed to isolate them.

The accompanying "passage tree" gives an account of the passages and cross-protection experiments with serum performed with one strain; similar experiments were done with the other two strains we isolated from dogs.

PASSAGE TREE OF STRAIN DOG III LEPTOSPIRA.



P.M.T. = Typical post-mortem appearances. † = Died. G.P. = Guinea-pig.
Lep. + + = Many leptospiræ present. S. = Animal survived with no symptoms.

With respect, then, to the three strains that we isolated, we have satisfied almost completely the postulates of ætiology usually demanded before ascribing a disease to a given micro-organism. We were able to show that the virus was living and could be propagated through many generations, that it was capable of producing the typical disease in dogs and other animals, and that pure cultures of the three strains were fully and characteristically pathogenic. All the strains are still being maintained in culture and in guinea-pig passage, and there is no evidence of any deterioration in their virulence. In all respects they seem identical with *Leptospira icterohæmorrhagiæ*.

We were less fortunate in satisfying Koch's important postulate, "that the parasitic micro-organisms are to be found in all cases of the disease." We met with, however, an almost equal difficulty in finding and isolating the leptospira in dogs we infected with massive doses of leptospiræ of canine and rat origin, which produced death with typical post-mortem lesions. A similar difficulty is, of course, met with in isolating leptospiræ from human beings suffering from Weil's disease, though nobody now seriously questions that the disease is due to leptospiral infection.

We had hoped to obtain confirmation of our hypothesis by finding leptospiræ constantly present in the tissues of affected dogs, and we made a full examination of the material by means of Dobell's modification of Levaditi's stain. In one dog only did we find characteristic leptospiræ in a hæmorrhagic lumbar gland. We found equal difficulty in finding leptospiræ in the tissues of dogs experimentally infected with massive doses of leptospiræ.

It is an interesting question, which I hope will receive attention in the subsequent discussion, as to why the leptospiræ, which can be found with comparative ease in infected guinea-pigs, should be so elusive in man and in the dog. A further important line of confirmation is the study of the immune properties of the serum of dogs which have recovered naturally from the disease. The serum of animals which have been experimentally infected with leptospiræ and subsequently recover, usually has marked protective properties. In our own experience untreated enzootic jaundice is so fatal that we have been unable to obtain much material of this kind.

We have obtained a small amount of serum from only one dog which had recovered from the natural disease, and that reached us in a contaminated condition. Two guinea-pigs were injected with 0·25 c.c. of this serum and afterwards with a virulent strain of leptospira, one of dog and one of rat origin. Both died prematurely from peritonitis, but without any trace of jaundice or other symptoms of leptospiral infection, while all the control animals inoculated with the same strain died typically of jaundice on the preceding day. May I take this opportunity of making a request for the serum of recovered dogs? The result of the study of a few such sera would do much to advance or destroy the leptospiral hypothesis of canine jaundice.[1]

It is an interesting question as to why human cases do not more often occur in association with canine enzootics of jaundice if the latter is due to leptospiral infection. Krumbein and Frieling, in 1916, reported an instance in which two officers, living in the same room as a dog suffering from jaundice, developed what appeared to be typical Weil's disease. The investigators were, of course, at that time unable to confirm the nature of the infection.

Uhlenhuth and Fromme (1919), while investigating an area in which cases of human Weil's disease had occurred, met with a case of jaundice in a dog which they proved almost conclusively was leptospiral.

Dr. Passey has also met with a case of human jaundice associated with a case of jaundice in the dog. A further case of associated human and canine jaundice has been brought to our notice by Dr. Stirling and Mr. Lornie, M.R.C.V.S., of Perth. These isolated cases are, however, all we have been able to trace. The problem is, of course, related to the other problem, namely, why is Weil's disease in human beings relatively rare in this country, while 20 per cent. to 30 per cent. of adult rats harbour virulent leptospiræ? It seems possible that sporadic cases of Weil's disease are not so uncommon as published accounts would suggest, and possibly some cases diagnosed as catarrhal jaundice are in reality mild cases of Weil's disease.

At the same time, the spirochætal group of organisms present many obscure problems of virulence, and it is possible that strains may vary in their virulence to certain animals. These are matters that can only be cleared up by further experimental work. The work of recent workers in this field, particularly the recent important study by Buchanan, has shed

[1] Since this was written the serum of another dog which had recovered from enzootic jaundice has been examined. 1 c.c. of this serum protected against certainly lethal doses of leptospiræ obtained from both the dog and the rat.

much light on the biology of the leptospiral group, but at the same time has shown how complicated are their activities in nature and in disease.

If enzootic jaundice in dogs is, indeed, a leptospiral infection, the main line of both prophylaxis and treatment is obvious.

In the enzootics we have investigated, we have found that the affected kennels have always been infested with rats. In the only three outbreaks in which we have been able to examine the rats from the kennels we have found that they harboured virulent leptospiræ. A source of infection by leptospiræ is therefore not far to seek, and we consider that anti-rat measures offer a most promising method of prophylaxis.

The use of anti-leptospiral serum in both prophylaxis and treatment has already been put to the test, and although our experience is small, we believe we have obtained significant results. We are also testing the value of leptospiral vaccines in the prevention of the disease.

In conclusion, I should like to take this opportunity of thanking my veterinary colleagues, Mr. Dalling and Mr. Pugh, for giving me the opportunity of co-operating with them in their work on this very interesting subject of what may be properly called comparative pathology.

Mr. L. P. PUGH: Clinically, canine jaundice caused by *Leptospira icterohæmorrhagiæ* can be classified as (1) the hyperacute type, (2) the icteric type.

Hyperacute Type.—This form is characterized by a rapid febrile course, extreme prostration, complete inappetence, and the passing of blood-stained mucus-coated fæces. Hæmorrhages from the lips, gums, nose, and occasionally from the eyes and face, are observed. Dyspnœa frequentiy occurs, accompanied by a short cough, and great thirst is often observed. Death may take place in a few hours or the dog may live for several days. Icteric symptoms are only occasionally met with.

Icteric Type.—This form is generally referred to as "yellows" by the lay public, because of the coloration of the various mucous membranes and tissues. The onset and course vary. Some dogs show icteric symptoms when first affected, whilst in others, the disease commences with general malaise, discharge from the eyes and nose, constipation and loss of appetite, the discoloration of the tissues appearing at a later stage. It is rare to find the initial temperature above 103° F., and it usually falls to normal or subnormal on the appearance of icterus. The buccal mucous membrane is at all times normal except for the yellow coloration. Vomiting is a constant symptom, the vomitus often being blood-stained. Constipation is usually marked in the early stages, and later gives place to diarrhœa with blood-stained stools. The urine is highly albuminous and dark-coloured. The death-rate is high—usually over 90 per cent., intussusception of the intestine being apparently an important contributory agent in the causation of death.

Differential Diagnosis.

Canine Piroplasmosis.—It is doubtful if this condition ever occurs under natural conditions in this country. We have examined many blood-smears from naturally occurring cases of infectious jaundice and on no occasion have we observed any evidence of an intracorpuscular parasite.

Moreover, we have treated many naturally occurring cases of infectious jaundice with trypan blue and have failed entirely to obtain the same gratifying results that are obtained abroad with this treatment. A perusal of the literature on canine piroplasmosis brings out considerable differences between it and the disease we are now considering, though in some points there is apparent agreement. In piroplasmosis, hyperæmia of the internal organs, especially the liver, kidney and bone-marrow, is common. There is marked enlargement of the spleen, and the gastro-intestinal tract is pale; in leptospiral jaundice these features are uncommon. In both diseases the mesenteric lymphatic glands are red and enlarged, and hæmorrhages may occur on the serous membranes. In these points there is agreement. Again, recovery is said to be common in piroplasmosis, while in jaundice there is death in at least 90 per cent. of affected cases.

Mr. T. DALLING: Dr. Okell and Mr. Pugh have discussed leptospiral jaundice from an experimental and a clinical standpoint. I propose to add a little to what has already been said, taking two points only: (1) a comparison between the naturally occurring and the experimentally produced disease; (2) prophylaxis.

Comparison Between the Natural and Experimental Disease.

As Mr. Pugh has pointed out we classify naturally occurring leptospiral jaundice as hyperacute, hæmorrhagic, and the icteric, i.e., the form in which dogs die early in the course of

the disease without showing yellow coloration of the mucous membranes and skin, and that in which the course is more insidious and the characteristic symptom is a general icterus. Of the ten naturally occurring cases we have examined, three were of the former type and seven of the latter. In no experimentally produced case of the disease have we seen the hyperacute hæmorrhagic type. Of the eighteen dogs used for experiment with dog-strain leptospira, sixteen developed marked jaundice symptoms, and all of the three dogs inoculated with the rat strain became typical clinical cases of the icteric type.

The points of resemblance between the natural and experimental disease during life are the following: (*a*) Constipation early in the course, giving place later to loose, clay-coloured stools, often blood-streaked and mucus-coated. (*b*) Vomiting; with the experimental dogs this does not occur till the third or fourth day after infection. It is not a constant feature of either disease to find blood in the vomitus, but it may be present. (*c*) An elevated temperature preceding the development of icterus with a fall to normal or subnormal immediately following the appearance of icterus and continuing to fall till the death of the dog. (*d*) The appearance of general icterus. The time that it takes for icterus to make its appearance after the first symptoms are observed in the naturally occurring cases is shorter than that needed for its development after inoculation of leptospiral material in the experimental dogs. This may be accounted for by the time taken for the onset of symptoms after infection, for we know that it is common for experimentally infected dogs to show nothing abnormal till jaundice appears, and yet death may occur the following day. (*e*) Depression and general malaise occur in both forms, and though they may not be marked early in the disease, they become very obvious as the disease progresses, and for hours the dog may lie as if dead before death actually occurs. Such symptoms as hæmorrhages of the gums, discharge from the nose, &c., are seen in both conditions, though they are not common in either form.

From the standpoint of morbid anatomy the natural and experimental diseases are identical, and while in neither form are constant gross lesions found, yet in both the essential lesions are present. These lesions can be grouped under the term hæmorrhage, for no matter in which tissue the lesion occurs it is essentially a hæmorrhage. The tissues most commonly involved are the lungs, intestine, lymphatic glands and serous membranes. The *lungs* exhibit hæmorrhages varying much in size. Cases have been seen in which the whole lung is involved, while on the other hand, we have observed other lungs where it was only after careful search that small lesions could be located. In the *intestine* the lesions may be confined to small areas or the whole tract may be involved. The hæmorrhages are subserous, and submucous. Any or all of the *lymphatic glands* may contain lesions, but in all cases, both natural and experimental, the mesenteric and bronchial gland lesions are constant. The gland may show minute hæmorrhagic points or it may be enlarged, congested and œdematous. The lumbar and hepatic lymphatic glands are usually but not constantly affected. Hæmorrhages occur on any or all of the serous membranes, and, as in the case of other tissues, the size and appearance of the lesion vary.

Prophylaxis.

The general lines upon which prophylactic measures should be undertaken have been pointed out, i.e., the use of anti-leptospiral serum and the eradication of rats from kennels. We have shown experimentally that anti-leptospiral serum has the power of protecting dogs from developing leptospiral jaundice even after infection has taken place. Eleven dogs were inoculated intraperitoneally with guinea-pig liver containing the parasite in doses which experience had shown were sufficient to produce death in five to six days. Twenty-four hours after inoculation two dogs received 10 c.c. of antileptospiral serum. Forty-eight hours after inoculation, two more received a similar inoculation. On the third day yet another two received such an injection, and on the fourth day four more were treated, one being left untreated. Those dogs inoculated with serum on the first, second and third day following infection, presented no abnormal characters at any time, while all the others, i.e., those inoculated on the fourth day, and the control dog, developed the typical icteric form of the disease and died. We have shown many times, under carefully controlled conditions, that the inoculation of antileptospiral serum at the same time as virulent leptospiral material into guinea-pigs, has given a complete protection. In the field we have also had proof that protection is afforded by the use of the above serum; hence it can be definitely laid down that an important prophylactic measure is the use of such serum. It may be necessary to repeat the inoculation of serum if the dogs are still believed to be exposed to infection.

The presence of leptospiræ in rats and the association of infected rats with disease-producing kennels induces us to believe that such rats play an important part in the transmission of the disease. Since publishing an account of our experiments we have received several communications from veterinary surgeons and others who have had experience of this condition. They draw attention to the association of jaundice with rats, and two state that the infected dogs had been bitten by rats before the appearance of symptoms. We are not in the position to say that the bite from an infected "carrier" rat may give rise to leptospiral jaundice, but we believe that the common means of transmission is food and bedding soiled by rat urine containing living leptospiræ. We have no evidence that lice or other arthropods play a part in transmission. Many, but not all, infected dogs harbour lice.

In dealing with prophylaxis the fact that one of our experimental dogs, which recovered after showing icteric symptoms, still harboured leptospiræ in the kidneys for some weeks, must be borne in mind. Recovered dogs, therefore, should be treated as "carriers" until it has been proved that they do not excrete leptospiræ in the urine.

Dr. A. BALFOUR said that Dr. Okell and his colleagues had done an excellent piece of work. It was curious that the discovery they recounted had been so long delayed considering that canine jaundice was by no means a rare disease in this country.

This by no means detracted from the credit due to Dr. Okell and his colleagues for the way in which they had thrown fresh light upon dark places. He thought that the influence of this important work would spread far beyond the confines of this country and might well have a bearing on human tropical pathology.

To take the West Coast of Africa, where there was much confusion at present as regards the acute febrile disorders of man associated with jaundice there. We had blackwater fever, and yellow fever,—at least, so-called yellow fever, for none had yet demonstrated the leptospira in West Coast cases. He understood, however, that the Americans would soon be on the spot to settle the question.

Then there was a condition described by French observers, and known as spirochætosis ictero-hæmoglobinuriæ—yet another febrile state associated with icterus. The question required thorough exploration, and he hoped and believed that this inquiry into the ætiology of enzootic jaundice of dogs, if continued and amplified, would help in the solution of these West Coast problems.

He would like to ask the authors of these papers if hæmoglobinuria was present in any of their cases, either in the natural disease in dogs or in their inoculated animals. This was possibly a matter of some importance.

He would also like to be told whether Dr. Okell and his colleagues had looked for lime casts in the kidneys. Hoffmann, of Havana, had recently done an interesting piece of work. He had collected the organs of a number of people who had died of true yellow fever in Havana during the last outbreak of the disease there. In the kidneys he had demonstrated the presence of lime casts, and he found these also in the kidneys of animals inoculated with yellow fever. He regarded the appearance as pathognomonic of that disease, for it was not seen in the case of Weil's disease or other forms of acute febrile jaundice. If Hoffmann was correct, there should be no lime casts in the kidney material examined by the present authors. It would be instructive to know if this was the case.

He had been much interested in the remarks upon the probable method of acquiring infection. Much obscurity surrounded Weil's disease in this respect. He noted that the evidence of insect transmission was negative so far as it went, and this was true of human infection. It was a curious thing, because there was no doubt that in yellow fever, which was also due to a leptospira, *Leptospira icteroides*,—practically indistinguishable from *Leptospira icterohæmorrhagiæ*,—the only method of transmission was by means of an insect, the mosquito, *Stegomyia fasciata*, or *Aëdes aegypti*, as it was now usually called.

Yellow fever and Weil's disease very closely resembled each other, especially perhaps in the inoculated guinea-pig. To the naked eye there was practically no difference between the post-mortem appearances presented by a guinea-pig dead of infectious jaundice and one which had perished from yellow fever infection. Microscopically, however, the liver showed more pathological changes in yellow fever.

Probably the authors were correct in attributing infection to contaminated food. The urine often contained leptospiræ. There was a good deal of evidence to show that Weil's disease could be contracted by patients bathing in polluted water, as for example, in certain reaches of the Seine. He himself had seen a case in a man who had not been in contact

with rats, but who had developed the malady after immersion in the Thames at Gravesend. It was, of course, impossible to say whether this accident was actually to blame, but no other cause was apparent.

Sir WILLIAM WILLCOX congratulated the authors on the brilliant manner in which the subject had been worked out and also on the practical result in obtaining a serum which appeared to have a high therapeutic value both from a prophylactic point of view and as a remedy in acute cases. He had seen a few weeks ago a case of severe jaundice in a young foxhound, this being associated with fever and marked emaciation. The case terminated fatally about the tenth day. Canine jaundice of a severe type with high mortality appeared to be common among foxhounds.

He (Sir William Willcox) said he had had a great experience of jaundice during the war; he saw an enormous number of cases. In Mesopotamia and in the Dardanelles jaundice was extremely common among the troops and was of the epidemic, catarrhal type; the mortality was only about 0·5 per cent., the disease usually being quite mild.

The most careful investigations were carried out in Mesopotamia by Colonel Ledingham and Colonel Mackie, who made special researches in order to discover a spirochæte, if possible. In all cases the investigations were negative, and it was clear that the jaundice in Mesopotamia and the Dardanelles was of the epidemic, catarrhal type and not due to a leptospiral infection. A certain number of cases of leptospiral jaundice occurred in France, and among these the mortality was much higher than that occurring in the catarrhal type, also the symptoms were entirely different.

It was quite clear that in human beings leptospiral jaundice was comparatively rare compared with the epidemic catarrhal type.

He (Sir William Willcox) wondered whether the same condition held good in the case of dogs: in other words, did a mild form of jaundice of the epidemic, catarrhal type exist or was all the jaundice of the leptospiral variety? From analogy with human medicine it would appear probable that both varieties of jaundice occurred in dogs, though the high mortality pointed to the leptospiral jaundice being perhaps the commoner.

Mr. HENRY GRAY said that with regard to the peculiarity of lung lesions so frequently encountered in enzootic jaundice, he had pointed that out as far back as 1897. About twenty years ago, after making an extensive study of the disease, he was forced to the conclusion that it had neither any connexion with distemper nor with piroplasmal jaundice, which was frequent in tropical, subtropical and Continental countries and especially in France and Holland. With the exception of the enlargement of the spleen seen in chronic cases and of the frequency of hæmoglobinuria in acute cases of piroplasmosis, there seemed to be very little difference between the morbid anatomy, symptoms and course of that disease and of our form of malignant jaundice, in one case of which, however, he had observed hæmoglobinuria. In many cases the lungs and other organs were free from gross lesions.

He thought it was wrong to say that 95 per cent. of cases of jaundice were fatal. This statement might be true for particular outbreaks or certain seasons, but it was not correct as regards all outbreaks, especially during some periods when the disease was mild and the majority of cases recovered even without any treatment. In some parts of the country, especially in that part from which he came, where rats, dogs and rat-catchings were frequent, the disease was not known.

It was chiefly a disease of packs of hounds or of sporting dogs, in kennels of which it recurred from time to time with more or less malignancy after various intervals. Although he had, during the past forty years, seen thousands of cases of distemper and typhus (epizootic gastro-enteritis) in London dogs he had never seen more than two or three cases of jaundice that he could attribute as complications of distemper, and none to typhus. All cases of jaundice other than those associated with some structural disease of the liver or of the surrounding organs were seen in the country or brought from there to London.

As to a spirochæte or leptospira being the cause, he reminded the authors that such a parasite had been found by many observers, not only in various lesions in different diseases such as jaundice and typhus, but also as a normal habitant of the dog's alimentary canal. Although these gentlemen had brought forward a good deal of evidence in support of the leptospiral theory, which was first advanced and then retracted in 1920 by a gentleman present, they had not, in his opinion, adduced any proof of its validity.

Mr. F. C. GILLARD said that he had for many years been particularly interested in spirochætal jaundice. From his earliest days he had seen periodically very many cases of

jaundice in foxhound puppies between the time of weaning and being sent away to walks; and again, after their return to the kennels as young hounds. The mortality had been very high and the disease had created great financial loss. Whilst mentioning these particulars he said he would offer the suggestion that the disease to which young hounds fell victims in this way might be due to the actively infected rats being more constantly found in kennels, or due, more probably, to older hounds being carriers of the leptospiral infection.

Since 1899 he had, at certain seasons and very often after a few foggy nights, seen many cases not only in connexion with packs of hounds, but in other dogs, particularly in those living in dog-kennels out of doors, especially in damp low-lying districts. These invasions were perhaps due to rats leaving the ditches and brooks at these times and entering buildings for warmth.

In 1905, Theobald, in an article on "Flies and Ticks as Agents in the Distribution of Disease," mentioned that in blood-smears from harrier puppies dying from jaundice, organisms similar to those noticed in canine piroplasmosis had been observed. But as these specimens had been derived from his (the speaker's) cases and as he afterwards had proved that these bodies were not piroplasms, but either blood-plates or nucleoli of polynuclear leucocytes, he considered that the error should be specially pointed out in the event of this reference being quoted at any future date.

It was in 1906 for the first time, that he had had any direct proof of jaundice blood being infective to other dogs; from a natural case in a harrier puppy a fox-terrier succumbed to inoculation, and a second terrier inoculated from the first terrier also died, both on the thirteenth day after inoculation, from typical jaundice.

In 1908 he found many spirochætes on staining a smear from the duodenum of a jaundiced puppy, and at first he thought he had discovered the causal organism, but afterwards he demonstrated similar spirochætes in this position from an apparently healthy puppy.

These observations were recorded at a meeting of the South-Eastern Veterinary Association in May, 1920, and had been referred to that evening by Mr. Henry Gray. At the meeting just mentioned he further stated that he had seen occasional but very indistinct forms of spirochætes in smears taken from the kidney and stained by Leishman's and Giemsa's methods, but this was not included in the report as given in the *Veterinary Record*, July 3, 1920, pp. 5, 6.

He emphasized what Mr. Gray had stated in regard to the close similarity of the leptospiral disease with the canine piroplasmosis, excepting the rarity of dark-coloured urine and entire absence of splenic enlargement. The characteristic fall of temperature to subnormal when death took place was most constant, and further, the microscopic changes in the blood were nearly identical, namely, destruction of red cells, increased numbers of leucocytes, the appearance of nucleated red cells, and marked phagocytic action of the polynuclear leucocytes.

The opening speakers, in dealing with the symptoms, though mentioning coma, had not sufficiently emphasized the importance of this symptom in the so-called icteric type. And they had omitted to refer to what was undoubtedly a most outstanding feature, viz., rapid and extreme wasting. Yawning and stretching were also a constant symptom in early stages. Intussusception was certainly very common; could this be due to irritation of the muscular coat of the small bowel by large numbers of spirochætes which were to be found in the duodenum in particular? Duodenal ulcers had not been mentioned; these were fairly common in occurrence. Also the yellow staining of the brain and its meninges had apparently not been seen by the opening speakers. He would be glad to be informed if cases had been seen in the cat or ferret; and as ferrets were constantly coming into contact with rats and their homes the inquiry should be specially extended to them.

Section of Comparative Medicine.

President—Mr. FREDERICK T. G. HOBDAY, C.M.G., F.R.C.V.S.

Immunization of Animals and Man against Tuberculosis.

By NATHAN RAW, C.M.G., M.D.

THE problem to which I wish to direct your attention to-day is that of the immunization of animals and man against tuberculosis. Such an enormous amount of work has been done all over the world on this subject that it would be impossible in this short paper to attempt even a brief survey of it, so I propose to limit my observations to my own work in the hospital and laboratory during the last twenty-five years, and to place before you the results which I have obtained.

Tuberculosis is one of the most serious diseases the community have to face and a general knowledge of its contagious nature is very necessary for agriculturists.

Bovine tuberculosis has been, and is at present, the subject of great controversy in the scientific world, and the fact that it is freely communicated to children through infected milk, causing an immense amount of suffering and death, is an urgent reason for using every endeavour to eradicate the disease from dairy cows.

Milk being a vital necessity for children it is imperative that it should be tubercle-free, and an immense amount of thought and effort has been given to this subject by the veterinary profession and scientific workers. The Legislature has passed many Acts of Parliament and framed innumerable orders and regulations with a view to eradicating the disease; immense sums of money have been voted in the way of compensation, and yet to-day high veterinary authorities state that there is no diminution in the number of tuberculous dairy cows.

I have great hope that the Tuberculosis Order, which was suspended during the war, will have a most beneficial effect, and I trust it will be put in operation as soon as possible.

It is estimated by competent authorities that at the present time over a million dairy cows are tuberculous and that 5-10 per cent. of these have udder tuberculosis, but one cannot be certain that only the udder cases will yield tuberculous milk, and the fact that the milk of a whole herd is mixed before delivery may render the whole yield dangerous. It is also estimated that about 10,000 children die every year in the British Isles as the result of drinking tuberculous milk, to say nothing of thousands of others who suffer for years or are permanently crippled. These figures are appalling, and no effort should be spared, monetarily or otherwise, in endeavouring to suppress it. The tubercle bacillus is the sole cause of all this distress, the disease is always contagious, and for all practical purposes we can disregard heredity as a causative factor. Almost all children and animals are born free from tubercle; if they are not exposed to infection they will never develop the disease.

It has been said by many writers that the tubercle bacillus is ubiquitous—that it is everywhere in the civilized world, and that we cannot avoid infection. I cannot agree with that view, and I will give you my reasons: For twenty-five years I was continuously in charge of large general hospitals, and in addition to treating over 100,000 patients in the wards, I had the opportunity of personally conducting over 7,000 post-mortem examinations. In fully 50 per cent. of these cases the most careful search for tubercle was negative, and in certainly 75 per cent. of the admissions to hospital no clinical evidence of the disease was manifest. It is quite true that a large number of people are infected in early life by bovine

bacilli, causing enlarged glands and mesenteric infection, but these lesions either suppurate or calcify, and the patient is not aware of having become infected.

The same applies to cattle and pigs. Thousands of them receive mild infections which never progress in the body, and yet, when they are tested with tuberculin, give a more violent reaction than those animals in an advanced stage of the disease. The late Professor Bang, of Copenhagen, showed conclusively that a herd remained tubercle-free until an infected animal was introduced, and then very often a large percentage of the herd became tuberculous.

Observations and experiments show that tuberculosis is not met with frequently among those breeds of cattle which are usually kept out of doors, while on the other hand those breeds which have been kept indoors in warm cowsheds, and which have been forced for the production of milk, become infected with the disease to a very great extent. The Shorthorns, Ayrshires, Jerseys and Guernseys are the breeds of cattle in which the disease is most prevalent. It is well known that in the Channel Islands the disease is almost unknown, but if these delicate cows are brought to this country they often contract the disease.

The most frequent method of infection amongst cattle and pigs is the milk of the infected animal, or the insanitary and contaminated condition of the cowshed. If we could eliminate these two factors we should be in control of the disease, but we cannot and, in my opinion, we never shall. Neither can we hope to control them in human surroundings; so long as we have children compelled to live in insanitary and overcrowded rooms—very often in close association with members of the family suffering from open tuberculosis—so long will infection continue.

Better conditions of housing, more nourishing food, higher wages, and vigorous action by the health authorities and tuberculosis officers have, I am glad to say, substantially reduced the incidence and mortality from human infection in the form of pulmonary tuberculosis; yet we still lose 40,000 persons every year from it. But the same reduction has not taken place as regards bovine infection to children through milk, and this is a matter to which our energies should be primarily directed. Feeling like many other workers that the only hope lay in the immunization of cattle against infection, I attempted this work in a series of animals in 1905, but failed completely.

In 1901 I published a paper in the *British Medical Journal* stating my view that, as a result of clinical observation: (1) human and bovine infections produced separate and distinct diseases in the human body, and that they were antagonistic to each other; (2) that human bacilli caused primary pulmonary tuberculosis, and that bovine infection caused enlarged glands, mesenteric disease, bone and joint lesions, &c.; (3) that patients suffering from pulmonary tuberculosis rarely developed bone-joint or gland lesions, and vice versa. In fact the human was protected by one infection against the other. Cattle and pigs are immune against infection by human bacilli, but the human subject is susceptible to both. It was evident to me at that time that it would only be possible to immunize cattle by using human bacilli. A series of animals were tested in this way, but again failure resulted. The bacilli used were too virulent and toxic, and in every case killed the animals.

In 1906 I decided to attempt to attenuate the bacilli by repeated subculturing, thus weakening virulence and removing toxicity. The only certain way of permanently attenuating virulent cultures of tubercle bacilli is by long-continued and systematic subculturing spread over a number of years.

In 1906 Prof. Koch gave me, when I was in Berlin, a pure culture of glycerine agar of human bacilli from a virulent case of pulmonary tuberculosis. Prof. Calmette, of Lille, supplied me with a virulent bovine culture from the mesenteric glands of a cow, and Prof. Bang gave me a culture of avian bacilli from the liver of a chicken.

These cultures have been subcultured on glycerine potato and transferred to glycerine agar every month for eighteen years in my laboratory, and the present

growth which I show you to-day represents the two hundred and sixteenth generation. At the end of 1914 I was afraid the work would be interrupted but I took the cultures with me to France and as I had an excellent laboratory in the hospital I was able to continue the subculturing for the next four years. The growths are still profuse and true to type, but they are now quite non-tuberculogenic and completely non-pathogenic to animals.

Every year since 1906 I had injected these bacilli into animals with a view of testing their pathogenicity ; until the year 1913, that is the eighty-fourth generation of subcultures, I noticed no change in their virulence. After that time attenuation became marked, and in a series of animal inoculations of these bacilli in 1914 they were observed to be avirulent.

These experiments, which have occupied eighteen years, conclusively prove that virulent tubercle bacilli can be attenuated to such a degree as to be avirulent and non-pathogenic to highly susceptible animals.

A further series of animals were inoculated a year ago with a protective vaccine prepared from attenuated cultures of human bacilli, and these animals, together with unprotected controls, were all given ½ mg. of virulent bovine bacilli. All the unprotected animals rapidly developed general tuberculosis and died within four months, whilst the protected ones are all alive to-day, and apparently none the worse for their treatment.

If an animal can resist the laboratory dose of virulent bacilli, it can certainly resist the comparatively mild infection in its ordinary life. If we can produce some immunity by artificial means either in man or animals, sufficient to enable them to resist ordinary infection, we shall have gone a long way towards stamping out tuberculosis.

The vaccine that I have prepared from these attenuated cultures is a bacillary emulsion containing all the products of the bacillus. Living cultures are not used, as the bacilli are destroyed by heat before preparation.

For the purpose of immunizing calves, they should be separated from the mother at birth and fed on healthy milk, or the pasteurized milk of the mother, for a week, when the first dose of vaccine is given, followed in another week by the second dose of vaccine. They then associate with the other animals. It is given hypodermically in the usual way, with strict antiseptic precautions, under the skin of the abdomen. It is only applicable to non-infected animals.

During the last three years the vaccine has been used in valuable pedigree herds, where careful records have been kept, and given by a careful veterinary surgeon. Up to the present over 200 calves have been vaccinated. At the end of a year, and in many cases again at the end of the second year, these calves were tested by Pasteur's tuberculin, and in practically all cases the result has been so far negative.

I am awaiting results from other veterinary surgeons, which will be published as soon as possible, but so far the results are decidedly encouraging, and as the vaccine is perfectly harmless I hope it will be given an extensive trial ; it is only by the results in a large number of animals that its final value can be determined.

The problem of tuberculosis resolves itself into the question of immunity. The protection of the community by some safe scientific method of active immunization is the only effective way by which this preventable scourge can be eradicated.

Turning to the question of human immunity the problem is much more difficult, as children are susceptible to both human and bovine infection. At the special request of parents in a large tuberculosis colony in which I am interested, and where each father is affected with pulmonary tuberculosis, I gave all the children—about 100—a year ago two doses of protective vaccine prepared from bovine bacilli with the object of protecting them against human infection. So far they appear to be quite healthy, but it will require years of observation before we can give any definite opinion as to its efficacy. I believe that in time it will be possible to

immunize children against tuberculosis, especially those who are directly exposed to infection.

It is impossible in this paper to discuss the theory of Ehrlich, the demonstrator of the mechanism of antitoxins, or those of Behring and Pfeiffer, the discoverers respectively of antitoxin and the bacteriolytic action of immune blood-serum, or the theory of Gruber and Widal, the originators of the agglutination tests. All point irresistibly to the conclusion that the human body possesses means of defence other than that obtained alone from the phagocytic action of the leucocytes. The tubercle bacillus has been found to respond to no bactericidal or bacteriolytic action of the blood and, generally speaking, to no antitoxic influence, after the manner of diphtheria and tetanus. In the light of recent investigations it is apparent that in tuberculosis phagocytosis still remains a most important element in the production of immunity (Bonney). The brilliant work of Wright and Douglas has taught us a great deal in this connexion.

The monumental work of Pasteur regarding anthrax is a lasting tribute to his genius. In 1877 widespread epidemics of anthrax had been carrying off enormous numbers of sheep and cattle in France and Prussia. Some 56,000 head of cattle were destroyed. Pasteur succeeded in artificially growing the bacillus of anthrax in broth. He then proceeded to Chartres, where the epidemic had produced a loss in stock of a million pounds, and he studied the disease on the spot. He found that if he inoculated cultures, grown artificially at 98·6° F., into sheep, they all died, but that if he grew them at 108° F., at which the organism could still grow, but could not form spores, and kept it a fortnight before use, a mild or benign form of the disease was produced from which the sheep recovered and were then found to be immune to the ordinary fatal form of anthrax. By this means the disease was practically stamped out.

Vaccine therapy is still in its infancy, and we are specially conservative about it in this country. The natural and specific remedy for the prevention and treatment of tuberculosis exists in the products of the bacilli themselves.

When Koch introduced tuberculin as a remedy, the whole world clamoured for it, and doctors rushed to Berlin to beg for a few doses. I used it in 1902 in the treatment of a severe case of lupus in hospital, and the results were disastrous. The reaction was terrific—temperature 106° F., a violent inflammatory condition of the lupus, severe malaise, and constant vomiting for twenty-four hours. I thought the patient would die, but the symptoms slowly subsided and he recovered. Nevertheless the state of the lupus was not changed. The tuberculin was prepared from virulent cultures and the dose excessive.

Since that time and for twenty years afterwards tuberculin fell into disrepute, but I have no doubt it will come into its own again, and preparations from non-virulent attenuated cultures, which produce no reactions, will be of the greatest service in the treatment and prevention of all forms of tuberculosis.

Attenuated cultures are still specific; they produce tuberculin, but have been deprived of their virulence and toxicity, thus rendering them perfectly safe for use.

We cannot cure a case of malignant small-pox by vaccination, but we can for a certainty prevent the patient getting it, by using an attenuated or modified vaccine.

The war has taught us a great deal as to the value of vaccines and serums. In the first year of the war numerous wounds were complicated by tetanus, but after it became the routine practice to give antitetanic serum in all wound cases, tetanus was rarely seen. Antityphoid inoculation worked wonders, and when a combined vaccine prepared from dead cultures of typhoid and paratyphoid bacilli was used generally, the cases of pyrexia of uncertain origin rapidly disappeared. A very small amount of such a vaccine is sufficient to protect a person from infection. I believe there is no such thing as natural immunity to tuberculosis; all mankind is liable to attack,

and pulmonary tuberculosis, although healed, does not confer immunity in after life.

It had long been thought that avian tubercle bacilli could not be conveyed to either animals or man, but recent researches in Denmark have shown that it is common for pigs and cattle to be attacked as the result of tuberculosis amongst chicken on the same farm. Regulations in many parts of Denmark now forbid cattle and chicken to be reared on the same farms.

CONCLUSIONS.

(1) Human and bovine bacilli are separate and distinct types of a common species.

(2) The human body is susceptible to both.

(3) Cattle and pigs are not infected by the human bacillus, whilst horses, goats, dogs and sheep are not susceptible to either.

(4) *An infection of a person by human bacilli* may cause (*a*) primary pulmonary tuberculosis; (*b*) tuberculous laryngitis; (*c*) secondary intestinal ulceration; (*d*) lupus and fistula.

(5) *An infection by bovine bacilli* may cause (*a*) tuberculosis of lymph glands; (*b*) abdominal tuberculosis; (*c*) lesions of bones and joints; (*d*) meningitis; (*e*) acute miliary tuberculosis; (*f*) lupus and (rarely) secondary extension to lungs.

(6) These infections in the body are antagonistic to each other, and the bacilli are practically never found in the body at the same time.

(7) A case of pulmonary tuberculosis rarely develops any enlarged glands or bone and joint lesions, and vice versa.

(8) It is possible to immunize a healthy animal by using a vaccine prepared from an attenuated, non-toxic and a non-virulent dead culture of human bacilli.

(9) It will be seen that we have yet a great deal to learn about tuberculosis.

Discussion.—Mr. HENRY GRAY said he did not quite agree with all that which was said on the subject by Dr. Nathan Raw. Cattle, however badly fed or housed or deeply milked, would not develop tuberculosis unless they had a tuberculous animal in their midst. It was purely a contagious disease. Similar agencies, such as overwork, insanitary stables and bad feeding had in the past been invoked as a cause for prevalence of glanders in the horse, which was a disease analogous to tuberculosis. Yet in spite of such theories this country had been rid of that scourge by means of the mallein test and the slaughter of the reactors. He thought that a similar practice, that was to say placing all the cattle under veterinary control, testing them with tuberculin and slaughtering the reactors would be the quickest, surest and cheapest method of getting rid of the plague and of protecting the population of these islands against its ravages. To allow ten thousand children to die annually of this preventable bovine malady was a disgrace to this country. What was the use of individuals going to the trouble and expense of freeing their herds when the majority of the reactors were sold in the open market to scatter the disease far and wide? The sale of reactors was the prime means of increasing its incidence. Again, as long as the majority of cattle owners were left alone to do as they pleased and to spread the disease or to keep it alive, what security had those who had freed their herds from it against its re-introduction? Unless the test was applied to all the cattle, and the reactors eliminated, they would never abolish its ravages from this country. Assuming that Dr. Nathan Raw's proposed method of vaccinating young calves was really efficacious in the prevention of the malady, of what great value would it be if it were not compulsorily adopted in every herd? If it took twenty years by this method to eliminate the disease it would not during that period prevent its incidence in children, with many thousand deaths resulting. He did not believe in any temporizing method, but thought that what the United States Department of Agriculture was doing in the matter was right to do here, namely, to eradicate speedily the disease from our cattle. Up to the present the U.S.A. Department had tested 6,000,000 head of cattle and passed as accredited, or tuberculosis free, 60,000 herds containing 1,100,000 cattle, while the remainder that had not reacted at the first test were undergoing further testings. All the reactors were slaughtered and the owners compensated up to as much as 90 per cent. of their pre-testing value. There were still on the waiting list over 2,000,000 head of cattle included in 200,000 herds or lots. In the supervision list there

were nearly 9,000,000 cattle, included in just upon 900,000 herds or lots. During the time we were considering our tinkering proposals the mortality among our children and the manufacturing of cripples and chronic invalids were continuing. Surely humanity deserved the first consideration, and the best way of observing this principle was to adopt the practice of the United States Department of Agriculture.

Dr. HALLS DALLY said he found especially many points of interest and of agreement in the remarks which Dr. Nathan Raw had made with reference to tuberculin immunization, and could only admire the painstaking manner in which these researches had been carried on over a period of so many years. At the same time, he (the speaker) was of opinion that Dr. Raw had attacked a small portion only of a much greater problem, and that the results of his experiments so far went to prove the absence of infection rather than the production of immunity to tuberculosis by the injection of attenuated subcultures of dead bacilli. It might afford useful information at this stage to expose the injected calves to the possibility of direct infection, and to await results.

As regarded humanity, while large numbers of the community became infected, only a minority of these developed evidences of active tuberculosis. What was the basic factor which determined latency or activity? This factor was to be found in the natural immunity of the individual, which might be enhanced or diminished by circumstances of environment, using this word in the broadest sense, of which good hygiene formed only a part.

The use of tuberculin had largely gone out of fashion. This was to be regretted, since from an experience of many years one knew that by the intelligent and gradually increasing application of tuberculin, the tolerance to it of an infected individual could be greatly increased. This fact increased the belief that tuberculin was a true immunizing agent, and certainly from the point of view of prophylaxis, especially in the case of young children, its more extended employment was to be advocated. Nevertheless, if a non-tuberculous animal were given injections of tuberculin, its susceptibility to tuberculous infection remained unaltered.

Dr. Rowlands had made a statement of considerable importance in saying that he had succeeded in eradicating tuberculosis from a herd of badly diseased animals by means of appropriate vitamin feeding,[1] and one would like to know more of his methods. If such successful results could be accomplished in the case of one herd, it might perhaps not be unreasonable to hope for a much more universal adoption once the method became known. Isolated work, however careful, was of little avail, and with the greatly increased output of modern knowledge, it might be possible at not too distant a date for medical men and veterinarians to formulate a combined scheme of prevention.

Mr. G. P. MALE said that any method of immunizing cattle against tuberculosis would be welcomed by the veterinary profession, who were doing their best to combat this scourge. He would like to see further experiments carried out by feeding the immunized calves on raw milk from cows affected with tuberculosis of the udder. If they resisted this infection for a fortnight he would believe in its efficacy, as he had found that calves very quickly became affected with tuberculosis when fed in this way. He believed that all cattle were very susceptible to tuberculosis and that very few indeed had any strong immunity. If Dr. Nathan Raw's method was compared with that of Calmette it would be seen that in the former dead cultures were used and cattle did not react to the tuberculin test as a result of its use, whereas Calmette employed living cultures of attenuated organisms and argued that no immunity could be produced unless a more or less active focus of the disease was set up in some part of the body; and, as a consequence, the animal reacted to the test.

His injections were given once a year whereas Dr. Nathan Raw's were used only on the calf—twice in all. If this method was equally effective without causing a reaction it would be a great advantage. With reference to Dr. Rowland's contention that the eradication of tuberculosis was chiefly one of correct feeding, he could not agree with him, as he had found tuberculosis rife in cattle whatever method of feeding had been adopted.

[1] *Lancet*, 1925, i, p. 491.

Section of Comparative Medicine.

President—Mr. Frederick T. G. Hobday, C.M.G., F.R.C.V.S.

A Review of Some Problems of Foot-and-Mouth Disease.

By Sir Stewart Stockman, M.R.C.V.S.

There is a colossal amount of information about foot-and-mouth disease, so much that it would be impossible to summarize it for discussion, or even one-tenth of it, at one sitting. Further, a discussion on this subject and its problems admits of one wandering into the problems connected with filterable viruses in general, and intricate matters such as colloidal suspensions, filtration, and hydrogen-ion concentration. That expansion of the subject, of course, has very great advantages, because it opens up so many avenues for discussion to those who wish to follow them; but to cover such a wide field would be out of the question. Further, I am a member, like some others I see here, of a Committee which was appointed to carry out research into foot-and-mouth disease. The members of that Committee, although working independently, are also working in very close co-operation and collaboration as regards the interchange of results, and therefore I am anxious that in speaking at a meeting like this I should not give away anything which is not mine to give, and that I should not speak of some of the Committee's work which is only inceptive. Your Secretary, however, was good enough to give me one lead by saying that he would like me to bring here specimens which illustrate some of the lesions of foot-and-mouth disease. He told me that probably a good many members had not had the opportunity of making themselves acquainted with the lesions. Therefore, although it may be somewhat elementary, it was thought some Members might find it interesting. I propose to do that, and afterwards I will say a few words about the problems connected with the invasion of this country by foot-and-mouth disease; secondly, about the susceptibility of different animals, and, if time permits, make a few remarks on the problems of immunity. The subject is so large that it might be very well put on the agenda for another meeting.

For the information of those who have no clinical knowledge of foot-and-mouth disease, I would begin by saying that the incubation period is, not infrequently, as short as forty-eight hours; the average is five days. It would appear that, at least in some cases, the incubation period may last ten, fourteen, eighteen days, even three weeks. My own view of that is, that the records of these longer periods are based on the fact of animals having been removed from the seat of infection and, after that removal, falling in with the disease. I do not think these long incubation periods are valid; I think in those cases the animal may have carried away some of the virus, not in its system, but on its coat for example, and that after this longer period it has licked it off, and so introduced the virus into its system.

The slide now shown represents a specimen of a kind which it is difficult to get: the tongue of an ox with an unburst vesicle. At the tip of the tongue you see a burst vesicle. Both these lesions are of the same age, the burst one having been manipulated. Such a vesicle is usually spoken of as single, but it results from the coalescence or aggregation of several small ones.

Here is the tongue of an ox in a more advanced stage, a few days after the rupture of the vesicles, three of which can be seen on this tongue, all practically of the same age. The destruction of tissue is never very deep, never below the Malpighian layer. That accounts for one of the characteristics of these foot-and-

mouth lesions, namely, that they heal very quickly. When the vesicle has burst the epithelium begins growing, and in a short time the wound is healed and very little scar tissue is left.

The next slide shows the muzzle of an ox, and you see many burst vesicles where the epithelium has peeled over huge areas, leaving the rete beneath exposed.

Here is the snout of a pig, a common seat for lesions, and you can see a burst vesicle on the snout; the pig may also have vesicles on the body, or on the teats, particularly a suckling sow; and if a post-mortem be performed in the early stage you can sometimes see similar lesions in the intestines. This also applies to bovines.

Now with regard to the foot lesions. One of the suggestions of those who have studied the subject is, that the virus is most destructive of the horny layers of the epithelium. Whether that be correct or not, certainly when lesions form on the coronet, the horny structures may peel off; in other words, the hoofs are cast. The next slide shows the same lesion in a more advanced stage; here the separation is very evident, the sole being left bare. These are feet of pigs. The same process occurs in the ox. It has been suggested—and there is some experimental proof of it—that the virus may be retained in the inside of the horny layers of the hoof for many months, and when the hoof is cast it may be the starting point for a fresh outbreak among susceptible animals. That is what is called a "carrier." Those cases are not frequent. My own experience, after seeing thousands of cases, is that of having encountered only three outbreaks which had originated in that way, so far as I could, from the evidence, bring myself to believe.

Here is the same sort of lesion, showing a much more definite separation of the horn, and in this case one claw has been completely cast, showing the sensitive foot. It is curious how long it takes to cast these hoofs. In a normal hoof, it takes six to eight months for the layers at the top to grow down to the bottom. In some of the recent cases I have observed, where we had to adopt isolation in the case of very fine herds, I have been surprised to note that though the hoof is coming off, it has taken six, seven, or eight months for it to become completely detached; and it is after these long periods that recurrence takes place. Here in this slide you see well the bare pad of the foot. The next slide is a very exaggerated picture of a calf suffering from foot-and-mouth disease. With these sores on its mouth it begins to chew its own saliva into froth, as it were, and there is a constant smacking of the lips, which can he heard a good distance away. Thus the saliva is seen frothed up around the lips.

Foot-and-mouth disease is not a fatal disease; 4 per cent. in an ordinary outbreak, I should say, is a high mortality; but in some of the more malignant invasions it is 20 per cent. We have never had anything of that kind in this country, and we could not, because we resort to slaughter. But the trouble which arises here in connexion with the disease is the enormous dislocation of business it causes, due to restriction of movement to prevent spread of the disease; it disorganizes the meat supplies and the milk supplies. Not only have we to deal with the disease itself, but the Department in charge has to make provision, sometimes, for supplying big towns with their meat and milk. And there is also enormous damage done by the disease, especially to milch cows; there is the loss of the hoof, and, particularly, there is the damage which is done to the udder. A milch cow without a good udder is worth very little, and foot-and-mouth disease will reduce a cow which has cost £40 to £60 to something less than £10. These are the things which frighten the public and the farmer in this country, not the mortality which may arise from the disease.

Now a word on this question of invasion. Remember the insular position of Great Britain, the fact that susceptible animals are excluded from importation—you can put out of your minds what has recently happened in the case of Canadian cattle; they come in, but you can disregard that in dealing with this question, because

Canada, as far as we know, has never had an outbreak of foot-and-mouth disease in its territory. We can truly say, even now, that it cannot come in through the importation of animals. Remember, also, that we have years, or at all events very long periods, of complete freedom from the disease. Again, it has been the rule for years that if the disease breaks out in any country, automatically an order comes into force that no hay or straw can be imported from that country, and that is rigidly observed. But there is an exception to that, because the Department finds itself quite unable to forbid the importation of hay and straw as packing material. To do so would absolutely dislocate a large percentage of the trade of the country. Packing hay or straw must come under grave suspicion, but in all our inquiries into outbreaks we have not, except in a very few cases, been able to get evidence at all convincing that imported packing material might have been the cause of a particular outbreak.

The next thing to be remembered is that we only get invasions in this country, as a general rule, when the disease is prevalent on the Continent, particularly in Holland, Belgium and the North of France. Sometimes, from the circumstances of initial outbreaks, I have thought it possible that the Argentine was also a source of origin. The disease is often very prevalent in the Argentine, and it is allowed to go uncontrolled ; it is hopeless to attempt to control it in such a country. After my recent experimental experience with the virus and its viability under certain circumstances, I think it even more probable than I ever thought before that the disease may come from the Argentine, and that it may come from even longer distances, for reasons which will be given later. But the prevalence of the disease on the Continent is, I feel sure, the most important question.

Let us exclude importation of animals, as we may, and ask what remain as media of communication between farms and the infected world outside uninfected Great Britain. The answer seems at first obvious: the main link must be men from abroad or food-stuffs from abroad. We know that the virus can be conveyed on the hands, the clothing, &c., of human beings ; we have innumerable examples of a man going to an outbreak, and going afterwards to see his neighbours and leaving the disease, I will not say everywhere he went, but in many places which he has visited. We have had examples of veterinary surgeons who have gone to a case, who have changed their clothes afterwards, disinfected themselves, and gone round on their practice and have not spread the outbreak. But two or three days afterwards they have taken out their clothes again, without having had them properly disinfected, and then the disease has followed on their rounds. You cannot get over these observations ; they become so numerous as years pass that they practically constitute facts. The disease can be conveyed mechanically and by food-stuffs. There are many examples of food being removed from a farm before the farmer or the authorities knew there was an outbreak ; it has been taken to another place and the disease has been lighted up by it. I think we may also claim knowledge, from the happenings in practice, that the virus must remain active for considerable periods in some media and under certain conditions. But we have not definite knowledge of all those conditions, and of all the media which are favourable for prolonging the life of the virus ; it is one of the problems which call for research. I spoke of men and food-stuffs as carriers, but in practice, although we have definite instances, associated with established outbreaks in this country, in which we could connect the movement of an infected man, or the movement of infected food-stuffs, with an outbreak elsewhere, with regard to the initial outbreaks of invasion it has been very difficult to obtain such evidence. In fact it has not been possible, in most cases, to correlate outbreaks of invasion with the visits of human beings from abroad, or with the use of imported food-stuffs, except in a very few cases in which it has appeared that imported meat or vegetables might have been the vehicle of infection.

When face to face with this absence of evidence, after studying a very large number of outbreaks, I thought it advisable to review all the papers that were in my office,—papers in regard to the various outbreaks over a period of thirty years. I possessed this advantage at least, that for nearly twenty of those years I had had a personal knowledge of almost every initial outbreak which had taken place. The first step was to separate the initial outbreaks from the outbreaks of ramification, and leave the latter out of consideration in this question. I defined, for working purposes, an initial outbreak of invasion as one which appeared in Great Britain after a long period of freedom, or one which appeared in Great Britain, though not after a long period of freedom, at a long distance, say 100 miles or so from the former outbreak, with no probable connexion between the two. Thirdly, I took in, but only in a very few cases, and only in cases of which I had intimate personal knowledge, outbreaks only two or three miles apart which had occurred on the same day, with no connexion between each other. I may have taken in one or two in excess of what I should have done, or one or two below the right number, but on reviewing them again I do not think I can have gone far wrong in the number. And what I found was that for a period of thirty years we have had only about eighty outbreaks of invasion; all the other thousands of outbreaks recorded were outbreaks of ramification round about the initial invasion. You will agree that eighty outbreaks of invasion over a period of thirty years is not a large number for purposes of investigating factors of frequency, for instance of any particular food being connected with the outbreaks, or foods from any particular country. And it seems to show that whatever agency or agencies are operating, they only act fortuitously; only act more or less by chance, as it were.

In mapping out the localities of these outbreaks of invasion, I found something which rather led me to believe that our continuous failures, in most cases, to obtain definite evidence against human beings and against food-stuffs, which we know can convey the virus mechanically, might receive an important explanation.

About eighty initial invasions are marked out in the map now shown. At the time this map was made there had been in Scotland, during thirty years, only one initial outbreak; that occurred at Edinburgh—near a port, you will notice. The significance of the port is not definite, but it is a place at which foreign stuff is imported, and it is open to inroads from birds. Since that date, however, we have had another initial outbreak in Scotland, at Paisley—again near a port. Apart from these, Scotland has been free from initial outbreaks, and Ireland has been as free. England is more severely hit. If you draw a line from the Wash to the Severn, you will find that over 50 per cent. of the initial outbreaks have occurred in the area south-east of that line, and by far the larger proportion have occurred in Kent and Sussex. Following a line right up the East Coast, the sites of initial outbreaks are seen to be all dotted within a short distance of the coast and about the river valleys. With regard to the West Coast, there is a particular area in which there have been two or three outbreaks of invasion in different years. When we come to Somerset, Wilts, Devon and Dorset, there is another crop, second only in importance to that on the South-east coast.

The first thing which occurred to me on looking at that map was, that if invasion were due to infected men or food-stuffs, it was difficult to explain how all the infected men and food-stuffs from abroad obtained access only to these particular areas, especially when we know that Scotland is a feeding country and takes a very large quantity of these imported materials. I come to no final conclusion on that point, I merely put it forward as one which has to be investigated.

But if we conclude that this deprives men and food-stuffs from abroad of their apparently great importance, we have to fall back on something else, and ask what other thing or animal can form a link between clean Great Britain and the outside world where the disease prevails. Naturally birds only remain to be considered. I have copied this map now shown from Clarke's "Migration of Birds." Scotland

and the North of England get a large invasion of birds; they come from Norway and Northern Europe. And, curiously, Norway has always been free from foot-and-mouth disease. The Low Countries, including Holland, appear to be one of the great crossing-over places. The majority of the birds strike Kent and Sussex, and stream along the north and south coasts of England in the direction of one or other patches of frequency. And there is a crossing line in the middle of England, from the Wash to the Severn, followed by the migrating birds entering England. There is also a crossing point in the North, which comes near to those other centres of initial outbreaks. Therefore, one cannot leave out of account the question as to whether bird movement and migration has not some connexion with the problem?

When I noticed what the map showed, I got out what you see in this table: the total number of initial outbreaks in every month of the year, and the number of years in which outbreaks had occurred in these months. You will see that the months in which there were the greatest number of outbreaks also show the most frequent recurrences over years. If you take it merely from the point of view of the ordinary migration of birds, there might be some features difficult to explain; the very high number of outbreaks in December, when the ordinary migration is not going on, also in November, and in January. I sought the advice of persons (naturalists) with a skilled knowledge of bird life, and they pointed out that in these winter months there is an enormous interchange of birds between the Continent and England. The only difficulty I have in squaring the data of this table with any theory or hypothesis in connexion with birds is the month of July: nine initial outbreaks in thirty years, and occurring five times. In July there is not, so far as I know, a great deal of bird movement; but I am told that the information about bird movement is not yet complete, and that it is possible that in July there are these movements, not amounting to mass migration between this country and the Continent, but still of importance. But in regard to food-stuffs, I want to express the opinion that, while in a free trade country like this, which cannot provide itself with sufficient material, it is difficult to know what to do to render the thousands of tons of imported food-stuffs harmless; if there is a true bill against food-stuffs, it is not necessarily an insoluble problem. If, however, the bird theory is correct, I do not see any solution.

We may now ask, how does our knowledge of the virus, such as it is, fit in with what I have said about conveyance by men and food-stuffs? I will expand the bird theory later, in discussing susceptible species. As regards food-stuffs, we are informed that the virus is, as a rule, short-lived outside the bodies of susceptible animals, but that there are certain conditions which favour its survival for a period of about three months. We know the virus is filterable, and that it is ultra-microscopic, therefore we cannot expect much help from the microscope in sorting out uncontaminated food-stuffs from those contaminated. Some other method must be found. We know cold preserves the life of the virus in the dark for months, and that anything approaching the incubation temperature seems to set working the biological processes which destroy it; in such a temperature therefore it may become harmless in a few hours. But these remarks must be stated with important reservations, because we know that certain media are enormously more favourable than others for its preservation in heat or in cold. I and all my inspectors have a very strong conviction that where there are recurrences, the infection had remained in food-stuff, and it is almost always hay or straw that is under most suspicion. It seems as if these substances are especially favourable for prolonging the life of the virus. Desiccation may either preserve or destroy virus. That may seem to be rather a contradiction; but what it means is, that desiccation in sunlight is very fatal. Many animals may be affected in a field in bright, sunny weather. You kill the infected animals and so remove them. In a few days the field has disinfected itself without anything having been done to it. I do not say we should make a practice

of putting animals in a field a few days after infected animals have been removed, but in the old days farmers did not hesitate to do so in fourteen days. If, however, you dry the virus rather rapidly by gentle heat artificially applied, it may remain active for several months. That suggests a reason why it may remain alive in foodstuffs stored in a dark loft. The virus can be active after a dilution of 100,000 times or more, and that, I suggest, is one of the explanations of the extraordinary capacity for spread, once it is manufactured.

These are all problems which the Research Committee will have to attack, for what we want to discover are the worst and the best conditions for survival; and when we have found the best conditions for shortening its life we may be able to apply the means in practice.

I can hardly leave the question of the virus without drawing attention to a recent observation by Vallée, because it is of great importance in questions of epizooticology, also in regard to questions of immunity and immunization. Vallée found there were two strains of virus, "A" and "O," and he said so for the following reasons. If he inoculated a bovine animal with "A" it contracted the disease, and, after its recovery, if he inoculated it again with "A" it did not have another attack. But, if he shortly afterwards inoculated that animal with virus "O," it went down with foot-and-mouth disease at once. The same happened if "O" was given twice and then followed by "A." I have seen M. Vallée lately and I have examined his records, and I cannot doubt that his conclusions are correct. This, therefore, enormously complicates the question of immunity, and the preparation of the agent which may be expected to give immunity.

Next a word about susceptibility of species. I am particularly anxious, seeing that the meeting is a combination of veterinarians and medical men, to get some information about the susceptibility of mankind and the clinical symptoms shown by human beings. My only experience of it is in signing up three of our own inspectors who claimed compensation for an attack of the disease said to be contracted on duty; I do not know whether they had it, I have never seen any published medical articles detailing what were the symptoms in these men, and I do not know on what the diagnosis rested in their cases, except that they had been in intimate contact with diseased animals. I should have thought that their medical attendants after certifying would have written up the cases for a medical journal. But the subject raises this question: hundreds of men, farm hands and others, who have no strict idea of personal hygiene, and who are brought into the most intimate personal relation with the virus, never seem to suffer from the disease, at least not in a visible or obvious form, and the possibility of insignificant symptoms must not altogether be flouted. If it be true that man may suffer from it in an insignificant form, it will add to the epizooticological knowledge and may explain how the disease can be carried a long way.

Cattle, sheep and pigs are in the first line of susceptibility; goats are rather less susceptible, but there is not much difference between them and other ruminants in this respect. Comparative susceptibility must be spoken of with some reserve, whilst it is true that if you find the disease in cattle, and sheep are intimately associated with them, it takes a longer time for the disease to spread from cattle to sheep than from cow to cow. Once it gets a footing in the sheep, however, it spreads as rapidly in them as it does in other animals. I have never noticed any such difference in the rapidity of spread from cattle to pigs or from pigs to cattle.

There are still left for consideration the horse, the dog, the cat, and rodents. There are a few cases recorded of these species of animals being naturally infected; there are also records of experimentally-produced cases, and I think we have to acknowledge that those species are at least susceptible, though the disease does not spread among them as it does among ruminants and among other animals on a farm. There are also on record instances of foot-and-mouth disease in fowls. I have cross-

examined elderly practitioners who have recited their experiences of this, and I have never, either from their statements, or from literature or otherwise, been able to exclude epithelioma contagiosum. I have seen many thousands of outbreaks, in most of which fowls were running about closely in contact with the diseased animals, and I have not seen anything which shows that the fowl can naturally suffer from foot-and-mouth disease. That, however, does not mean that no bird can suffer from the disease; there are different kinds of fowls amounting almost to different species. Even if the bird does not suffer from foot-and-mouth disease we know that its feathering can be infected just the same as a man's coat, and I do not hesitate to say that a bird with infected feathers dropping one or two feathers on to the farm or into the water supply is a distinct source of infection.

Lastly we come to the rodent, which is very interesting. All the other animals I have mentioned have been accused of being susceptible to foot-and-mouth disease on account of actual observations in practice; the rodent has not been so accused. It has been employed on account of susceptibility in the experimental way and for purposes other than epizooticological. I do not think anyone has, in practice, picked up a rodent suffering from foot-and-mouth disease; but about twenty years ago Hecker showed that guinea-pigs could be inoculated with it. That fact was forgotten for a time; it was brought to light again, and it is now being made great use of in experiments to keep up the virus in attempts to cultivate it. It is not the guinea-pig so much as the rat which may be of practical interest. Professor Beattie and others have infected the rat with the disease, but we may take it that, like other rodents, it is not very susceptible. The rat as a possible agent of infection is no new thing, and has not been neglected in the practical sense. For many years epizooticologists have been trying to get evidence against the rat, and our inspectors, with the assistance of farmers, have pursued a rat inquiry; we have trapped them and poisoned them, and we have examined many hundreds dead and alive in infected places, but we have never been able to find a lesion which would lead us to give any definite opinion that the rats were suffering, or had suffered, from foot-and-mouth-disease, and no farmer we have questioned has seen rats going about lame. If they had been found to be suffering from foot-and-mouth disease it would be a most serious question for the epizooticologist.

What, however, are the facts? Our inspectors go to an outbreak, they find rats on the place, they kill the animals, they disinfect the place, and in due time they re-stock it, and the rats are still there in large numbers. No harm has come to the rats, and there yet is no recurrence of the disease in the great majority (thousands) of cases. Foot-and-mouth disease can be stamped out, we must accept that it has been stamped out over and over again. It comes in again, but you can re-eject it, and that would, I think, be impossible if the rats took the disease and passed it on to each other; this would keep it up practically for ever. I heard one of the men who is responsible for spreading this rat theory declare that when the premises round about his home were being disinfected he saw stray rats coming up his avenue which he was able to recognize as strangers, and that following their entrance there was foot-and-mouth disease on his place. None of our inspectors have seen rats leaving a place in droves during disinfection, as rats are supposed to leave a sinking ship; I think that is romance, and that the picture of rats leaving an infected place in droves is a figment of the imagination. Still, it is a problem which is suitable for research, and I merely put before you the information we have obtained, because I hold that the actual facts in the field of practice cannot legitimately be flouted by reason of such laboratory experience as we possess.

I will briefly discuss the problems of immunity. In this disease, usually, the immunity is not a long one. Sometimes it is very short, but after the discovery of Vallée about the two viruses, we have to review the position in regard to very short immunity. The common idea among clinicians is that the average immunity is six

months, but it may be a year or more. Lately, on account of the very unsatisfactory position of affairs on the Continent, where they feel that the disease is so widespread that they can do next to nothing towards eradication, much attention has been concentrated on methods of immunization. Examination of these methods shows there is nothing new in them; they simply amount to attempts to apply the usual methods of giving immunity, of which there are several, to foot-and-mouth disease. No one has arrived at a fixed, attenuated virus. You can get one virus more attenuated than another, but there is no fixed virus of attenuation. A very common practice on the Continent is one which was adopted by farmers and recommended by many veterinary surgeons; that when a man gets the disease on his farm, he should take a wisp of hay and rub it over the lesions of the infected animal, and then go round the rest of his stock and infect them by rubbing the wisp of hay inside the mouth, with the idea of getting the disease over as quickly as possible.

Loeffler showed that in the febrile stages of the disease the blood was virulent. Acting on that knowledge Cossco and Aguzzi have strongly advised the inoculation of the animals with this virulent blood. They claim that, given in this way, the disease is always milder than in a natural attack. I do not think that is quite correct, for some of the animals so treated have had very severe attacks. The method of sensitizing the virus with hyper-immune serum was also tried, but nothing was found in that. They have also tried, with some success, the rinderpest method of inoculation, giving a dose of serum together with a dose of virus. To all those methods there is this very great objection: that you may not get immunity at all; or, as often happens, you may find the animal attacked by the disease from which it was supposed to be immune, or you may cause an actual outbreak by the inoculation. To adopt these methods in a country like this, which is clean, would be equal to spreading the disease all over the land. Once inoculation were begun here, it would have to be continued; the circle would become ever wider, and you might as well let an outbreak take its course, as it were. In this country I do not think any of these methods of inoculation are likely to be of much use. Another practical objection to the serum combination method is that an enormous dose—200 to 300 c.c.—of serum has to be given to each animal. Outbreaks involving many thousands of animals would mean the production of serum on a colossal scale, and it would have to be continued (keeping up infected stations) in times of peace, as it were, because of the risk of the disease appearing from outside. I admit that could be got over if we arrived at a method of cultivating the virus artificially.

There remains, however, the question of getting an immunizing agent which will not cause any infection at all—a serum for example. In this country it would be of the greatest value if we could obtain something which would confer immunity without infectivity for fourteen days to twenty-one days. I say that because there is no doubt that the stamping-out method is effective; if we get early reports we can stamp it out at a small cost. But having regard to what goes on in the country—huge markets with two or three thousand animals—if the disease gets into a market before the authorities can surround it, there are the elements favouring a widespread outbreak over the whole country. The great expense of the stamping-out method is not in killing the affected animals, but in killing the enormous number of animals which are in contact on the farms, which will otherwise become agents for the manufacture of virus to spread the disease all over the country. At present these animals have to be killed; there is no way out of it. But if we could have an immunizing agent which would give contacts protection for three weeks, during which the place could be disinfected, I think it would save nine-tenths of our present expenditure, and I believe that the stamping-out method, under these circumstances, would be a negligible expense. It is common knowledge that one of the objects of the Foot-and-Mouth Research Committee is to cultivate the virus. We have no

guarantee, however, that if we could cultivate it we should be able to make a potent serum with it, but it is a distinct possibility.

These are only some of the problems—I have not attempted to cover all of them—arising for solution in connexion with this disease, which is one of such enormous economic importance.

Discussion.—Dr. JOSEPH A. ARKWRIGHT (Lister Institute) said that to those who had followed the Continental literature on the scientific research into foot-and-mouth disease during the last thirty years, the practical results, especially as regarded the problem in this country, must appear at first sight disappointing. Until the last three or four years knowledge had but little advanced since 1900. In reality, however, two very important additions to the information on the subject had been made as the result of the first attack by Loeffler and Frosch in 1897.

(1) The first was the discovery that the virus of foot-and-mouth disease was no ordinary bacterium or protozoon, but was a truly filterable virus. This had been amply confirmed, proving that the virus was capable of passing easily through a Berkefeld or porcelain filter, which held back small bacteria, and that it was not merely filterable in the restricted sense of sometimes making its way through a not very efficient filter, like the virus of typhus fever, or vaccinia, or trench fever.

(2) The other important discovery was that animals which had had the disease were immune, at any rate for a time, and that this immunity was at least in part due to the changes in their blood, so that a passive immunity could be communicated to other animals by injections of serum.

The practical application of these two facts had been hampered, and perhaps its due development prevented, mainly by the extreme expense and the cumbersomeness of experiments sufficient to test the activity and strength of a given virus or the protective power of a prophylactic or curative serum. In spite of the large sums of money, and the labour and skill which Loeffler and his colleague had expended on developing a system of immunizing inoculation, disaster had followed some of his efforts, no doubt largely because of the difficulty, or rather impossibility, of making sufficient experiments on swine and cattle to test his materials satisfactorily. A very expensive serum had, however, been made and used in special cases.

Almost independently of the results obtained from laboratory researches, the French and German workers had made many modifications in the old system of inoculation or "aphthization," with or without the additional treatment with blood or serum of recovered animals. How far these trials had proved useful was no doubt a matter of opinion, but apparently they had only had a limited applicability and in their present form were quite unsuitable for use in this country.

Again, attempts to simplify and lessen the cost of production of a vaccine or serum by obtaining cultures of the virus had been held in suspense owing to the need for pigs and cattle on which to test such cultures and the consequent cost of such experiments. Large animals, moreover, had not been found uniformly susceptible when used experimentally, with the result that either the issue was confused or a much larger number of experiments was required.

The whole subject had, however, now been raised to a higher plane of research by the discovery of Waldmann and Pape, in 1921, that guinea-pigs could be successfully inoculated and develop the disease regularly, so that they were extremely suitable for experimental purposes. An added quality which they possessed was their inability to communicate the disease to each other in the animal house, except in rare instances. Their use was therefore not attended by this error of accidental transmission which had sometimes crept into experiments on large animals.

Now that the Ministry of Agriculture in this country had taken the enlightened step of arranging for systematic research, all the many questions at issue would be, and indeed were

being, again submitted to detailed examination with the great assistance afforded by the guinea-pig technique in testing and standardizing materials. Though past experience suggested that the task was a tedious one and that no forecast could be made as to the ultimate success, nevertheless there were new means at the disposal of the workers, and the problem in this country was viewed differently from the way in which it was regarded by Continental workers. This made it hopeful that help would eventually be afforded to the agricultural community. Each doubtful point was a separate problem and the subject must be attacked in detail before any useful results could be attained. The individual points for investigation could not now be enumerated, but they comprised exact knowledge as to the stage and duration of infectivity of animals, the various means of transmission, the efficacy of living and dead intermediaries, practicable and efficient means of immunization without incurring the risk of spreading the disease, and the study of the properties of the virus with a view to culture. All these were intricate and difficult problems but they could now be attacked and their solution would be materially assisted by laboratory experiments on small animals.

Professor G. H. WOOLDRIDGE said an important point was the degree of susceptibility of species other than those most commonly affected, and he was much interested in what Sir Stewart Stockman had said about the goat in this respect. He was often told by goat-breeders that their particular fancy was not susceptible to the disease, and it was difficult to persuade them otherwise.

With regard to the horse, in view of statistics of previous outbreaks he had wondered whether sufficient importance was attached to the horse as a possible source of introduction. In the statistics of the early seventies there were detailed the species which had been affected with the disease and slaughtered, both because they were infected and exposed to infection. One hundred to two hundred horses said to have been infected were slaughtered. Was it possible that any of the recent primary outbreaks were due to horses imported from affected areas?

The author said he had not come across detailed accounts of the disease in the human subject, and he (Professor Wooldridge) wondered whether Sir Stewart Stockman was acquainted with a report of a veterinary inspector in the Department of Agriculture and Technical Instruction for Ireland having become infected in 1912. The inspector was Mr. George Bell, M.R.C.V.S., whose case was minutely reported upon by Dr. C. M. O'Brien, Sir Charles Cameron and Sir Thomas Myles. It was said to have occurred as the result of a wound in the finger when he was examining the mouths of some cattle. The first lesions were around the finger nails, and they extended to the wrist. Then similar lesions occurred on the foot and extended to the ankle, and a few days later his tongue and lips began to swell and vesicles developed. He had difficulty in articulation and deglutition, and there was copious salivation and serious systemic disturbance.

Mr. F. C. MINETT said that with regard to the question of human infection referred to by Professor Wooldridge, perusal of the literature made one think that human beings did become infected occasionally, but nevertheless there was an element of uncertainty which was increased when one thought of the prevalence in man of such diseases as labial herpes. Being one of the workers for the Research Committee, he had abstracted a good deal of the literature on the subject and was able to produce the following information. A few experimental observations had been made since the war. Thus, Pancera stated that he had succeeded in infecting a guinea-pig and a calf from cases in children. The French worker, Lebailly, in one case failed to infect both a man and a monkey with bovine virus, while in another he was able to infect an ox with human material. But unfortunately this ox became infected shortly after an immunity test. He believed, from what he had been told by veterinary inspectors, that cases had occurred in man from time to time, but he would like to see some good experimental evidence adduced by inoculation of human material into oxen.

With regard to certain outbreaks in this country there was one important factor which, he thought, had only been fully realized within the last year or two. He referred to those premises on which there had been more than one outbreak, separated at times by an interval as long as twelve months. While there was no absolute evidence, there was strong circumstantial evidence connecting the first and second outbreaks on such premises. In Switzerland a similar state of affairs had been noticed. There, in certain parts, it was the practice to graze cattle during the summer on pastures at high altitudes, where possibly they would become affected with foot-and-mouth disease, and to bring them down to the valleys during the winter. When the same, or fresh animals were sent up again the following spring not infrequently there would be fresh outbreaks. Such occurrences could be explained by a knowledge of the resistance of the virus to agencies such as desiccation and cold among other things. Not only was the virus resistant to cold, but it could actually withstand freezing and thawing repeated many times over several months. This was a very severe test, and it was this which he now wished to emphasize.

Dr. S. P. BEDSON mentioned the advantages which the availability of small animals, like the guinea-pig, conferred on the experimental investigation of foot-and-mouth disease. Working at the Lister Institute, he and his colleagues had been struck by the regularity with which the guinea-pig reacted to the inoculation of foot-and-mouth virus. Moreover, the clinical picture of the disease in this animal was quite typical, leaving one in no doubt, in the very large majority of cases, as to what one was dealing with. Further, the susceptibility of the guinea-pig had made it possible to estimate the strength of each sample of virus as a routine procedure; working with large animals this had been impossible on the count of expense. He pointed out the great value of this experimentation in their work. The strength of each sample of virus worked with was known, thus making experiments with different samples of virus comparable. In attempting to arrive at some conclusion as to the resistance of the virus to different agents—chemical or physical—it was of paramount importance to know the strength of the virus in each experiment, otherwise erroneous conclusions might be drawn. As a further example of the use of titration, he cited experiments dealing with the viability of the virus in different media at 37° C. These experiments would have as their object the elucidation of the problem of cultivation. Here it was of importance to know, not only how long the virus could survive, but the rate at which it died. Should it be found that the amount of virus fell steadily from the inception of the experiment, then, presumably, no multiplication had taken place, and the length of time the material remained infective would depend on the initial concentration of the virus and the rate at which it was killed off. On the other hand, if it were found that the amount of virus remained more or less constant for some days and then fell rapidly, it might be concluded that the conditions had permitted multiplication of the virus, and an indication as to the right time for subcultivation would be obtained.

Mr. A. L. SHEATHER said that in India he had noted cases in which the period of immunity conferred by a natural attack was very short. It occasionally happened that an animal would become infected for a second time after a lapse of some six weeks only. This had some bearing upon what Sir Stewart Stockman had said about the plurality of the virus. It was difficult to believe that more than one virus was concerned in the cases mentioned. Outbreaks of the disease had occurred among newly-purchased cattle at Muktesar during the cold weather only. Cattle in India appeared to be less readily infected than the animals in this country. On a few occasions cases of foot-and-mouth disease had occurred among the serum-producers. The steps taken had been sufficient to prevent the spread of the disease to animals in other sheds only forty or fifty yards away.

Major-General Sir JOHN MOORE said that Sir Stewart Stockman had given a very instructive and lucid address on this extraordinary disease, the causation of which seemed to baffle solution. In common with other army veterinary officers he had had considerable

experience of it in India. Of the two contagious diseases met with amongst transport bullocks on active service in that country, viz., rinderpest, and foot-and-mouth disease, he would almost prefer the former—you knew where you were with the one and not with the other. His experience was that an attack of foot-and-mouth disease conveyed an immunity for five months, and viability of the organism causing the disease was sixteen days in the open air.

He had had experience of it also on the Continent during the war. It prevailed in the areas occupied by our troops in France and Belgium, so much so that a special routine order was published regarding it as early as August, 1915, and continued in force until the conclusion of hostilities. Farms on which the disease occurred were vacated, men who had been in contact had their clothing and accoutrements disinfected. If there was any doubt, or if disinfection was impossible before proceeding on leave to England, a new issue of clothing was made. He read a statement of outbreaks of foot-and-mouth disease in Great Britain and Ireland during the years of the war showing how small a number these were, and the large increase after demobilization, in 1919, and after return of troops. He (Sir John Moore) said that surely this tended to disprove the theory of bird agency, for it must be presumed that birds migrated during the war the same as afterwards. It rather pointed, in his opinion, to packing materials as an agency on the resumption of trade.

He commented on the prevalence of the disease chiefly in the cold and wet season of the year, and he said that all aphthous diseases were more rife at that season than at any other. He mentioned as an instance the stomatitis contagiosa which prevailed so severely amongst the horses in the Expeditionary Force in France in the winter of 1917-18, when this disease spread with terrific rapidity, the cases running into thousands. With the advent of the fine weather of spring it disappeared as mysteriously as it came. He considered aphthous diseases essentially winter and cold weather diseases.

Dr. M. J. ROWLANDS said he had seen four cases of what he considered to be the disease in human beings. The first was that of a young woman who had what would be regarded as aphthous stomatitis, followed by the development of vesicles on the fingers. The father, who nursed her, also became infected in five days, and two other children in the house suffered from the complaint, in each instance after five days' incubation. The doctor who called him in to see the first of the cases did not suspect the nature of the condition. He (the speaker) regarded it as a "true bill" in regard to the human species. The temperature in those cases rose to 102° F.; there was difficulty in moving the tongue, blebs appeared, and they then burst. The superficial ulceration soon healed, and the patients became quite well in seven days. The source of the milk that the family had been consuming could not be traced.

With regard to the bird theory of spread, if one looked at the map Sir Stewart had shown it was significant that most of the initial attacks occurred near sea-ports. He (Dr. Rowlands) knew, to his personal regret, that the outbreak always occurred in Kent at Christmas time, as he was often debarred by it from exhibiting his animals at shows.

Lastly, the large number of hides which came to London from the Argentine suggested the possibility of hides being a form of vehicle of the disease. The question of their sterilization, therefore, demanded close attention.

Mr. R. DAUBNEY said he was glad to hear Sir Stewart Stockman's reference to the probable value of an immune serum in combating the disease in this country. The fact should not be lost sight of that there were two possible avenues of research, each of which might lead to the attainment of very valuable results. On the one hand there were the laboratory efforts at cultivation of the virus, and on the other experiments with large animals to devise a means of protection, even though all attempts to cultivate the virus artificially had failed. There were already in wide use in veterinary practice two prophylactic agents in the treatment and prevention of two filterable-virus diseases, namely,

the anti-rinderpest and the anti-hog cholera sera. Both of these sera had proved successful, particularly the former. They were commonly used for inoculation simultaneously with the injection of virus. In the case of foot-and-mouth disease a serum was already being manufactured by the Prussian Government. The animals immunized were oxen, and the virus for hyper-immunization was obtained by scarification and inoculation of the muzzle of pigs and the extraction of lymph from the unburst lesions. Six or seven inoculations were usually sufficient to produce a serum of standard potency. Here, again, the guinea-pig proved its usefulness. Samples of sera were tested in standard doses for their power to protect guinea-pigs against inoculation of virus. The serum was required to protect guinea-pigs in standard doses from generalization of the disease. When this standard had been reached the oxen were bled out and their carcases marketed after retention for a short period. So far as his inquiries had gone the serum appeared to cost about £2 a litre to produce. If produced on a large scale it was possible that these figures could be subjected to further reduction. The Prussian authorities claimed that in 1923 the animals handed over to France and Belgium under the Reparations Scheme were successfully protected by this serum before passing through the infected territories on "trek" to their destination. They claimed that no outbreak of foot-and-mouth disease occurred among these animals during transit.

Referring to Sir John Moore's remarks on the comparative infrequency of foot-and-mouth disease in this country during the war years and its heavy incidence since 1919, he considered that the disease was not widespread in Europe during the war years, but it appeared that an epizootic had arisen in Northern Italy in 1918, and had spread to the north and west during the autumn of that year, heavily invading the whole of Germany and Holland and contributing largely to the complete breakdown of the Swiss control which had proved so successful during the years of mobilization. It was one of those intense waves of spread which had been known to occur previously on the continent of Europe, and it seemed likely that the number of appearances of the disease in this country was related to its continued prevalence in Western Europe.

Mr. HENRY GRAY asked whether Sir Stewart Stockman considered that the great incidence of the disease in July was due to the great migration of starlings from one country to another. He asked whether experiments had been carried out to ascertain whether the fæces of birds which had eaten worms and other material from infected cattle had been examined in order to see whether such fæces were infected. And were not the birds' feet more likely to carry the infection than the feathers?

Mr. F. T. G. HOBDAY (President) asked whether Sir Stewart Stockman had had experience, on the Continent, or where he had been, of the virulent form of the disease. During the war, he (the speaker) saw, in conjunction with some distinguished Italian scientists who were investigating it, an outbreak in the north of Italy in which hundreds of cattle died. They said this form visited Italy about every twenty years. The cattle in that outbreak died on about the ninth day of the illness. The animals gave a peculiar cry, and then dropped dead. All kinds of sera and preventive measures had been tried against it without avail.

SIR STEWART STOCKMAN (in reply), referring to Professor Wooldridge's remarks, said that with regard to the horses killed in the seventies a different explanation from that offered was forthcoming. He had heard farmers demand that all the poultry should be killed, and the dogs shot. But it was known that in the horse there was stomatitis, also horse-pox, and one could understand farmers being unable to differentiate between a variolous eruption and a foot-and-mouth eruption. He did not think the importation of horses in the seventies had anything to do with the prevalence of the disease. In the seventies there were perhaps only one or two initial outbreaks which set alight the agricultural country. In the 1922 outbreak, in which there were 1,100 separate outbreaks of ramification, it was certain that there was only

one introduction, viz., through a large market. In 1923, there was evidence of about twenty different invasions.

He had not intended to deny that foot-and-mouth disease occurred in human beings; it was vouched for by serious medical men in all parts of the world. There had not been so much attention devoted to this human aspect by medical men as veterinarians would like to see. If man could be infected with the disease, it followed that man might be a disseminator of it, and that fact would account for some of the most mysterious occurrences of spread of the disease there had been in this country. He did not know whether the diagnosis in some of the supposed human cases was valid.[1]

[1] Owing to the lateness of the hour the lecturer (Sir Stewart Stockman) asked leave to break off the discussion and offered his apologies to those who had been good enough to take part in it.

Section of Comparative Medicine.

President—Mr. Frederick T. G. Hobday, C.M.G., F.R.C.V.S.

Skin Affections Transmissible from Animals to Man.

By J. H. Sequeira, M.D., F.R.C.P., F.R.C.S.

Dermatologists are frequently confronted with conditions which necessarily suggest the possibility of infection from the domestic animals which come into such close contact with all classes of the community. As our experience increases we learn more and more the value of scientific investigation and perceive that the wider the sources of our information the more light is thrown upon the baffling problems we meet with in our endeavour to prevent and cure disease. It is not long since certain forms of skin disease in the human subject which were classed as "eczema" have been shown to be due to parasitic vegetable organisms, and we are now able to deal with these on rational lines. Similarly, we are all familiar with cases which have been called "eczema" and which were probably ascribed to gout or some other vague source, that have proved on investigation by the microscope to be caused by the parasite of scabies. I was particularly interested to note in the rather cursory survey which I have been able to make of veterinary literature that conditions in domestic animals which have been grouped as "eczema" have been shown by the microscope to be caused by animal parasites of the same type.

Our problems, therefore, in many respects are similar, and my endeavour in this short survey of the affections of the skin which are transmissible from animals to man, will, I trust, be elaborated in the discussion which I understand is to follow this communication.

The cutaneous affections which are communicated from animals to man may be conveniently divided into groups :—

(1) Diseases caused by animal parasites of domestic and other animals which directly infect the human skin. The commonest of these parasites are the varieties of animal sarcoptes which occasionally are communicated to man.

(2) Diseases caused by the moulds known as ringworm fungi, communicated either directly or indirectly to the human skin. This group is of considerable importance, as many of the domestic animals suffer from various forms of tinea.

(3) Diseases due to the inoculation of the human skin with recognizable micro-organisms derived from diseased animals. The micro-organisms may be introduced directly through an abrasion, as in tuberculosis, anthrax and glanders, or may be conveyed from one host to the other by blood-sucking insects, particularly in tropical and subtropical countries. The organisms may be directly inoculated by the blood-

sucking parasite biting the human host or may be deposited on the skin in the insect's fæces. In some of these conditions the cutaneous affection is a symptom of a general infection. It is impossible to-day to do more than mention this attractive subject which I hope may be the subject of a discussion at another meeting.

(4) Diseases due to the introduction of a virus, probably a micro-organism which has hitherto escaped recognition, as in cow-pox, foot-and-mouth disease, and, possibly, molluscum contagiosum.

(I) Diseases Caused by Animal Parasites.

The most important animal parasites affecting the skin belong to the order *Acarina*, and the groups affecting domestic animals are:—

(1) *Ixodidæ*, the ticks, which cause trouble directly or through the diseases which they transmit. Ticks are the disseminators of a number of grave tropical diseases.

(2) *Gamasidæ*, the red mites, which chiefly affect rats, rabbits and fowls, and may cause temporary irritation in man.

(3) *Thrombidiidæ*, the harvest bugs.

(4) *Sarcoptidæ*, the mites of mange and scab in animals, and scabies or itch in man.

(5) *Demodecidæ*, the follicular mites, which cause follicular mange in animals and may occasionally cause disease in the human subject.

For the purpose of this discussion I propose to direct attention chiefly to the affections produced by the mange mites.

Mange, caused by (*a*) *Sarcoptidæ*, (*b*) *Demodecidæ*.

(*a*) *Sarcoptidæ* [1].—There are several groups of acarine parasites which cause different varieties of mange in the domestic animals. They are (1) *Sarcoptes*, (2) *Notædres* vel *Sarcoptes minor*, (3) *Psoroptes*, (4) *Chorioptes*, and (5) *Otodectes*.

Sarcoptes scabiei is a minute parasite the female of which is just visible to the naked eye. A characteristic feature is that the impregnated female burrows into the epidermis and lays her eggs in a little tunnel and the eggs are hatched there. There are several varieties, each peculiar to a particular species of animal, but in their main anatomical characters they closely resemble each other and the *Sarcoptes scabiei* of man. Although they are usually confined to a particular animal the sarcoptes of one species may be transmissible to another species, e.g., horse sarcoptes to cattle, &c., and in this country it is not rare to find the *Sarcoptes equi* and the *Sarcoptes canis* infecting the human subject. Abroad, the parasites of the goat and camel not infrequently attack man. I am not aware that the sarcoptes of the rabbit and the ferret have been found in the human subject.

The Sarcoptes minor or *Notædres cati*, a smaller parasite which is now placed in a separate group, is the cause of a type of mange in cats and occasionally attacks man. The egg-burrows made by this parasite run vertically to the surface of the skin.

I have found no evidence to suggest that the other parasites of this group, i.e., the *Otodectes* or *Symbiotes auricularum*, causing "canker" of the ear in dogs and cats, nor the *Psoroptes*, which produces sheep-scab, are responsible for more than transitory irritation in man. Scabies in poultry, due to varieties of *Chemidocoptes*, does not appear to cause trouble in human beings.

Practically, therefore, we have to consider infection of man by the *Sarcoptes equi*, the *Sarcoptes canis* and the *Notædres cati*.

The Sarcoptes of the horse, which has been known since 1856 (Delafond and Gerlach), was the cause of much trouble among the men of the mounted units during the war. Just as the medical service of the Army had to make special provision for the treatment of scabies in the men, the veterinary department had to combat widespread sarcoptic mange in the mounted units, transport animals, &c. The men in charge of these infected animals were frequently infected. Some cases of this type of infection came under my observation in the military hospitals, and, occasionally, I have met with the trouble in private practice. Recently I had under my care a child and her governess, who had both contracted scabies from a pony. An interesting epidemic was reported in 1922 by MacDonald [2]. The carcase of a horse which had suffered from generalized sarcoptic mange, was used by a class of veterinary students, who spent from two to four hours on the subject. With the exception of onlookers and those engaged on the head and neck, which had been cured by treatment, all suffered from most pronounced and continuous itching. Forearms, ankles and arms were affected by a pin-head size, fiery red, papular eruption. The hands sometimes escaped, as they were frequently washed with carbolic soap. Some of the students developed secondary lesions on the trunk, thighs, shoulders and upper arms. Rapid cure followed baths and the use of sulphur ointment, with disinfection of the clothes. When the latter precaution was neglected re-infection occurred. The sarcoptes were found on the carcase in abnormally large numbers.

Sarcoptes of the camel frequently infects camel drivers and others who come closely into contact with these animals, and it has frequently caused trouble in camel corps in Eastern campaigns. Dyson reported an epidemic in Palestine in 1919 [2A].

Sarcoptes of the dog, recognized in 1814 by Gohier, is not uncommonly conveyed to human beings. It is not difficult to make the diagnosis if one is aware of the possibility of infection. The eruption itches intensely, the lesions, in the cases I have seen, are minute vesicles, but the characteristic burrows of the *Sarcoptes hominis* are not present.

Though the lesions are most common on the extremities, I had, some years ago, a lady under my care in whom the itching vesicles occurred round one side of the neck. She was in the habit of placing her toy dog on her shoulder so that it rested against the neck. The dog was found by the veterinary surgeon who examined it, to be suffering from sarcoptic mange. I have never seen a case of sarcoptic infection derived from dogs which became widely spread, and all the cases rapidly yielded to treatment by sulphur.

Notædric Mange in the Cat.—The parasite of this affection, the *Notædres cati*, was formerly known as *Sarcoptes minor*. It is smaller than the acarine parasites causing sarcoptic mange in horses, dogs and cattle, and the impregnated female burrows her way vertically into the epidermis, laying there a packet of eggs. Kittens appear to be more severely affected than full-grown cats, and in them the disease may prove fatal. The lesions in man are intensely itching, pin-head size vesicles, and an intelligent patient will often give a history of recurrent outbreaks at intervals of from six to eight days, corresponding with the period of development of the notædres

Personally, I have not been able to demonstrate the parasite in the human lesions. In a recent case under my care the back of a man's hand was affected and he remembered that a pet kitten found to be suffering from mange was in the habit of rubbing its neck on the part where the infection started. In some cases notædric mange in man is difficult to cure, but it usually yields to sulphur.

(*b*) *Demodecidæ.*—The *Demodex folliculorum* was recognized by Simon [3] as a parasite of domestic animals, and was described by Erasmus Wilson as the steatozoon of man [4]. It appears certain that each species of animal has its own particular variety of demodex. In dogs the demodex produces a type of disease which is serious. The lesions produced are follicular, and the demodex, in penetrating the follicle, carries with it the *Staphylococcus pyogenes aureus.* This microbe sets up an obstinate suppurative folliculitis. The disease is not highly contagious like sarcoptic mange, but it runs a chronic course, and the coccal infection may become generalized with high fever and end fatally.

Demodex Impetigo in Man.—Dr. Whitfield [5] has called attention to a form of impetigo which, while not differing greatly from the common form met with in man, has an entirely different ætiology. The lesions are ringed and resemble a circinate impetigo. Dr. Whitfield says that the edge of the lesion is formed of the remains of the bulla, and if a piece of this roof is placed under the microscope it is found that there are numerous examples of what is apparently the ordinary *Demodex folliculorum.* He points out that "follicular mange" in dogs is caused by a parasite of identical characters, and that these cases may have been instances of the disease, though in none of his cases had the subjects of it been known to handle infected dogs. The affection, like that due to other animal parasites, rapidly yields to sulphur. Though Dr. Whitfield was the first to describe the affection in this country, Dr. Herman Lawrence, of Melbourne, showed cases and specimens in 1915. This association of *Demodex folliculorum* is mentioned in the British Museum treatise on *Demodex.*

That *Demodex impetigo* is rare in man is, I think, agreed, and I must confess that I find it difficult to recognize it. Moreover, it is curious that in the dog the lesions should be of a chronic follicular type, more closely resembling the staphylococcal folliculitis of the beard region in man, while the *Demodex impetigo* of Lawrence and Whitfield appears to be always of the superficial, vesicular or bullous type, closely resembling the circinate impetigos due to streptococci, which rarely involve the follicles.

Flies as Causes of Disease.—Apart from myiasis, which is the name given to diseases in animals and man caused by infestation of the skin and mucous membranes with the larvæ of flies, there is every reason to believe that local infections by pathogenic cocci may be conveyed from animals to man by flies. In most cases this would appear to be a matter of contact and not due to actual inoculation by parasites conveyed by blood-sucking. The conveyance of trypanosomes by the tsetse-fly from animals to man is of serious importance in tropical countries, and can only be mentioned here.

Fleas are believed to convey the *Leishmania* of kala-azar from dogs to man. Here the dusky pigmentation of the skin is merely a symptom of a general infection [6].

Cysticercus.—Hydatids. Hydatids of the skin and subcutaneous tissue are rare, the majority of the cases having been studied in Germany [7]. In 1892 Dr. Pye-Smith described a case in which there were numerous tumours [8].

By the courtesy of my former colleague, Dr. W. J. Hadley, I had the opportunity of examining a man with multiple hydatid cysts of the subcutaneous tissue. The lesions were very numerous and presented characters suggesting multiple fibromata. By puncture of the tumours and by the microscopical examination of the fluid withdrawn, the hooklets of this stage of the echinococcus were demonstrated. There was no history as to the source of infection (unpublished case).

(II) Diseases Caused by Vegetable Parasites.

No one studying the affections produced by the moulds in man and animals can fail to appreciate the labours of Dr. Sabouraud of Paris [9]. His monumental work, "Les Teignes," is a mine of information. We must also acknowledge the valuable contributions to the study of the subject made in this country by Colcott Fox, Adamson and Blaxall, and the researches in tropical ringworm conducted by Castellani.

There are references in the literature at the end of the eighteenth century to contagious skin affections affecting the body, head and shoulders of animals, and in 1820, Ernst, a veterinary surgeon in the canton of Zürich, described a case in which a young girl was infected from a cow [10]. Carrère, in 1838, noted that the virus might remain for a long time in stables and advised burning harness and cleaning any objects which might have been rubbed by the infected animals. Letenneur pointed out that cattle ringworm is most common in the spring after the animals have spent a long time in their winter quarters. His description of the lesions in the calf are remarkably accurate. The neck is most often affected, the lesions are isolated or confluent plaques, the latter often with a festooned outline. The affected lesion is smooth and covered with whitish scales, in the midst of which partially destroyed hairs are visible. He advocated quarantine of the infected animals. He pointed out that those in charge of the cattle are liable to infection, particularly in the thumb, forearm, chin, and sometimes around the mouth, the latter infection occurring especially in children who are in the habit of kissing the young calves committed to their care. He also observed that the scalp is rarely affected.

Similar observations on horse-ringworm were made in Paris by Bazin. The first case recorded was in a remount at the police barracks at Caen, from which seven other horses were infected. The disease spread to the men themselves and one man conveyed it to his wife. Two of these horses were sent to another station, where they started a small epidemic. In this instance Bazin found the fungus infiltrating the hair.

Tilbury Fox [11], in 1871, described seven cases of ringworm in human subjects infected from one pony. Experimental inoculation was practised in many instances, and it was shown that different species of mammals were susceptible to certain forms of tinea and that certain human tineas could be inoculated into animals. Friedberger described infection of a child and of a maidservant from a dog. Fenger, in 1866, appears to have been the first to show that cat ringworm might infect human beings. Michelson described the case of a cat having scabies and ringworm simultaneously and infecting a whole family. Three students who were inoculated developed ringworm but not scabies.

Other animal ringworms have been described, but for practical purposes those of calves, horses, dogs and cats are the most important.

The human subject is most commonly infected from horses and cattle. Many cases of tinea derived from horses and mules were seen in the men attached to the mounted units, transport services, &c., during the war. In country practice calves are frequently the source of infection. Though comparatively rare in urban clinics I have had numerous cases sent up to me from districts near London. Cases of infection from asses, sheep and pigs have also been recognized.

Animal Ringworms.—The fungi of the tinea (including favus) are divided into groups:—

(1) *Microsporon*—small-spored fungi. One variety, *Microsporon Audouinii*, is confined to the human subject and is responsible for 89 per cent. of the scalp ringworms in school children in London. The animal microspora which may attack man are *Microsporon equinum*, derived from the horse, the cat ringworm—

Microsporon felineum, and the dog ringworm—*Microsporon lanosum* vel *caninum*. The lesions resemble those of the human type but are more inflammatory. That of the cat is well illustrated in the slide shown, where the lesions are concentric rings on the scalp of an infant, derived, it was believed, from an infected kitten sleeping in the child's cot. These ringworms attack the smooth skin and also the beard region and scalp hair. (Photographs of cultures of the common microspora on Sabouraud's proof medium were shown.) These cultures develop more rapidly than those of *Microsporon Audouinii*.

(2) *Trichophyta*.—These fall naturally into two groups, (*a*) those in which the spores are large, formerly grouped as *Megalospora*, and (*b*) those in which the spores are small—*Microsporidia*. They are further classed as endothrix and ecto-endothrix—as to whether the interior of the hair shaft is or is not mainly involved. In all these groups the mycelial rods break up by transverse fission into chains of spores (photographs shown). They form numerous types of culture on the proof medium and can be classified by the colour and formation of these cultures.

It would appear that the endothrix ringworms are rarely conveyed from animals to man except the type classed by Sabouraud as *Trichophyton cerebriforme*.

Ecto-endothric trichophytons are all of animal origin, and as a rule cause highly virulent and suppurative lesions in man. They may affect the smooth skin, the hair of the scalp and beard, and the nails. They are the cause of the majority of ringworms in the adult.

The group classed as *Trichophyton gypseum* has chalky-looking cultures and is derived from horses and cattle. It is common on the beard, and the photographs shown are characteristic examples. The group classed as *Trichophyton niveum* chiefly affects dogs and cats, from which human beings may be infected. The *Trichophyton equinum* is not frequently found in man.

An interesting fungus is the *Trichophyton rosaceum*, which is the cause of "white crest" in fowls, and also it occurs in pigeons and canaries. In the human subject it produces raised patches which are scaly or covered with small vesicles and pustules. Similarly, it may affect the beard and scalp.

Some of the trichophytons produce corrugated cultures closely resembling those of favus. These, too, are of animal origin, and Sabouraud recognizes several varieties, one, *Trichophyton violaceum*, giving violet cultures, and another, *Trichophyton ochraceum*, with yellow cultures. These fungi occur in calves and horses and are transmissible to man.

The animal ringworms in man produce:—

(1) Rings, which may be scaly, vesicular or pustular. Sometimes, as in the cat ringworm, there are concentric rings. The degree of inflammation varies, but as a rule it is more intense than in lesions due to purely human parasites.

(2) Patches or plaques which have a rounded or festooned outline. These plaques may be scaly or show vesicles and pustulation. The most severe lesions are kerion of the scalp, characterized by raised, boggy, phlegmonous swellings with numerous, minute perifollicular abscesses. From the centre of these abscesses loose hairs can be easily extracted. In the beard region the suppurative lesions begin with a perifollicular abscess. The lesions may be isolated, but they often form large masses, as shown in the photographs. The scaly type of beard ringworm is usually an endothrix (human) infection. In some cases, however, the fungus is of animal origin (cattle), and may form flat plaques.

(3) Suppurative nail lesions occasionally occur, but the majority of the nail ringworms are caused by *Trichophyton endothrix*, or the epidermophyton which affects the groin and the extremities, fungi which are peculiar to man.

Favus.—The fungi of favus are the achorions, several varieties of which are recognized.

The mouse is the animal most frequently affected by favus, but cats, dogs, rats,

rabbits and hedgehogs have also been found suffering from it. Some authors have suggested that where human beings have been infected by cats and dogs, the disease was originally derived from mice and rats. Sabouraud [12], however, is of opinion that transmission from animals is uncommon. Favus is not so contagious as ringworm but it runs a very chronic course and is difficult to cure in the human subject.

(*a*) The commonest type of favus is produced by the *Achorion Schönleinii*, which is responsible for most of the cases of scalp favus. It may also attack the body and limbs, and occasionally the mucous membranes. The characteristic lesion is a cup-like yellow crust which contains masses of mycelium spores. The hairs are infected and a destructive process leading to pitted scarring is a common result.

(*b*) The *Achorion Quinckeanum* is the cause of favus of the mouse. It occurs also in the hedgehog. That it is communicable from animals to human subjects is well recognized, and Dr. Adamson [13] has published an excellent example of this transmission from a mouse to an infant.

(*c*) The *Achorion gallinæ*, causes "white crest" in birds, and has been found by experiment to be inoculable in man.

(*d*) The *Oospora canina*, which is the fungus found in dog-favus, has not been found spontaneously in the human subject, but inoculation experiments have proved positive.

(*e*) The *Achorion gypseum*, which in man produces lesions closely resembling ringworm and rarely forming cups, has also been found in dogs.

Sabouraud [14] concludes his study of the achorions by stating that animal achorions are so rare in man that they may be looked upon as a scientific curiosity.

(III) Diseases Due to Infection by Micro-organisms.

(1) *Tuberculosis Verrucosa.*—Verruca necrogenica—butchers' tubercle—post-mortem wart, is caused by the direct inoculation of Koch's bacillus. The infection may be derived from another human subject, but it is frequently seen in veterinary surgeons, butchers and others who handle the carcasses of tuberculous animals.

Recently I have had under my care a farmer who had a characteristic infection of the wrist of two years' duration. He informed me that his cattle had frequently had warty swellings on different parts of the skin from which he believed he had been infected. He assured me, and I give his assurance for what it is worth, that he was unaware that any of his beasts had been suffering from tuberculosis.

In man the lesions usually occur on the fingers and backs of the hands. They begin as an oval or lobulated warty swelling. This cicatrizes in the centre and spreads slowly at the margin. The freshly developed lesion has a central, depressed, often pigmented scar, around which is a ring of dark red warty nodules, covered with a crust resembling dried putty, and beyond this again is a zone of erythema, often purplish in colour. The lymphatic glands are infected early. The appearances are well seen in the slide shown. Histologically, tubercles with characteristic giant cells are found in the true skin and there are often miliary abscesses. As will be seen in the section exhibited, the horny layer is enormously thickened. It may be difficult to find the tubercle bacillus, but inoculations of guinea-pigs give a positive result.

(2) *Anthrax* (Malignant Pustule).—The infection usually occurs from the hides and occasionally from the bodies of animals which have died from splenic fever, hence workers in tanneries, wool-sorters and butchers are the most frequent victims. The carrying of infected skins on the shoulders is the reason why in so many cases the face and neck are attacked. In a recent visit to East Africa I saw at one hospital a number of natives who had been infected from diseased goats which had been skinned and eaten for food.

The cause is the *Bacillus anthracis*. The primary lesion occurs usually from one to three days after infection. It is at first onset a papule which soon becomes a vesicle or small bleb, containing at first blood and later pus. The infected area

rapidly necroses and round the slough a ring of tense vesicles develops. There are œdema and infiltration of a considerable area, and the patient complains of intense pain. Auto-inoculation may cause secondary vesicles, but these are rare. The general symptoms are high fever and vomiting. In some cases death occurs in two or three days, but occasionally mild cases are seen. In East Africa I was informed that the native cases usually ran a mild course, and were cured by local antiseptics only. In this country the mortality, which used to be 35 per cent., has been reduced to about 6 per cent. Immediate excision and the use of anti-anthrax serum have effected this great improvement.

(3) *Equinia, Glanders, Farcy.*—This affection, which is rare in the human subject, is due to infection by the *Bacillus mallei* derived from an infected horse. I have not seen a case and can only base my remarks on the observations of others.

After local inoculation of the skin a papule or pustule appears, which rapidly breaks down into an irregular ulcer with undermined edges. The ulcer spreads, and the lymphatic vessels and glands in the neighbourhood are acutely inflamed.

Generalized equinia is characterized by the formation of small cutaneous and subcutaneous swellings known as "farcy buds," which break down. The mucous membrane of the respiratory tract becomes affected, the nasal cavity undergoes extensive ulceration, with the formation of foul discharge and crusts. Accompanying these are manifestations of grave sepsis with high fever and joint pains. Acute cases are rapidly fatal, but occasionally we meet with a chronic type which may end in recovery after some months. Early removal by the knife, and injections of mallein, offer the best chance of recovery. The slide shown illustrates a chronic type of case reported by Dr. Jacob and Dr. Turnbull [15].

(4) *Acute Malignant Pemphigus.*—This rare affection, which occurs in butchers' slaughtermen and tanners, is often rapidly fatal. It is characterized by the formation of bullæ or blisters, varying in size from that of a pea to that of a pigeon's egg. The mucous membranes of the nose, mouth, pharynx, &c., are involved. There rapidly supervene symptoms of septicæmia and the patient usually succumbs within a week of the infection. In some cases a wound of the finger is the site by which the virus is introduced. My colleague, Professor Bulloch [16] [17], has isolated a diplococcus, which is the possible cause of the disease. This organism has also been found by Demme and Wells.

(IV) Diseases Believed to be Microbic.

The affections here considered are believed to be due to ultramicroscopic organisms which have so far eluded observation. In some instances the virus will pass through a filter.

Vaccinia (*Cow-pox*).—I need not here enter into the phenomena of vaccination, but will confine my remarks to cow-pox [18]. The lesions in the cow are usually on the udder and teats, though they may occur elsewhere. When, therefore, the disease is conveyed to man, the site of infection is usually the fingers or hand, and the patient is commonly a milker. In the cow the papular stage is rarely seen, the veterinary surgeon usually being called for treatment when the lesions are vesicular. The size of the vesicles varies from that of a threepenny piece to that of a shilling; they are umbilicated in form, and surrounded by a zone of erythema. Vaccinia is communicable to horses, sheep, and goats, as well as to man.

In 1913 I had a characteristic case, that of a cowman, aged 51; the lesions began as papules, rapidly becoming vesicles, but when first seen they were superficial ulcers covered with a dark scab, one on the left index finger being the size of a sixpence, and the other on the right little finger being as large as a threepenny piece. The lesions were tender. On the back of the left hand and on the left forearm were two pustules. The patient had been vaccinated in infancy. He had not had small-pox. He had noticed sores on a cow's teats about one week before the first lesion appeared. He milked some fourteen

cows twice a day. Another cowman in the same dairy was similarly affected. (Photographs of the patient and of a monkey and rabbit which were inoculated in Professor Bulloch's laboratory are shown.)

Foot-and-Mouth Disease.—I have never seen a case which I have recognized as foot-and-mouth disease in my clinic, and I am indebted to Dr. Hugh Gibbon [19] for the illustration which I show of a characteristic case. Foot-and-mouth disease may be communicated directly to man by drinking infected milk and even from butter made from infected milk.

Professor MacFadyean quotes the experimental production of this affection by a German veterinary surgeon who drank a quart of milk from an infected cow. Five days later a vesicular eruption appeared on the mucous membrane of the tongue, cheeks and lips and on the skin of the hands and fingers.

Dr. Gibbon's case occurred in a Norfolk man aged 35. The condition began as a herpetiform eruption on the lips and spread to the buccal and lingual mucosæ. Within twenty-four to forty-eight hours bright red spots appeared on the hands, arms, feet and legs, principally on the extensor surfaces. Vesication occurred on them and extensively in the mouth and on the lips. Simultaneously, conjunctivitis with much photophobia appeared, and this went on till a purulent ophthalmia developed. The temperature rose to 103° F., with morning remissions. The patient complained of great discomfort of the eyes and mouth and tenderness of the extremities. He made an uninterrupted recovery.

Professor MacFadyean [20] has suggested that it is not improbable that some outbreaks of epidemic stomatitis in man may be foot-and-mouth disease.

Molluscum Contagiosum.—I do not propose to enter into a discussion as to the relationship between molluscum contagiosum of man and the similar tumours found on the beaks and eyelids of chicken, pigeons, sparrows and other birds. I have had the opportunity of seeing several instances of these tumours in fowls, and twice in game birds. To the naked eye, and microscopically, the resemblance is extraordinarily close. It has been suggested that the disease is identical in birds and man, and may be communicated from birds to man directly or indirectly—an opinion supported by such acute observers as Colcott Fox [21], Salzer [22], and Shattock [23].

These bird tumours have been successfully inoculated into the human skin. The virus of molluscum contagiosum passes through a Berkefeld filter, and the filtrate, though sterile, is capable of reproducing the disease in the human subject.

Mr. Jackson Clarke [24] in a recent communication has suggested a line of research to demonstrate a protozoon which he believes to be the causal organism; this he has provisionally called *Plassomyxa contagiosa*.

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Discussion.—Mr. HUMPHREY NEAME said that the only cases he recollected as having any bearing upon Dr. Sequeira's paper were two cases of vaccinia of the eyelid, in each case in a mother whose infant had been vaccinated recently. In each there was a lesion of a similar

nature to that described by Dr. Sequeira, but with intense œdema of the eyelids, so that the eye on the affected side was completely closed.

Captain H. KIRK said that Dr. Sequeira's highly instructive and interesting paper had seemed to infer that sarcoptic mange was easily and somewhat frequently contracted by man from the dog and cat. That had not been his experience in general practice, and he saw quite a number of cases every year. It had been very seldom indeed that an owner had ever complained to him of any skin irritation. As concerned the spread of the disease from the horse to man, he considered this was also infrequent. During the war he served for nine months in a mange hospital, the personnel of which totalled 2,500; but to his recollection, the numbers that contracted scabies were, relatively, very few. Later, however, when he joined a division, he contracted it himself, but owing to a fear of losing his division, he did not report to the medical officer. He (Captain Kirk) began a course of self-treatment which lasted three months, and at the end of that time he felt perfectly sure that no parasite could possibly have survived. Although the irritation still persisted, he decided to discontinue the use of sulphur, and from that time he became perfectly well. It was obvious that the sulphur had set up a dermatitis which he had mistaken for scabies.

It might be interesting to note that the *Sarcoptes minor* of the cat did not burrow in subcutaneous tunnels, but it was to be found just within the papillæ. We had in the dog and cat another type of mange known as symbiotic or otodectic, which was localized exclusively in the ear. It might be found in probably 95 per cent. of cats examined, its origin being often quite obscure. He had reared a kitten, in his flat, from three weeks of age, and in spite of the impossibility of its meeting another cat, or of escaping, he found that at twelve months of age it was suffering from auricular mange. How it could have been contracted had remained a puzzle.

He was not surprised that Dr. Sequeira had never seen a case of glanders, as he thought he was correct in stating that the veterinary profession had stamped out the disease in this country. He certainly had seen a case himself, but it was so long ago that he had forgotten all about it, and he thought it highly improbable that he would ever get another opportunity.

A skin disease in dogs which was quite the least amenable to treatment was follicular or demodectic mange. In these animals it was looked upon by veterinary surgeons as almost incurable, and the lesions in the later stages were very severe, and revolting to the sight. The last incurable case he dealt with was in an Alsatian wolf-hound, the skin of which was a mass of gaping, suppurating erosions. Many of these had become confluent, and large areas of skin were, in consequence, in an appalling state. Instead of the intense itching which characterized sarcoptic mange, a tenderness, and a disinclination on the part of the animal to have its skin examined or handled, were observed. The disease frequently began as a squamous type, with loss of hair, wrinkling and dry scaly appearance of the skin, and considerable deposit of black pigment upon the sites of the lesions. It then developed into the pustular form, unless resolution occurred in the meantime; and upon microscopic examination of the pustular contents, very great numbers of the *Demodex folliculorum* could be found—usually in association with micrococci. As far as he could ascertain, and apart from what the author had said, the human being did not suffer from this affection, but appeared to harbour the parasite normally in the skin without the production of any lesion. But upon the question of intercommunicability he welcomed the further views of Dr. Sequeira and others.

The case of follicular mange in the dog, which he exhibited, had been treated by him as follows: When the condition was only squamous, he used an ointment composed of sulphur 4, ol. picis 1, and vaseline 20 parts. After three days the malady spread and Hunting's dressing was adopted. This consisted of creosote 1, liquor potassæ 2, and ol. olivæ 14 parts. The whole of the abdomen was shaved and scrubbed, and the dressing applied once daily. The skin condition became rapidly worse during the next week, and he again changed the dressing to a $2\frac{1}{2}$ per cent. solution of zinc chloride, lightly scrubbed into the lesions once daily. This promised good results. In addition, he was bathing the dog every three days and inoculating it with a staphylo-streptococcic vaccine. He could now see an improvement after only twelve days of this treatment. A peculiarity about follicular mange was the fact that in spite of every appearance of a cure, the disease would reappear in as bad a form as ever, in a most persistent manner; it was therefore very difficult to say whether an animal was cured or not, and prophylactic measures must be adopted for a very long time after the animal's discharge from hospital.

Lastly, could any light be thrown upon the ætiology of chronic pruritus? Very large numbers of dogs were brought to veterinarians, exhibiting intense and constant irritation of the skin, and examination of these cases revealed absolutely nothing to account for it; nor were any lesions set up.

Captain C. DAVENPORT said that he could not agree that the Demodecidæ closely resembled one another in appearance. The equine parasite had special features which rendered it easily distinguishable from the human parasite which had been thrown on the screen. For instance the *Demodex equi* was very much shorter in the abdomen, the legs did not project so far from the edge of the thorax and the whole outline was more oval. The differences were sufficiently pronounced to render it easily differentiated from the human parasite should it ever be found on human skin.

The question of contagion, in relation to so-called demodectic mange, was of special interest. Several cases in the horse had recently come to his notice but there was no evidence to show that the disease was contagious in the ordinary sense. Most veterinarians held a similar view in regard to follicular mange of the dog. If the parasite was not readily transmissible from horse to horse, or from dog to dog, it would appear improbable that infection of man from animals would readily occur.

Dr. T. W. M. CAMERON said that it was surprising that no one had drawn attention to cattle as a source of infection of sarcoptic mange. In Scotland, at least, these animals were very frequently infected and were a source of the so-called dairyman's itch. A psoroptic mange existed in cattle, but the form to which he was referring was the sarcoptic variety. The question of the intertransmissibility of *Demodex* was most interesting. Dr. Hirst, of the British Museum, had recently published an exhaustive monograph on the genus, in which he clearly showed that the species in man was not the same as that in the dog. In fact all the domestic animals had its own peculiar species which differed anatomically from all the others—a possible exception being that of the sheep, which Hirst regarded as a variety of the canine form. This was a different state of affairs from what obtained in the case of *Sarcoptes scabiei*, only varieties of which were found on the various animals. It was therefore improbable that the few recorded cases of human infection by *Demodex* from a canine source were correct. The pathogenicity of the parasite was not yet settled; he had found it in dogs which showed no sign of demodectic mange, and he believed that it would be found to be as common as the human variety was reported to be—and as a rule a little pathogenic. It was believed in many quarters that the "mange" was really due to a micro-organism which was introduced by the parasite. It was at least equally possible that the parasite merely found a suitable medium for multiplication in the lesions caused by the bacteria; and that it had really nothing to do with the disease. There could be no doubt, however, about its contagiousness. It must be, to some extent at least, contagious, in order to be present in the skin at all. Otherwise one would have to believe in spontaneous generation.

Mr. G. W. DUNKIN raised the question of the comparative anatomy of the parasites, photographs of which had been thrown on the screen.

He stated that, contrary to the view apparently held by the reader of the paper, there were very gross differences between the parasites of human subjects and those commonly associated with analogous diseases in the lower animals. He mentioned in detail some of these differences.

In regard to camel mange, there was no question that this was exceedingly contagious, and the well-known case of a whole army division being thrown out of action from this cause during the Palestine operations in the late war was a case in point. In addition, natives who were in frequent contact with camels were not uncommonly affected, although this disease in native-owned camels of the desert tribes was by no means common.

Mr. R. G. HANCOCK said he would confine his remarks to one point. As a country practitioner he, of course, met with cases of transmission of the manges and ringworms from his patients to human beings.

The analogy of the plant which flourished on one soil, survived on another with difficulty, and died rapidly on yet another, seemed to be not sufficiently recognized. He had recently observed two cowmen who had contracted ringworm from the same cows; one had a few patches, which worried him very little, the other was very ill, had abscess-like lesions and suffered acutely. The body fluids and tissues of one patient were more favourable to the

parasite than those of the other. Undoubtedly the *Demodex* was a common parasite on dogs, but the soil was usually in such condition that existence was barely practicable for the *Demodex*. What suddenly rendered the soil exceedingly favourable to the life of this parasite in certain individuals remained to be shown.

Undoubtedly the scabies parasites were frequently harboured by virulent individuals who could act as carriers without showing very noticeable lesions. The spaniels as a breed were great offenders in this respect. Such carriers showed occasional exacerbations generally at moulting time, which often cleared up naturally. Where mange failed to yield to local applications, there was a large field open for experiments to be conducted in the endeavour to alter the state of the body fluids to a constitution inimical to the life of the invading parasite.

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Section of Dermatology.

President—Dr. E. G. Graham Little.

Psoriasis and Seborrhœic Eczema: Pathological Anatomy and Diagnostic Histology of the Two Dermatoses.

By A. Civatte, M.D.

Physician to the St. Louis Hospital, Paris.

I keenly appreciate the honour paid me by the Royal Society of Medicine in inviting me to be present at this meeting, that is to say, by appointing me to bring here an echo of the St. Louis, and take the place occupied two years ago by my master, Darier.

I am here at your invitation for a few moments as the accredited representative of French dermatology. I thank you; but I shall not flatter myself, nor shall I consider my mission accomplished, unless I succeed in persuading you to share my view on the subject I am about to present to you, namely, the essential differences which separate psoriasis from what has been called (after Unna) seborrhœic eczema. I bring with me evidence which seems convincing to me, and hope it will prove so to you; this evidence is histological.

At the present day histology attracts much less attention than it did twenty years ago. New methods of research seem to put it a little in the background, and young students are inclined to believe that it has had its day. This is hardly a fair view to take. No scientific method entirely fulfils the horoscope cast at its birth. An enthusiasm, which survives all deceptions, makes each generation believe that it has found the key to the riddle hitherto unsolved. Nevertheless the next generation, fascinated in its turn by new methods of investigation, treats its predecessors with disdain, without reflecting that they, too, may have had a glimpse of the truth from another point of view. Let us not be surprised that too much has been asked of histo-pathology; at the same time let us not forget that even if it cannot pretend to solve all dermatological problems for us, it will often prove a great resource and will sometimes enlighten us, when the clinical examination leaves us undecided.

I hope to illustrate the value of this resource in a problem of everyday familiarity, in which, it appears to me, sufficient use of it is hardly ever made. I mean in the differential diagnosis between dry and squamous seborrhœic eczemas and psoriasis.

Who is there who has not hesitated in the presence of an erythemato-squamous psoriasiform eruption? Of course, when all the lesions are of about

the same type, and present, on scratching, the sign "*tache de bougie*," and the "*rosée sanglante*," we all diagnose psoriasis. And if this eruption has been preceded by one or more identical outbreaks, our diagnosis is still more certain. But if the eruption is situated more on the flexor than on the extensor surfaces, if it is asymmetrical, discrete, and above all confined to one or two patches, what are we to diagnose?

Psoriasis may present these characteristics, but they are also those of the squamous dermatoses which, for fifty years have received successively the names of "seborrhœic eczema," "seborrhœides," and "parakeratoses," and which Darier to-day calls "eczématides." According to Brocq they are of a lighter red and their elements are less infiltrated. The curette never separates the last pellicle at one stroke, and when this is removed we do not see the *rosée sanglante* of Auspitz, but a purpuric surface and cupuliform depressions, from which a little serum exudes. But these are merely shades of difference, and Brocq adds—"In making a differential diagnosis it is necessary to take typical cases."

We give way to this difficulty too often—the more easily because the treatment of the two affections is practically identical. Despite this, the prognosis in the two cases is quite different. Psoriasis will return. We bleach the patient, but do not cure him. Psoriasiform eczématide, on the contrary, disappears, if properly treated, and does not recur. It is important to be certain and to warn the patient, or at least those around him.

I hope to convince you that the differential diagnosis, although sometimes so perplexing even to the expert eye, is fairly easy under the microscope. But directly this problem is approached, the question broadens. If histology can establish a difference between seborrhœic eczema and psoriasis it must define and classify the former condition, on which so far there is no unity of opinion. It is impossible to treat completely a subject of such magnitude in a single lecture, but it is also impossible not to face it. After having described to you in detail the histological differences between psoriasis and the eczématides, I shall indicate in a few words the relations between these and eczema that the microscope reveals. Thus we shall have exactly defined this mysterious dermatosis.

Before showing you how histology has settled the long-standing dispute, it may be well to investigate how this dispute originated, and how it developed.

Your illustrious fellow-countryman, Willan, with the genius of a great clinician, applied the methods of Plenk, which classified dermatoses by their elementary lesions—truly an admirable work, on which is based the dermatology of the nineteenth century. Pioneer of a reform embracing the whole of the dermatoses, he could only take account of the most pronounced types, but bequeathed to his successors the difficult task of fitting into his classification the varieties that he (Willan) had left on one side. Much controversy, however, arose when the problem was approached, and it was necessary to modify more or less the classification that had been handed down. In psoriasis and the allied dermatoses we have a striking example of these difficulties.

Of all squamous dermatoses, psoriasis is the one in which the eruption is most characteristic; all the features are nearly identical, and each of them retains the same aspect during the whole period of the eruption. A scale is always found, which on scratching throws off a white powder; this is the so-called sign "*tache de bougie*." When the scale is removed, a soft red and shining surface is revealed. This is formed by a pellicle which the last scrape

of the curette removes in one piece; it is the Duncan Bulkley sign. This pellicle removed, a large number of minute drops of blood, all of the same size, glisten on the denuded surface. This is the sign "*rosée sanglante*" of Auspitz.

But side by side with this well-defined dermatosis there are others. Their eruptions are a little less monomorphic; their sites of election are those which psoriasis is inclined to avoid; their elementary lesions are not quite so red, and are often a little less dry. There are still others; these have always erythemato-squamous elements; some are nearly related to the preceding, but the majority resemble psoriasis only very distantly or not at all. In some the scale seems impregnated with fat, and sometimes minute, early lesions consist merely of perifollicular fatty crusts. In others the surface of the lesion is but faintly pink and covered only by small scales of powdery appearance. Thus all stages are observed from psoriasis down to the squamous eruption called by Willan pityriasis alba, and all these dermatoses, from the least psoriasiform to that which resembles psoriasis so closely, are evidently related; they occur on the same type of skin, they have the same distribution, and above all, their evolution presents the same uncertainties. Some are sometimes dotted over with minute crusts, such as are seen in eczema when it is disappearing. Others became transformed rapidly; their surface strips suddenly and begins to ooze; on the bright red base the eczematous pits of Devergie appear as dark red points; the lesion can no longer be distinguished from weeping eczema.

Is it possible that this long series, varying from psoriasis to pityriasis alba, is an attenuated psoriasis which may be accidentally complicated by eczema? Must we here recognize only eczemas capable of assuming for a time the mask of psoriasis, or are we dealing with distinct dermatoses, neither eczema nor psoriasis, which, under certain influences, may indifferently assume the appearance of either?

These three hypotheses have been advanced:

(1) They are separate dermatoses, according to the first disciples of Willan, who include them in the group of pityriases next to those already established by Willan.

But how can we avoid being struck by the frequent association of these dermatoses and the sebaceous flux, and by their predilection for the regions where this sebaceous flux is most in evidence? Plenk had already stated that the scales were only dried sebaceous material. Hebra made the same mistake independently, and called all these erythemato-squamous eruptions dry seborrhœa. This confusion, perpetuated by Hebra, will persist for a long time; even to-day we hear dry seborrhœa spoken of.

On the other hand, since the time of Biett and Rayer, eczema is no longer the eczema of Willan. Cazenave already acknowledged a dry eczema; and all the old authors of Saint-Louis recognized that the boundary is uncertain between this eczema and the pityriases. Further, the lesion, later named "seborrhœa corporis" by Duhring, had for some time been called "flannel eczema" at Saint-Louis. A step further, and all these dermatoses would come under the heading of eczema.

(2) This step was definitely taken in England by Erasmus Wilson and in France by Hardy. But it was Unna who succeeded in bringing the theory into the limelight; first because he brought forward fresh arguments, and, it seemed, tangible proofs founded on histology and bacteriology; and possibly because he coined a new word. For him all these dermatoses, ranging from psoriasis to pityriasis alba, constitute nothing more than varieties of eczema —*seborrhœic eczema.*

Here we are approaching contemporary history. The discussion opened by Unna is not closed. It may be said that it has met with as much criticism as acceptance. And, speaking only for the French school of to-day, I see its three masters, Brocq, Darier, Sabouraud, holding three different opinions with regard to this.

Brocq, who insists on vesiculation in the formula of eczema, cannot envisage eczema in the purely erythemato-squamous dermatoses. For him they are "seborrhœides," or better still, as he has asserted that from henceforth seborrhœa will not come into consideration, they are "more or less psoriasiform parakeratoses." These parakeratoses may become eczematized accidentally.

Darier, on the other hand, struck by the histological and clinical relations between these dermatoses and eczema, calls them "*eczématides*" thus recognizing at least an affinity between them and eczema.

Sabouraud, on the contrary, separates them clearly and gives them their old name of "*pityriasis.*" At the same time he does not thus designate them all. Many of these dermatoses (one-third at least, according to him) are psoriasis bacteriologically and histologically. They might be termed the "*psoriasides.*"

(3) Indeed we find another difficulty here, quite as great as the first one. The relationship of this group to psoriasis is no better defined than to eczema; and encroachments were to be anticipated on the two frontiers. Unna had effaced the second as well as the first, and had himself decided with implacable logic to classify psoriasis with eczema, psoriasis becoming the driest form of seborrhœic eczema. We have just seen Sabouraud, on the contrary, make psoriasis encroach on Brocq's seborrhœides. Török, more radical still, unites all seborrhœic eczema with psoriasis; it is, he says, a more or less eczematized psoriasis. Thus he goes still further than Sabouraud, and takes exactly the opposite view to Unna.

These three contradictory theories of the old dermatologists of Saint-Louis, of whom one sees in the pityriases separate dermatoses, another eczema, and the third psoriasis, involve among other arguments the aid of pathological anatomy. Are we to consider this branch of science as bankrupt?

Certainly not, and in my turn I am going to rely on it to show you that *seborrhœic eczema—with Darier I say, the eczematides—dermatoses very nearly related to eczema, are to be separated entirely from psoriasis, and that none of Brocq's seborrhœides, or Darier's eczematides, should be credited to psoriasis any more than the latter category should be impoverished on their account.*

I have too little time at my disposal to discuss in detail their relationships with eczema. On the contrary I wish to insist upon their distinctions from psoriasis; this is the point from which we started, and it must remain the principal object of our study.

During the course of this demonstration, you will see that, if the border lines between psoriasis and the eczematides are clinically somewhat vague, they are histologically very clearly defined. But this is true only on condition that the microscope is made use of in a manner different from that too often adopted.

Why is it that so many medical men who, in their clinical examinations, multiply their investigations, study every organ and attach the utmost importance to the evolution of the symptoms, ask ingenuously that they shall be shown the diagnosis of the affection studied written clearly in every microscopical section? The dermatoses characterized by an abnormal cellular form, or a particular arrangement of the cells, are not very numerous. Moreover, even in these it is not to be hoped that characteristic pictures will be found in

every section. In the majority of dermatoses these pathognomonic pictures are entirely wanting. But even in this case histology is not disarmed; it will find in the mode of the appearance and arrangement of the elementary microscopical lesions, the same help that clinical examination demands in the evolution of macroscopic lesions. Let us learn to reconstruct a living image with pictures rendered immobile by means of fixatives. We shall thus arrive at characteristic histological formulæ for dermatoses, in which fragmentary examinations would only reveal trivial anatomo-pathological lesions, differing from one examination to another.

Now, if with the microscope we thus make an analysis of psoriasis on the one hand, and of all the forms of eczematides on the other, we obtain two histological formulæ which have no similarity at all between them, although the two processes thus summarized result in lesions which are almost identical.

If the terminal lesions only were looked at, there would be almost as much difficulty in the laboratory as at the bedside of the patient. But, while there the eye cannot penetrate any farther, the microscope will always reveal in some section at least one of the preceding stages by the side of the fully-developed lesion; from this the intermediate stages can be reconstructed and the diagnosis made.

I will give you proofs of this assertion. First, look at this section of psoriasis; let us take it from a well-developed patch:—

The silvery scales appear as a mass of superimposed lamellæ, with little adhesion between them, formed of horny nucleated cells, containing but little fat. Below this scale the stratum granulosum is absent or is reduced to a few islets of granular cells. The absence of kerato-hyaline and the imperfect keratinization constitute parakeratosis. This is the most striking lesion in psoriasis. Here and there, between the beds of horny parakeratotic cells, masses of shrivelled polynuclear leucocytes are found, and in certain cases of psoriasis the scale is literally stuffed with these islets of leucocytes. The scale rests on a rete mucosum, in which the interpapillary portions are elongated owing to hyperacanthosis, but the epidermal strata are reduced in number above the papillæ. Between the scale and the rete here and there is seen an ill-defined cavity, full of leucocytes, mostly polynuclears. That is what Munro and Sabouraud have called the micro-abscess of psoriasis. Below this mass polynuclears are as a rule seen migrating through the rete mucosum. Corresponding to these epidermal lesions there are *lesions of the derma.* The papillæ are elongated, cylindrical or clubbed, and œdematous. The vessels are congested, and surrounded by a sheath of mono- and poly-nuclear cells.

These histological lesions explain all the clinical characters of the elementary lesion of psoriasis. The œdema and infiltration are responsible for the projection of the erythematous base. The air between the horny layers makes the scale silvery, opaque and friable. The rete mucosum, being devoid of kerato-hyaline, is transparent, thus revealing the subjacent congestion, to which the reddening of the lesion below the scale is due. Finally, when this thin epithelial layer is removed by the last scrape of the curette, a small drop of blood will escape from each decapitated papilla, hence the abundance and regular arrangement of the hæmorrhagic points, which form the "*rosée sanglante.*"

Now we are going to see how this eruption develops. It is the most delicate, but at the same time the most interesting part of our study. Here

are illustrations showing what may be seen in early lesions, or when the fully-developed ones are in process of extension.

A group of migratory cells emerges from a vessel at the summit of a papilla and opens up a passage through the rete mucosum, which is disintegrated by their transit. This disintegration is effected in a very peculiar way. At the point where it is about to occur, the Malpighian cells become swollen and massed together, their borders become less distinct, and there is considerable *intracellular* œdema. It follows, then, that the dictum of Sabouraud that "the exocytosis of psoriasis is dry and without exoserosis" can only be accepted with reserve.

In this swollen and softened mass, the emigrated cells dig a sort of tunnel for themselves, directed obliquely towards the surface, by which they ascend towards the upper layers of the rete body.

In suitable sections a sort of fissure is thus seen of more or less considerable size, according to the age of the lesion, or more probably according to the extent of the exocytosis. The narrowest fissures allow the passage of only a few cells, for the most part large mononuclears, but in the largest are seen a large number of cells, among which the polynuclears predominate.

This tunnelling of the rete is, moreover, not the only way by which the exocytosis takes place; polynuclears are seen all around insinuating themselves between the swollen epithelial cells which form the walls of the tunnel, and crossing the softened rete without tearing it.

Having thus reached the upper Malpighian layers, the migratory cells encounter a barrier, formed by the more or less keratinized layers of the surface; and the little vertical trail they trace spreads out like a coil of smoke beaten down by an obstacle. Then, when the exocytosis is finished, the crevice closes and the emigrated cells find themselves imprisoned in the upper Malpighian layers, where they form a lenticular mass. If the number of cells is considerable, this mass of polynuclears simulates a small abscess—the micro-abscess of Sabouraud and Munro. This does not consist of actual pus: the leucocytes have not degenerated and are not seen floating in a serous fluid; they infiltrate between the cells, and, as Sabouraud has remarked, the mass is divided into partitions. I may add that in masses of moderate size, where this partitioning is not evident, the presence of numerous mononuclears prevents confusion with a true abscess.

The rete, disorganized by this cellular migration, does not produce any kerato-hyaline at this spot; hence the parakeratosis; it is the origin of the scale. If there is at this point a leucocytic mass of some size, it is included between two layers of parakeratotic cells, and borne with them towards the exterior. The section will show this mass lying at varying levels in the scale, according to whether the eviction is more or less advanced. And while the first parakeratotic focus is being eliminated, with or without a cellular nest, a new one will be formed below, and others at the side; and thus, from place to place, the element of psoriasis persists and spreads. The thickening of the scale will be proportional to the frequency and rapidity with which this process is repeated at the same points. Simultaneously the rete will proliferate more and more, but, being constantly worn away on the surface, it will only show traces of this proliferation at the site of the interpapillary processes.

Finally, in this incessantly renewed and softened mucous body, the tunnel is no longer formed; a little œdematous intracellular focus above the papilla, where the exocytosis is going to occur, is sufficient to give passage to the leucocytes.

You will observe that all these lesions are linked together; and it seems useless for me to attempt to attribute a special importance to one more than to another. All may equally well lay claim to the title of elementary histological lesion of psoriasis. Sabouraud gives this to the micro-abscess. When this nest of polynuclears exists, the designation is correct, but the micro-abscess is often absent. Haslund has observed this and I can confirm it. Indeed, I would add that it is merely accidental, and is only an indication of an extensive column of exocytosis. Therefore, it is this column which might be called the elementary histological lesion of psoriasis, as it is the essential and constant element. Its plume-like shape, the tunnel it often produces in the rete, and its composition, in which polynuclears predominate, give it a highly characteristic appearance, and make it an important element in histological diagnosis.

I shall pause for a still shorter time over a discussion which, nevertheless, greatly occupied the attention of the first histo-pathologists of psoriasis, namely, whether the dermatosis originates in the dermis or in the epidermis. The question can be considered from different standpoints, and therefore admits of apparently contradictory opinions. It is in the dermis that the lesion commences, since the exocytosis starts from the papillary body; but it is in the rete that it first becomes characteristic; moreover we are ignorant of the site of the agent which provokes it.

Opinions vary according to the manner in which a problem is visualized. This explains why so many different solutions have been given to this one with the same positive data. All the lesions I have described to you were seen by the first investigators, and it could not have been otherwise. What has changed is the interpretation that has been given them, or the importance that has been attributed to them. Haslund, in an article which exhausts the question, tries to render to Cæsar the things that are Cæsar's, and, with regard to each lesion, mentions the name of the author who first recorded it. This really matters very little. For example, the micro-abscess seen by Schutz, Kromayer, Mantegazza, Kopytowski, will remain the micro-abscess of Munro-Sabouraud, because it is to these two authors that we owe a wonderfully penetrating analysis of the formation of the scale from the starting-point of this abscess.

We are, then, in possession of a very pliable but very exact histological formula of psoriasis. We will now in the same way work out the histological formula of seborrhœic eczema, this protean dermatosis whose characteristics clinicians have had such difficulty in determining. As it is under its most psoriasiform aspect that this dermatosis raises the difficulties of diagnosis just recorded, it is under this form that we will study it especially.

Nevertheless, we must state at once that the microscope shows us that all the clinical forms of eczematides are constructed on the same plan, and from the same histological elements. There is no difference between them, except in so far as one or another of these elements may assume a greater or less predominance.

What do we see in a section of a very psoriasiform eczematide?

A foliaceous scale, adherent, or detached during the biopsy or embedding, a scale formed of parakeratotic cells—that is to say, cells the nucleus of which is still intact. This scale contains very little fat, contrary to what one would have expected. On the other hand, it encloses droplets of coagulated serum; it is this serum, and not fat, which has given it its oily consistency. Finally cell-nests are also sometimes included in it. Below, the stratum granulosum is

absent in part or completely. The rete mucosum throws out hypertrophied processes between the papillæ; but on the other hand it is often relatively thinned in the portion that caps the papilla.

So far the appearances are almost exactly the same as in psoriasis, or are even identical. There is, however, one feature that we shall often see in the eczematide, and that we have not so far seen in psoriasis: In the mucous body, intercellular œdema has disassociated the Malpighian cells and the intercellular filaments are stretched apart. This is what is known as spongiosis. In the enlarged intercellular spaces mononuclear cells, especially lymphocytes, have insinuated themselves.

In the dermis the papillæ are elongated, clubbed, œdematous, and congested. Around the vessels there are sheaths of cellular infiltration, where mononuclears predominate.

Let us pursue our inquiry by examining other sections, and we shall see how these psoriasiform-like lesions of hyperacanthosis and of parakeratosis are produced. We shall see that it is by quite a different mechanism from that which leads to the hyperacanthosis and parakeratosis of psoriasis.

Let us examine some foci of spongiosis. If we are careful to cut serial sections, we shall always see a little vesicle at the upper pole of this focus, or at least vestiges of vesiculation. This vesicle is hardly distinct in the middle of the lesion, but is much more obvious towards the margins. One will be found sometimes still more clearly defined, although much smaller and without any surrounding spongiosis right at the border of the element, in the zone of extension. Here we have the key to the whole process. As there is no spongiosis without a vesicle in the centre, whereas vesiculation can be found without spongiosis, it is evident that the vesicle must represent the initial element. Thus it is obvious that the lesion originates from the small isolated vesicle sometimes found at the advancing edge of the eruption.

Let us follow the progress step by step: Here is the primary small vesicle. It is minute and superficial, situated immediately below the stratum corneum. It has very clear contours, and is as if hollowed with a drill in the upper stratifications of the rete. Shrivelled Malpighian cells are seen still adhering to the horny roof. These cells, probably injured to begin with, seem to have been detached roughly from neighbouring healthy cells by the pressure of lymph, which is nevertheless insufficient to rupture the remainder of the epidermis. This pressure increases, however, and the healthy cells themselves are more or less dissociated; hence spongiosis is produced around the primary small vesicle, which often increases in size a little at the expense of this spongiosis. Large mononuclears and lymphocytes pour into the vesicle and the surrounding area of spongiosis. Normal keratinization is impossible above this spongy mass; desiccation therefore takes place, and the diseased Malpighian islet is cast off in the form of a squamous crust, that is to say, of a mass of dried cells and droplets of coagulated serum. A very slight superficial area of spongiosis will produce a chip of almost dry parakeratosis; a large mass much infiltrated will be transformed into a thick squamous crust, impregnated with serum. Below, the rete reacts and becomes hyperacanthotic, as in psoriasis, in proportion as the process is repeated.

If the lesion spreads from place to place, we shall see the same phenomena repeated at the margins. In the centre, however, in this epidermis, incessantly changed and deprived of its horny layer, the primary small vesicle is hardly seen; it will appear only as a flattened ill-defined vesicle in the middle of a

rete, no longer spongy, but made homogeneous by the intracellular œdema. Then only outlines of vesicles will be seen, and finally all vesiculation will cease. The upper Malpighian strata will dry up over considerable areas, and will form large strips of parakeratosis all in one piece. When these strips are not too much impregnated with serum, the scale thus constituted will be nearly identical with that of psoriasis.

Now if, as we did with psoriasis, we wish to select those lesions which best characterize the eczematides, at least if it is a question of contrasting them with psoriasis, it seems to me that these are the two kinds of vesicle ; the large, ill-defined vesicle that Sabouraud saw in his pityriases, and in which, as I have just shown you, we find principally mononuclears ; and, preceding this, the primary small vesicle.

I will mention, in passing, that Brocq, whose eye nothing escapes, divined the existence of these microscopic vesicles and their pathognomonic value ; he sees them as cupuliform depressions when he scratches the surface of the element with the curette.

From now onwards we can compare our two histological formulæ. They can be contrasted feature by feature.

In the eczematide : Mononucleosis, vesiculation and spongiosis (that is to say, above all, intercellular œdema).

In psoriasis : Polynucleosis, no vesicle, but a rupture of the rete mucosum ; intracellular œdema much more than intercellular, and little or no true spongiosis.

You see that it is not in the scale that I invite you to look for the elements of your diagnosis. You will find nothing more there than the effaced impression of a delicate process that must be caught at its inception, and consequently in the rete.

Does this mean that the diagnosis will always be clear at first sight? Certainly not ; for sometimes it will be necessary to examine several sections before finding the tunnel or crevice in psoriasis. And if the section is not cut in line with the axis of the tunnel, one might hesitate in distinguishing between the crevice of psoriasis and spongiosis. It is the presence of large œdematous Malpighian cells and the abundant polynucleosis, therefore, that determine the diagnosis of psoriasis.

A double cause of error may still further complicate your task : on the one hand, the eczematide may become infected and be invaded by polynuclears ; and on the other, there are cases of psoriasis which become " eczematized." These difficulties are not, however, insurmountable. The presence of numerous lymphocytes among the polynuclears ; the invasion of the rete by the latter in abundance and no longer in little columns ; their presence in the scale in large sheets and not in small masses are all arguments in favour of the diagnosis of infected eczematide. As for eczematized psoriasis, you will recognize it with certainty by the presence in the same section of lesions characteristic of both dermatoses.

Nevertheless, these complex cases are of necessity in the majority. Generally the microscope easily removes all doubt. I will give you two examples. In these two cases the clinical diagnosis rested between attenuated psoriasis and slight psoriasiform eczematide. To the eye the two lesions were the same : a rose-coloured spot with slight scaling. The sign of " *tache de bougie* " and that of Auspitz were uncertain. This is what the microscope revealed :—

The first case was psoriasis. There were the tunnel and the polynucleosis. The patient was therefore liable to incessant relapses whatever one did.

The second was a psoriasiform eczematide. There were the vesicle and the mononuclears. The patient should recover completely under appropriate treatment.

Here is, then, a group of squamous dermatoses strikingly elucidated by histology, in which clinical examination vaguely suggests only something other than psoriasis, but is unable to establish a clear differentiation between what should be and what should not be accepted as psoriasis.

Let us glance backwards and sum up the points established. I see three at once :—

(1) First of all we have proved that psoriasis has a histological formula apart, which cannot be compared to any other. And we separate this dermatosis clearly from the eczematides, in contradistinction to Unna, who included all with eczema.

(2 Then, continuing, we have seen that there are cases of weeping psoriasis, so that, up to a certain point, Török and Sabouraud were right in considering as such certain dermatoses, which numbers of dermatologists would have labelled " seborrhœic eczema," in view of the weeping.

(3) Finally, we have convinced ourselves, on the other hand, that it is paradoxical to include with psoriasis all the seborrhœides of Brocq, like Török, or even a large number of them, like Sabouraud.

To summarize, the microscope traces a clear frontier for us between psoriasis and the eczematides, and apportions the contested territories without any possible grounds of appeal.

Besides these three points which we have demonstrated in detail I perceive also the premises of another discussion. It has been agreed that we shall not leave it entirely on one side, since it must lead to the complete definition of the eczematides. I shall just touch upon it.

We have discovered that the primary lesion of the eczematides is a very characteristic small vesicle. Unna, and then Holland, appear to have observed this small vesicle in Gibert's pityriasis rosea ; and Sabouraud has seen it and accurately described it. For my part I, too, have seen it ; and, moreover, have always found it in all eczemas. Therefore, although histology raises a barrier for me between psoriasis and the eczematides, it pulls down the one which—up to the time of Unna—tradition had left between these dermatoses and eczema.

Just now we saw that elementary lesions, of different origin, give rise to the same terminal complex lesion in psoriasis and the eczematides. Inversely, I could show you that the same initial lesion produces different end-results in the eczematides and eczema. To confuse two dermatoses under the pretext that they originate from the same lesion, or because they arrive at the same terminal lesion, is to form an opinion that is wrongly based equally in the two cases. Nevertheless I admit that a common origin creates a relationship. And with regard to this, if I reject the complete confusion implied by the doubly unfortunate term " seborrhœic eczema," I find that the name " eczematide," proposed by Darier, is perfectly suitable ; it does not assimilate the two affections, but it indicates their affinities.

Moreover, what is the great value of anatomo-pathological resemblances and differences ? This brings us back to our subject, in conclusion : What is the meaning of this demarcation established by histology between psoriasis and the psoriasiform eczematides ? Must we see in it the proof that it is a question of two different diseases ?

This throws us into the domain of pure speculation. Each epoch brings

with it its own conception of the disease. Since the days of Pasteur we have endeavoured to define diseases by their causes and no longer by their lesions. *A priori*, an anatomo-pathological difference proves nothing in this respect. But even on the still uncertain ground of ætiology, the differences we have just verified under the microscope can guide us a little. The initial subcorneal lesion of an eczematide immediately evokes the hypothesis of surface infection. The lesion of psoriasis, on the contrary, if it does not exclude this interpretation, is more easily explained by the action of a virus, or of a toxin coming from within.

This conception of the two dermatoses agrees precisely with what we have seen clinically. The incessant threats of relapse in psoriasis, in spite of local treatment, the lasting cure of an eczematide by external treatment, and above all by the exfoliation provoked by strong reducing agents, are explained perfectly by the double hypothesis to which anatomo-pathology has already led us.

This is, then, a reason for attributing very great importance to the indications supplied by histology, even in the matter of ætiology, and of regarding them as valuable arguments, if we wish to look upon psoriasis and the eczematides not only as two distinct syndromes, but as two different diseases in the precise meaning we attribute to-day to the word "disease."

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case for Diagnosis (? Sarcoid).

H. C. Semon, M.D.

Patient, a male, aged 73, has lived principally in Cochin China, where for many years he has worked as an engineer. Has had both malaria and dysentery, but cannot remember having suffered from any other serious disease.

Two and a half years ago a small pimple appeared on the left side of his lower jaw, near the angle; it has slowly enlarged to its present oval shape and size (2 in. by 1 in.). It has almost the same colour as the skin, is soft in consistency, but shows no tendency to ulcerate, although infiltration is marked and causes the patch to protrude about a millimetre above the level of the skin. The growth of the hairs of the beard has not been affected in any way. A similar, though much smaller, area developed at a later date in the left temporal region. There is a third lesion on the extensor aspect of the left forearm, in which cicatricial changes have occurred, and the upper margin of which appears to be bounded by a small semicircle of raised spines, exactly resembling those met with in lichen spinulosus (although the other lesions do not in any way resemble lichen planus).

Behind the left ear is what appears to be a cystic sebaceous condition, which I do not associate with the other lesions.

No history of tuberculosis in the patient's family; Wassermann reaction negative; blood count normal.

The biopsy from the forearm lesion shows a granulomatous structure, but does not suggest any specific condition known to myself.

The diagnosis of sarcoid is suggested by the history and the clinical appearances, which much resemble those of a case shown here recently by Dr. Barber. The absence of typical epithelioid and giant cells is of course against that view, and I shall be glad to hear the views and suggested diagnoses of other members.

I froze the upper small lesion on the temple about five weeks ago. It has completely involuted, without leaving a trace, I do not know if the genuine sarcoid reacts in the same way.

DISCUSSION.

Dr. A. M. H. Gray said he had not seen the patient, but he agreed that the section under the microscope was not that of sarcoid. The striking feature about sarcoid was the lobulated appearance, with very well defined bands of fibrous tissue between the lobules, which consisted of endothelial and giant cells.

Dr. Savatard agreed with Dr. Gray's remarks concerning the section, which was not diagnostic of sarcoid. He asked whether, after applying the carbon dioxide snow to a lesion, there was any ulceration, and if so, how long it took to heal. In a case of sarcoid, in which

he himself applied CO_2 snow, healing was slow, and even after the lesion had apparently resolved, histological examination showed active mischief still in progress in the deeper tissues.

Case of Multiple Basal-celled Epitheliomata.

By H. W. BARBER, M.B.

MISS M., aged 66. The first lesion to be noticed was on the front of the chest. One on the front of the chest has undergone partial involution spontaneously. No more were observed until ten months ago, when one appeared on the scalp in the right parieto-occipital region.

There are a number of basal-celled epitheliomata scattered over the scalp, back and chest. One on the scalp and one inside the left ear have ulcerated.

Case of Arsenical Dermatitis with Multiple Epitheliomata.

By H. W. BARBER, M.B.

F. P., AGED 70, male. Hairdresser. The patient is a Frenchman, for many years resident in England.

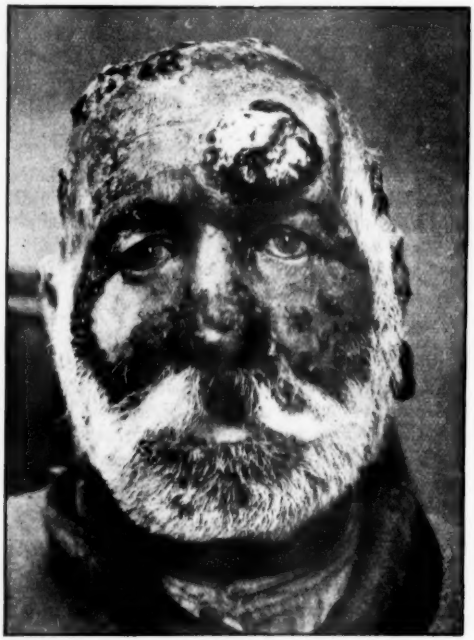


FIG. 1.

It is difficult to extract a satisfactory history from him, and his statements differ on essential points from time to time. He has, however, for a long time taken a mixture containing six minims of liq. arsenicalis to the dose, three times daily.

He has also taken a large number of other medicines, the composition of which is not known. Presumably the arsenic was given him for psoriasis, from which he has probably suffered for some fifteen years.

He came to see me at Guy's Hospital some weeks ago. At that time, apart from intense brown pigmentation of his skin, hyperkeratosis of his palms and soles, and scaly patches of psoriasis, there were large fungating tumours on his scalp, face and ears, with two large flat growths, one on each upper arm (see figs. 1, 2). The diagnosis of chronic arsenical dermatitis with secondary epitheliomata was made.

FIG. 2.

Unfortunately a section made from one of the tumours on the arms was not satisfactory and another is being prepared. The tumours on the head have been treated by a large heavily-filtered dose of X-rays, and there has been great improvement in a short space of time. There is no evidence of liver-insufficiency.

DISCUSSION.

Dr. S. E. DORE asked whether the epitheliomatous growths occurred on the psoriasis patches, as it was very rare for that to happen.

Dr. BARBER replied that it was impossible to decide this point.

Case of Parapsoriasis.

By H. W. Barber, M.B.

Miss Y. K. The eruption appeared first on the legs about five years ago, a year later on the body, and twelve months afterwards on the face.

Patient had "colitis" about three and a half years ago. The eruption on the face had, I believe, been diagnosed as lupus erythematosus. That on the body, I think, corresponds to the eruption described by Dr. MacCormac and tentatively placed in the parapsoriasis group. The lesions on the legs are scaly follicular papules.

I have not seen a case of parapsoriasis in which the face has been involved, and I make the diagnosis on the appearance of the chest lesions.

Dr. H. MacCormac said that this case, so far as the lesions on the trunk were concerned, seemed to resemble the cases shown by him at the Section some time ago; but in none of the cases coming under his observation had the curious condition of eyebrows and face been present.

Case of Diabetic Xanthoma treated with a Sulphur Compound of Histidine (Thiol-amino-methyl-glycoxaline).

By J. E. R. McDonagh, F.R.C.S.

(ABSTRACT.)

Mr. McDonagh said that the skin lesions vanished under the treatment. The fat and chloresterol in the blood receded to their normal levels and the percentage of the blood-sugar fell from 0·281 grm. to 0·162 grm. This drug was prescribed because he (the exhibitor) considered insulin to be a sulphur derivative of the amino-acid histidine. The glycosuria vanished after the first injection.

Mr. W. J. O'Donovan asked whether the patient's diet was restricted during this time.

Mr. McDonagh (in reply) said that no alteration had been made in the patient's diet.

Case of Leprosy.

By S. E. Dore, M.D.

I am indebted to the kindness of Dr. Farquhar Buzzard for the opportunity of bringing forward this case of nodular leprosy in a boy aged 14. His father, who was born in India of English parents, was under the care of Dr. Buzzard at the National Hospital, Queen Square. He came to England at the age of 14, and developed leprosy at 28, the child then being eleven months old. The father suffered from anæsthetic leprosy and died at the age of 38. The child developed the disease last May, and shows typical leprous nodules and brown infiltrated patches on the face, especially the eyebrows and chin, and on the limbs and trunk. There is no anæsthesia or hyperæsthesia, but the boy has recently complained of numbness on the ulnar side of the left hand. I should welcome any suggestions regarding the treatment of this case, as he has now been under my care during the last three months and the disease is steadily progressing. On Dr. MacLeod's suggestion he has been treated with injections of E.C.C.O. 1 c.c. every week, and three 5-mm. capsules of chaulmoogra oil three times daily, which is as much as he can take without sickness being caused.

Cases of leprosy have been treated by Dr. Winkelried Williams, Sir Norman Walker and others, by means of a vaccine made from the nodules, and Dr. Winkelried Williams, who is present, suggests that a vaccine should be administered in this case. The intravenous injection of sodium gynocardate has so far not been tried, as it necessitates in-patient treatment, and the patient's mother will not consent to this at present.

This is probably the first case of leprosy contracted in this country in a boy of English parentage, as there seems to be some doubt about the case reported in Ireland, about which Dr. MacCormac has reminded me. The boy lived in the same house with his father for ten years, but has never been out of the country.

DISCUSSION.

Dr. A. WINKELRIED WILLIAMS said that in making the vaccine an excised nodule was cut in half; one half was desiccated over sulphuric acid and reduced to a small horny mass. This was weighed, and, combined with microscopic examination of the other half, enabled a rough estimation of the proportion of bacilli to tissue to be made. In the process of desiccation one had a method analogous to the de-fatting of tubercle-bacillus vaccines. In cases treated with tissue vaccines without desiccation the effect of the vaccine had been temporary; but in one case dealt with as just described it was now nearly five years since the patient (a male) had had any treatment, and he was remaining absolutely free from any symptom of leprosy.

Dr. A. M. H. GRAY said he had at present under his care a case the experience of which might be of some use, as the patient had done very much better on intravenous injections of gynocardate of soda than under the intramuscular injections. He (Dr. Gray) had had that patient under observation four years, and he first saw him when he was 11 years of age. He was treated for a considerable time, under his own doctor, with chaulmoogra oil by the mouth and by intramuscular injection. The patient became very ill two years ago, the nodules on the face getting very large; he also had much mucous membrane trouble as well as severe pain and swelling of the legs. He took the patient into hospital and put him on a course of gynocardate of soda intravenously, and the condition subsided in an extraordinary way. It was continued as long as the material could be got into the veins. He was then put on to E.C.C.O. injections and the patient's condition had continued to improve.

Dr. G. PERNET said his impression was that boys with leprosy did not do so well in this country as adults, in whom chaulmoogra oil as a rule had such decidedly good effects. Leprosy was not hereditary. The present case was a record, having arisen in England in a patient never out of the country. Some years before the war, the late Professor A. Wolff, of Strasburg, had a boy in his clinic with leprosy, who had never been out of Alsace. The disease was traced to an uncle of the boy, who had also attended the professor's clinic, and had contracted leprosy in Tonkin. The boy occupied the same bed with his uncle. At the Saint-Louis Hospital in Paris there were always cases of imported leprosy, but no case of leprosy had ever been contracted from any leper at that hospital, although some of the lepers had sojourned there for years.[1] At any rate, he (Dr. Pernet) had never come across anything of the kind chronicled in French dermatological journals.

Case of Morphœa Guttata.

By S. E. DORE, M.D.

THIS is a striking case of the so-called "white-spot disease." The patient is a girl, aged 13, who developed the condition eighteen months ago.

There is an area of small, closely aggregated white patches on the inner sides of the knees, larger patches on the chest, abdomen and back, and a few scattered lesions on the neck and shoulders. One or two of the lesions are scaly and slightly raised above the surface, but I think the condition is a true morphœa and not lichen planus.

[1] See Pernet, art. "Leprosy," *Quarterly Review*, No. 394, 1903.

Xanthosis of Hands and Feet in Diabetes Mellitus ("Ochrodermatosis" of Castellani, "Xanthochromia Cutis" of French Authors).[1]

By F. Parkes Weber, M.D.

(ABSTRACT.)

The patient, a young man, aged 20, was in hospital from May 14 to June 16, 1924, for diabetes mellitus. Under insulin treatment the urine became free from sugar and the acidosis disappeared. He was readmitted on October 3, 1924, with sugar in the urine and urinary signs of acidosis; and a curious canary-yellow coloration of the skin, practically confined to the palms of the hands and the soles of the feet, was then first noticed. There was not the slightest icteric tinge of his sclerotics.

The blood-plasma has a milky yellow appearance and gives negative *direct* and *indirect* Hijmans Van den Bergh reactions for bilirubin. The "milky" blood-serum can be clarified by shaking with ether. The ether likewise extracts the yellow pigment, and a yellow supernatant layer of ether is obtained, which when evaporated on a glass slab (Dr. E. Bock) leaves a greasy film on the glass. The urine is free from bilirubin and any abnormal amount of urobilin or urobilinogen; it is rather pale at present, and no yellow pigment can be extracted by shaking it with ether. No "lipæmic" appearance of the fundus oculi can be detected.

When the patient left the hospital in June he understood that he was to eat plenty of green vegetables. This he did, especially cabbage. Green vegetables contain the yellow pigment carotin, as do carrots, and this (as Dr. H. G. Adamson remarked to me) may have an important bearing on the development of the present "xanthosis" of the skin.

DISCUSSION.

Dr. H. G. Adamson asked Dr. Weber whether he had considered the diagnosis of carotinæmia in this case. The bright yellow colour, its distribution, and the fact that the patient was a diabetic, and had possibly been on a largely vegetable diet, suggested this diagnosis.

Dr. Parkes Weber (in reply) said that he had not heard of this pigmentation doing harm in itself, but he had heard of a yellow carotinæmic pigmentation developing simultaneously with improvement in the case of an anæmic girl. She had been told to take *claret*, probably because it contained iron, but owing to a misunderstanding she took a great quantity of *carrots* instead of claret. She improved in regard to her anæmia, though after some months her skin became yellowish. The story was told to Dr. A. T. Todd (of Bristol), from whom he (Dr. Weber) had heard it.

Case of Lymphatic Leukæmia with Erythrodermia.

By J. M. H. MacLeod, M.D., and J. E. M. Wigley, M.B.

A. H., aged 59, valet.

Patient had never had any trouble with his skin till two years ago, when he complained of severe irritation all over the body, which was severe enough to prevent sleep. The skin was then of normal colour. He first noticed it becoming red in January this year. He then attended St. John's Hospital for Diseases of the Skin, where he was told his blood and urine were normal.

In May, this year, his face became bright red and began to "weep." He was sent to Charing Cross Hospital by Dr. Saint and was admitted as a case of "acute

[1] This case is included in a paper on the subject, which will shortly be published elsewhere.

eczema." During his stay in hospital there was much scaling and crusting of his face and ears, and the ears became very much enlarged. He complained of great irritation of the whole body and it was noticed that there was generalized redness of the whole skin. He improved considerably under simple emollient treatment.

In August he was readmitted to the ward with marked crusting of the face, ears, hands, and to a less extent, the feet. Skin over the whole body, excepting soles of feet, was brick-red in colour and felt thickened on palpation. Marked irritation. Slight, but definite enlargement of lymphatic glands of neck, axillæ, and groins. These glands were not tender. General health good; physical examination negative. Spleen not palpable. Urine normal. Wassermann reaction negative.

The blood was examined by Dr. Piney with the following result:—

	August 23, 1924	September 13, 1924	October 2, 1924
Red blood-cells ...	4,800,000	4,600,000	4,980,000
Hæmoglobin	90 per cent.	90 per cent.	92 per cent.
Colour index	0·9	0·97	0·9
White blood-cells (total)	13,100	10,000	12,700
Polymorphonuclears ...	13 per cent.	27 per cent.	58 per cent.
Small lymphocytes ...	82 per cent.	71·67 per cent.	39·34 per cent.
Large lymphocytes ...	1·5 per cent.	*Nil*	*Nil*
Eosinophils ...	0·75 per cent.	0·34 per cent.	*Nil*
Large hyalines ...	1·75 per cent.	1·00 per cent.	2·00 per cent.
Lymphoblasts ...	0·5 per cent.	*Nil* in 300...	*Nil* in 300

At the time of the last count the patient was running a temperature up to 100° F., which explains the difference between this and the previous counts.

A piece of skin was removed for biopsy and also an inguinal gland. Dr. Piney has kindly reported on them as follows:—

HISTOLOGICAL REPORT ON PORTION OF SKIN AND AN INGUINAL GLAND.

"*Skin.*—There is much thickening of the epithelial layers, with deformity of the papillæ. Around the papillæ there is infiltration with small round cells which are mostly lymphocytes, but a few plasma cells are present. There is new formation of connective tissue and the infiltration is most intense in the deeper parts of this, which is distinctly denser than the superficial part.

"*Lymphatic Gland.*—There is complete obliteration of the orderly arrangement of the glandular substance, in which no germ centres can be found. The capsule of the gland is thickened and infiltrated with small lymphocytes, which do not appear to extend into the surrounding structures. The lymph channels in the gland substance are much dilated and contain large, pale round cells of lymphocytic character. The endothelium of the channels is swollen and is in places desquamated."

This case conforms to the condition of which Dr. Sequeira has exhibited several cases under the heading of "Lymphoblastic Erythrodermia," in which there was generalized erythrodermia and a moderate lymphocytic leucocytosis.

In our view there seems to be no special reason for creating this new group, which might well be regarded as a variant of lymphatic leukæmia.

DISCUSSION.

Dr. PINEY remarked that from the hæmatological point of view, if one had done a count of the total red cells and leucocytes, one would have said it was not leukæmia. He found that in all the recorded cases of the kind there was a very low total white cell count, i.e., all the cases had been aleukæmic, and he suggested that the term lymphoblastic erythrodermia was incorrect, as the lymphoblast was of an immature character, whereas in the present case all the cells were mature, though small. He also suggested that the word "leukæmia" be dropped in this connexion, as the process was a proliferation of lymphatic tissue. One could divide lymphadenosis into leukæmic and aleukæmic varieties, characterized by hyperplasia of lymphatic tissue, and by an alteration in the composition of the blood

picture of a qualitative, not a quantitative variety. Therefore this condition should be regarded as aleukæmic lymphadenosis associated with a peculiar deposition of lymphocytes in the skin, causing erythrodermia.

Dr. PARKES WEBER said that this case helped to show that the condition which Dr. J. H. Sequeira and his colleague, Dr. Panton, had described under the heading, "Lymphoblastic Erythrodermia," was really a form of leukæmia. He understood that the man, aged 60, who died in 1923 in the London Hospital and who was claimed by Dr. Sequeira as an example of lymphoblastic erythrodermia, was found at the post-mortem examination to show leukæmic infiltration of the liver. The classes of leukæmic and the aleukæmic lymphadenosis mentioned by Dr. Piney were everywhere accepted, though not described in all text-books.

Case of Lupus Erythematosus and Arthritis treated by Saline Injections.

By H. MACCORMAC, C.B.E., M.D.

PATIENT, a married woman, aged 36.

History.—About fourteen years ago a condition of the joints developed, diagnosed as rheumatoid arthritis, not marked in degree, but very painful. The eruption on the face has been present during four years, beginning on the nose and spreading to the cheeks and ears. No history of tuberculosis, and no septic focus could be found in the teeth (mainly artificial), tonsils or elsewhere.

In these circumstances it was decided to attempt to get rid of the eruption by one of the non-specific de-sensitizing processes, and for this purpose intravenous injections of physiological saline were chosen. As in certain cases the efficacy of this method seems to depend upon the bacterial content of the saline, the solution was freely exposed to atmospheric contamination for some days before use. It was then sterilized and injected in quantities varying from 50 c.c. to 200 c.c. In all, six injections were given: after the first injection the joint-pain, which had been constant for many years, almost entirely disappeared, and in consequence the patient was for the first time for a considerable period able to sleep comfortably at night. The eruption on the face rapidly diminished, and this progress has been maintained, so that now, in comparison with the original state, the eruption is relatively slight. The injections were well tolerated and were without more than a slight amount of febrile reaction, except the last, which was followed by an attack of fever, which lasted over two days, the temperature rising to 101° F.

The special points of interest in this case appear to be: (1) The association of the lupus erythematosus with arthritis, pointing to some focal infection, and (2) the improvement of both conditions following the injection of a non-specific de-sensitizing agent, in this case "contaminated" physiological saline.

Case of Pityriasis Rubra Pilaris.

By A. WINKELRIED WILLIAMS, M.B.

PATIENT attended out-patient department, Royal Sussex County Hospital, on July 16, 1924.

History.—Condition first noted four months ago. Began on back of neck as rash and also as a widespread general pruritus.

July 16: Large areas of a scaly, dry, rather bright red erythema showing the skin markings somewhat exaggerated on arms, neck and back. These are distinctly marginated, but beyond the border are numerous acuminate papules, which extend

widely all over the arms, mostly with a horny plug at the top. Some are thickly grouped, some irregularly isolated. These papules are on the back of the fingers in groups.

There are septic stumps of molar teeth, and the gums are in a very bad septic state.

Treatment.—July 16: Tablets of parathyroid gland (Parke, Davis), $\frac{1}{20}$ gr. once daily.

July 23: Slight improvement.

July 29: Papules less prominent: spiny plugs less evident; sheet areas of redness softer, but skin markings more distinct. The last of the septic teeth removed this day.

September 2: Still further improvement.

October 7: Sheet areas quite pale; the site in parts just detectable on palpation by a feeling of slight roughness. Papules reduced in size and number.

The only local treatment applied was lotio calamine, with 15 minims liq. picis carbonis in an ounce.

The great and remarkably rapid involution in this case is, I think, the result of parathyroid administration combined with removal of septic foci from the jaws. It therefore seems evident that the malady originated in a septic infection.

Three Cases of Dermatitis due to the Internal Administration of Resin.

By W. F. R. Castle, M.D.

On October 13 the mother gave her youngest child (an infant not shown) some lump resin to relieve whooping cough. The child would not have it, but three older brothers, aged 7, 10 and 13, took it readily. To-day these three boys exhibit a diffuse erythema with puffiness under the eyes of varying degrees. Their urine does not contain any albumin or resin.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Lichen Planus confined to the Mouth.

By H. C. Samuel, L.R.C.P., M.R.C.S.

F. J., male, aged 45. I regard this case as one of lichen planus confined to the buccal mucous membrane and that of the hard palate and lips. The condition has been present in the patient over four years, and practically the whole of the hard palate (with the exception of the part which is covered by a plate), the buccal mucous membrane in its entirety, and the inside of the lip margins are affected. He has never had a lesion on the skin of the body; he has been many times examined for such. Wassermann negative.

DISCUSSION.

Dr. A. M. H. Gray agreed with the exhibitor's view. He did not think it was very rare to see lesions of lichen planus confined to the mouth. Not long ago he himself showed a case of very severe lichen planus in the mouth and without any lesions of it elsewhere.

Dr. H. C. Semon referred to a case he showed two years ago, in which the eruption was limited to a small brooch-like lesion on the glans penis. It had been diagnosed as a tertiary syphilide, as the Wassermann was positive. He did not see it at the time, but, in his absence, it was diagnosed tinea at the hospital, the patient ordered X-ray treatment, and immediately the lesion involuted. It relapsed from time to time, but a small dose of X-rays always cleared it up. He did not doubt it was lichen planus; no lesions of that disease had appeared elsewhere in this case.

Dr. Graham Little (President) remarked that Dubreuilh had said that in his experience lichen planus of the mouth without lesions on the skin was commoner than lichen planus on the skin without lesions of it in the mouth, a remarkable inversion of the usual experience. He (the speaker) mentioned that statement in a paper he wrote on lichen planus, and Dubreuilh had written to him stating that he found he had made the statement, but he had now changed his opinion about it. He (the President) could remember three cases in which the lichen planus was entirely confined to the mouth mucosa, with no lesion elsewhere.

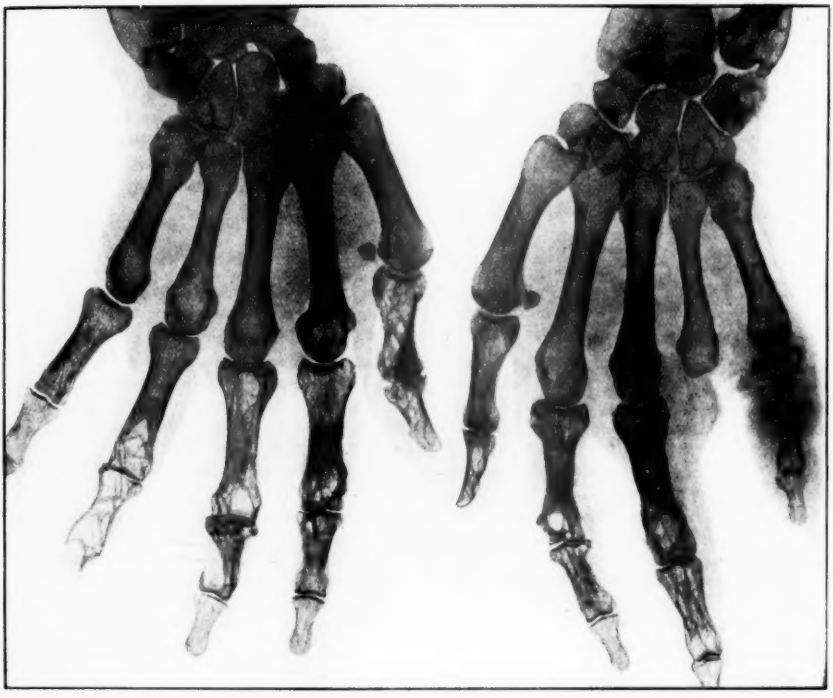
Case of Lupus Pernio (Sarcoid—Schaumann Type) associated with ? Lupus Vulgaris.

By H. W. Barber, M.B.

D. H., aged 53, single, nurse. This case is one of very great interest and importance, as it provides strong evidence in favour of Schaumann's view that lupus pernio, and one form of sarcoid at least, are due to tuberculous infection (probably bovine tuberculosis). I first saw the patient in August, 1921, for a single lesion on the right side of the forehead; she had previously seen Dr. Leslie Roberts, who had diagnosed it as tuberculous, and had applied some caustic, which caused ulceration and so altered its appearance that I was unable to make a certain diagnosis at the time. The whole lesion was excised by Mr. Davies-Colley, and was

sent to Dr. Nicholson for section, who reported that the structure was typically tuberculous. In the same year the patient damaged two of her fingers, ring and little finger of left hand, in a door, and fractured both. The ring finger refused to heal and was amputated.

I did not see the patient again until the 24th of last month, when she was admitted to Guy's under Dr. Beddard in the condition you have now seen. She gave a further history to the effect that about a year ago the little finger of the left hand became very swollen, and she was admitted to the Royal Infirmary, Hull, August 21, 1923, with a diagnosis of ? tuberculous dactylitis. The finger was incised to the



Case of lupus pernio (sarcoid– Schaumann type—) associated with lupus vulgaris.

bone, which was curetted, and swabbings were sent to the pathologist, who reported: "This is active tuberculous granulation tissue." The site of operation is well seen in the X-ray photographs, the greater part of the first phalanx having disappeared. After leaving the infirmary, there appeared for the first time the patch on the bridge of the nose, which strongly resembles *lupus vulgaris*, and the question arises whether, as a result of the operation, tubercle bacilli were liberated into the blood stream and produced this patch. From Christmas of last year (1923) for a period of three months she was given injections of tuberculin, but they seem to have done harm rather than good.

Condition at present.—She now presents the following lesions:—

(1) Typical *lupus pernio* of the backs of the hands and fingers (*spina ventosa*).

(2) In both forearms and upper arms nodules and indurated plaques can be felt, some at least appearing to follow the lines of the lymphatics. Some of the nodules are small and superficial, others deep, and a bluish coloration is seen over some.

(3) At the site of the lesion which was excised in 1921, there is an irregular scar covered by a crust, and there has evidently been recurrence of the inflammatory process at the periphery.

(4) On the bridge of the nose is a patch, in which nodules like those of lupus vulgaris are clearly seen.

Dr. Beddard could find no evidence of active tuberculosis of the *lungs*: a radiogram of the chest did not show the deposits described by Schaumann, and seen by me in another case of superficial sarcoid. *Radiograms of the hands* (shown) and of the *feet* show the typical clear areas seen in these cases. It will be observed that almost complete rarefaction has occurred in some of the phalanges. The clear areas are also well seen at the lower end of the ulna, a single one in the radius above the styloid process, and in the pisiform bone. *The lymphatic glands* accessible to palpation are not obviously enlarged.

The spleen, I think, can be felt, but Dr. Beddard doubts this. *There has been no iritis.*

Investigations.—Wassermann reaction negative. Complement-fixation test to tubercle negative. Von Pirquet reactions to human and bovine tuberculin negative. A section of a small nodule excised from the arm shows more or less circumscribed areas of tuberculoid structure with giant cells and epithelioid cells. A nodule has been excised by Professor Eyre and inoculated into guinea-pigs.

Dr. JÖRGEN SCHAUMANN (Stockholm) said that the case of lupus pernio shown by Dr. Barber was of great interest as regarded the question whether true lupus nodules were present or not. As he had already stated in the paper he read before the British Association of Dermatology and Syphilology, in July that year (1924), he did not believe in the possibility of the co-existence of lupus pernio and lupus vulgaris. It was true that when benign lymphogranuloma invaded the papillary body, it often assumed an appearance similar to that of lupus vulgaris, but the nodules were neither so soft nor so transparent; Dr. Barber had himself pointed out in the present case, that, in these respects, the nodules were different from those of lupus vulgaris. If the microscopic lesions showed themselves in the form of tubercles edged with lymphocytes, the histological examination could not definitely decide the differential diagnosis. If, on the other hand, the epithelioid tubercles were entirely bare, without any trace of a lymphoid zone, a lupus pernio must be supposed. In order to settle the diagnosis between lupus pernio and lupus vulgaris, the result of the inoculation already made on the guinea-pig must be awaited. Apart from the tuberculin cuti-reaction as practised by Dr. Barber, he (Dr. Schaumann) thought it would be advisable to carry out a series of subcutaneous injections of tuberculin in large doses. Personally, he was, he said, convinced that such injections, as well as the inoculation of the guinea-pig, would give negative results.

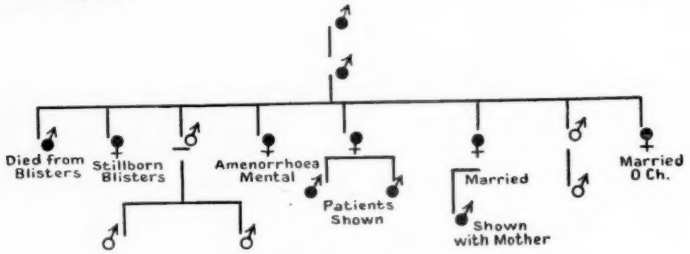
Epidermolysis Bullosa (Five Cases).

By W. F. R. CASTLE, M.D.

I SHOWED here two years ago one mother and her child with this condition.[1] Since then the mother has had another baby, and that also is affected. To-day she comes with her sister, who is also affected, and her child, who shows the lesions too. The condition can be traced back through four generations (see chart). Of eight children in the third generation, only the third and seventh have escaped the disease.

[1] *Proceedings*, 1922-23, xvi (Sect. Derm.), p. 53.

One of the mothers gave a strongly positive Wassermann reaction, and was treated as for syphilis. At present her Wassermann is negative. The blood tests of both her children are negative



EPIDERMOLYSIS BULLOSA.

Chart showing cases in four generations. Persons affected in *black*.

Dr. HALDIN DAVIS said that he had had a case of severe epidermolysis bullosa, a boy who, after many vicissitudes, was now employed in the Ministry of Pensions. He had tried treating him with hæmoplastin, and at first the patient improved a good deal, but ultimately relapsed; and it seemed probable that the improvement was not due to the hæmoplastin but occurred in the natural fluctuation of the disease. He had not seen the patient recently.

Case of Bilateral Symmetrical Atrophy. Pick's Érythromélie.

By W. F. R. CASTLE, M.D.

PATIENT, a male, aged 42, gas worker. The condition commenced in the left hand fourteen years ago. The right hand became gradually affected two or three years later, and the legs became affected about the same time. The arms are affected as far as the shoulders, and the legs as far as the groins. The skin is atrophied, shiny and wrinkled, varying in shade from a bright red to a violaceous colour. In many areas the subcutaneous fat appears to be absent. The hairs on the limbs are normal, as are also the nails. The patient complains that his left hand is extremely susceptible to painful stimuli, this tenderness being apparently in the bones. He complains also of stiffness of the toes and thumbs. There are no other subjective symptoms, and the patient can do his work, which is of an arduous character. The physical condition of the patient is apparently normal, except that his teeth are septic. The Wassermann reaction and blood count are normal.

This I believe to be a case of Pick's érythromélie, as described in the *Archiv für Dermatologie u. Syphilis*, 1900.[1] In its objective and pathological signs, it has a slightly suggestive relationship to erythromelalgia, although it is quite a distinct condition. It is perhaps a borderline case between the two conditions, as, although the burning and swelling of erythromelalgia are absent, yet the pain is present. This pain, however, is not typical of erythromelalgia, in which disease it follows the venous engorgement and throbbing; in this case, it is neither of a paroxysmal nor of a burning character, but it consists rather in a general tenderness of the bones, especially of the left hand. This case corresponds with Pick's original description in that the redness, atrophy and lividity are more marked at the periphery of the limbs, gradually fading at the groin and shoulder into normal skin. The disease is apparently of extreme rarity in this country, and Professor Pautrier,[2] in

[1] "Ueber Erythromelie," *Arch. f. Derm. u. Syph.*, 1900, Ergänzungsband, p. 915.

[2] "Contribution à l'étude de la dermatite chronique atrophique," *Ann. de Dermat.*, 1921, 6me sér., ii, p. 242.

describing a case, states that it is very rarely met with west of Strasburg, but that its incidence increases further east. It is fairly common also in Sweden. This patient has lived in Gloucester all his life. Herxheimer and Hartmann describe a few cases, and observe that the condition is more frequently found in the arms than in the legs; that pain is generally absent except that due to cold; that men are more frequently affected than women. Klingmüller and Rille have observed a progressive atrophy, whilst others have noted a partial disappearance of the condition.

Pathologically, the disease is stated by Professor Pautrier to be due to the dilatation of surface capillaries and obliteration by infiltration of the deeper blood-vessels.

Dr. F. PARKES WEBER did not doubt this was an example of acrodermatitis atrophicans or érythromélie. He took it that the inflammatory-looking patches represented an early active swollen stage of the later atrophic lesions. He remembered a case of this rare condition shown many years ago, under a different name, by Sir Malcolm Morris.[1]

A Case of late Congenital Syphilis of the Face.

By GEORGE PERNET, M.D.

THIS boy, aged 16, came with a palm-sized exuberant area of ulceration on the side of the face, which had begun as three pimples near the left ear two years ago. The facial aspect of the boy made me suspect syphilis, and I diagnosed late congenital syphilis and gave him iodide of potassium. In a week his state was much improved. The area has become smaller owing to contraction of the scar, which is rather hypertrophic. The Wassermann was positive.

Granulomatous Lesions of the Legs.

By A. M. H. GRAY, C.B.E., M.D.

THIS patient, a woman, aged 59, first developed a spot on the left shin in September, 1923. It increased in size and eventually discharged, apparently a little serous fluid. Subsequently other lesions developed round it until this lumpy swelling, occupying the front and inner side of the lower third of the left leg, was formed. Last July she developed two similar nodules on the other leg, which I excised, and I show a section of one of them under the microscope; it is the most recent of the two, and it is starting deep down in the hypoderm, and the skin itself has hardly become affected at all. It has a typical granulomatous structure, and there are a fair number of giant cells present. Both the Wassermann and von Pirquet reactions are negative. I have had some of the tissue removed inoculated into guinea-pigs, but with negative results. Repeated cultures from the lesions have failed to yield any growth but staphylococci.

When I first saw the patient I thought it might be a bromide eruption, but the section shown enables us to exclude that. I have had her in hospital for some months, and we know she cannot have had bromide during that time. I then thought it was possibly sporotrichosis or blastomycosis, but I have failed to grow any fungus at all,

[1] Sir Malcolm Morris, "Case of Spreading Telangiectases of the Feet and Legs," *Proc. Roy. Soc. Med.* (Sect. of Derm.), 1908, i, p. 89: A man, aged 32, whose illness had commenced seven-and-a-half years previously, with red patches on the outer side of his feet.

or to find any yeast or fungus in the pus. I gave her potassium iodide at one time, but she tolerated it badly, and she is now taking tincture of iodine, 120 minims a day, and there is no doubt the lesions are slowly drying up, which suggests that it is a fungus infection.

Note.—Since the case was shown the complement-fixation test for sporothrix has been done, but with negative results. The Wassermann reaction has been done twice again, once after a provocative injection of novarsenobillon, with negative results. The novarsenobillon, which has been given on three occasions, has had no influence on the lesions.

Case of Macular Non-anæsthetic Leprosy.

By W. J. O'Donovan, M.D.

Patient, a male, aged 55, was sent to me by Dr. Kissmeyer, of Copenhagen. He was twenty-one years in the East, two of the years being spent in China and in Japan. His health is good. For two years irritating red areas had appeared on the wrists and thighs. He now has blackish, oval, pigmented areas of otherwise unaltered skin in those areas [*cf.* Plate viii, MacLeod's "Diseases of the Skin," 1920]. The Wassermann reaction is negative, there is no abnormality in the blood-count and no improvement after salvarsan therapy.

Case of Erythema Circinatum.

By A. M. H. Gray, C.B.E., M.D.

Patient, a female, aged 31, who has a ringed eruption above the left elbow. She says she has had these lesions for four years. They usually last four or five days, and come on regularly a day or so before the commencement of the menstrual period. She has had them on both elbows; that on the right elbow, which is much smaller than the one on the left, has been removed for microscopical examination. During the last year the lesions have been more frequent, and have come on at every period. The lesion now seen has a distinctly nodular edge, like that in granuloma annulare, and the centre is flat and shows marked purplish staining. I thought it was probably a case of circinate erythema of the type which has been described by Graham Little and others. It has some resemblances to some forms of granuloma annulare, but its rapid appearance and disappearance is against this diagnosis.

Dr. Graham Little (President) said he regarded the condition as similar to that in a case he himself showed ten years ago, and there was a similar history. In that patient's case the disease lasted six years. He had seen her since, and there had been no return of it.

Nævus with Congenital Atrophic Lesions.

By A. M. H. Gray, C.B.E., M.D.

Patient, a girl, aged 11. There are two types of lesion, and both types were noticed at birth. What interested me most were the atrophic lesions, of which she has three. The largest is on the skin over the fifth metatarsal bone of the right foot and it is 2 in. by 1 in. in size. The skin is atrophic, of a whitish-yellow tint, and the vessels can be clearly seen. Where the follicles are situated the skin is studded over with pits, as in keratodermia punctata. There is another lesion over the lower end of the left radius, 1 in. by $\frac{1}{2}$ in. in size. There is a series of small lesions, not larger than a pin head, and grouped closely so as to form a patch on the outer side of the right arm just above the elbow.

The mother is sure they were present at birth, and there is no question of there having been an injury at birth.

The other lesions consist of several small tumours of the mucous membrane of the lips. The largest ones, on the lower lip, are about the size of a lentil. There is one just inside the right angle of the mouth. There are a few very small ones on the upper lip, and they also have been present ever since birth.

There is a definite family history of tumours of the scalp. I have seen this child's father, and he has two lesions looking like sebaceous cysts. Two aunts and a grandmother had similar lesions. I wondered whether they were cases of cystic benign basal-celled epithelioma, such as occur on the scalp, and therefore whether these lesions on the child's lip might be benign cystic epitheliomata. I do not think these have been described as occurring in connexion with mucous membrane. They are probably, however, of the nature of ordinary pigmentary nævi.

Dr. Parkes Weber said one might go by analogy in regard to the classification of the atrophic lesions in this case. Certain hyperplastic lesions of the dermis were certainly of congenital (nævoid) origin; for instance, the patches of congenitally thickened skin on the trunk sometimes associated with adenoma sebaceum (an admittedly nævoid condition) of the face. There seemed to be no reason why congenital hypoplastic or aplastic nævoid patches in the dermis should not likewise occasionally occur, as apparently they actually did in the present case.

Case of Erythema Circinatum.

By E. G. Graham Little M.D. (President).

Patient, a female, aged about 30, with ringed erythematous lesions on the anterior surface of both thighs. She had an exactly similar eruption last winter. It then disappeared, and reappeared last month. They begin in the same way: there is a rose spot, which enlarges centrifugally, remains two or three weeks, and then disappears, without leaving any trace. Their behaviour makes them rather like a group of cases described by Darier as erythème centrifuge annulaire. I think this is the second case of that type that I have had.

Case of Urticaria Pigmentosa without Mast Cells.

By E. G. Graham Little, M.D. (President).

I think this is almost certainly urticaria pigmentosa in an adult; but it is remarkable because of the extent of the eruption, especially on the trunk. In cases of it I have seen recently in adults it has been almost confined to the extremities, but here the distribution is more on the trunk. There are no mast cells in the section, and I have in the past been dubious about the diagnosis in their absence; but I am being converted to the view that it can exist without mast cells, because the clinical appearance in this case is very typical.

Case of Erythema Nodosum.

By E. G. Graham Little, M.D. (President).

This patient, a married woman of about 35, scratched her left index finger on August 25, and acquired an immediate suppurative lesion, with diffuse cellulitis of the whole arm. Apparently it subsided without operation, but on the arm which was affected a number of indurated areas are left. Exactly similar areas of induration of both legs followed. The diagnosis, from the leg condition, would be erythema nodosum; but the history of septic infection followed by patches of eruption, indistinguishable from erythema nodosum, renders it a remakable case. Some theories of erythema nodosum include streptococcal infection as a cause, and this case seems to support that view.

Case of Leprosy.

By S. E. Dore, M.D.

Patient, a female, aged 26, was born in Alexandria of a French mother and Egyptian father. There is no history of leprosy in her family. She married an Englishman (a soldier now in India) when she was aged 17, and has two children, aged 9 and 7, said to be healthy. The patient is a dancer by profession, and has lived in Egypt, India and France, and has been in England for the last ten months. At the age of 9 she entered a convent in India, where she remained until she was 15. She states that her disease began in her 13th or 14th year at Darjeeling, when she visited a leper cemetery and played with skulls which were lying on the ground. She had a whitlow on her finger at the time, and according to her statement this finger became anæsthetic within a few days of the supposed infection. The next symptom noticed was an eruption of circular red patches on her arms, and when in Darjeeling she states that she had an operation for "neuritis," an incision being made along the inner side of the biceps (scar now visible) and the nerve divided. There is now anæsthesia of both arms and legs. The fingers and toes are shrivelled and contracted. Both index fingers are missing, having been removed in 1920. The hands are scarred, and parts of the other fingers are absent. The feet are swollen and the toes contracted. Radiograms show extensive absorption of the metatarsals and phalanges, as in syringomyelia. On the soles of the feet are several trophic ulcers, one of a perforating type; and although these are

not painful, she experiences considerable difficulty in walking. There are several circinate, thickened patches on her neck and arms. The Wassermann reaction is negative, and bacteriological examination of the nasal mucus is negative also. She was treated for many years by Dr. Muir and Sir Leonard Rogers in Calcutta by intravenous and intramuscular injections, and is now, in accordance with a suggestion by Sir Leonard Rogers, having injections of sodium morrhuate and gynocardate of sodium pills.

Case of Erythrodermia.

Shown by W. J. O'DONONAN, M.D., for J. H. SEQUEIRA, M.D.

THE patient, a man, aged 49, a horsekeeper, was first seen at the London Hospital in 1919 with what was diagnosed as pityriasis capitis. Since that date, he states, the eruption, which began on the scalp, spread over the whole body. He has been under treatment at St. Andrew's Hospital since 1920 and was sent by Dr. Bray to the London Hospital.

The whole of the integument is red, scaling and intensely pruritic. On the lower part of the trunk there is some pigmentation, producing a dappled appearance. The hair has been shed for two years, and the finger- and toe-nails are lifted up by thick, soft, scaly masses under their free ends. There is a mass of enlarged, discrete glands in the right groin, and two flat, ovoid swellings on the skin of the right flank. Neither liver nor spleen is palpably enlarged. The Wassermann reaction is negative. There is no history of salvarsan-therapy. The blood counts are the following:—

	October 7, 1924	October 12, 1924
Polynuclear neutrophils	49·5 per cent.	44·5 per cent.
,, eosinophils	1·0 ,, ,,	2·0 ,, ,,
Small lymphocytes	39·0 ,, ,,	41·5 ,, ,,
Large ,,	4·0 ,, ,,	2·0 ,, ,,
Large hyaline cells	5·5 ,, ,,	9·0 ,, ,,
Coarsely granular basophilic cells	1·0 ,, ,,	1·0 ,, ,,
	100·0 per cent.	100·0 per cent.

Further investigations will be made, and the glandular tumour mass has been excised for microscopical investigation. A portion of thickened skin already examined shows a granulomatous condition, but it is not characteristic of mycosis fungoides. There has as yet been no improvement under X-ray treatment.

Dr. J. E. M. WIGLEY asked if the presence of the apparently mycotic tumour was the final deciding point against the case being of the same type as the one he showed with Dr. MacLeod at the last meeting under the title of "Lymphatic Leukæmia with Erythroderma." The blood-count of that case was, at present, almost identical with that of Dr. Sequeira's case. In this case there was also a generalized enlargement of the lymphatic glands. Was it usual, in mycosis fungoides, to find a generalized enlargement of lymphatic glands?

Postscript.—Two further blood-counts have been made as follows:—

	December 15, 1924	December 29, 1924
Polynuclear neutrophils	62·5 per cent.	72·0 per cent.
,, eosinophils	2·5 ,, ,,	3·0 ,, ,,
Small lymphocytes	23·5 ,, ,,	20·0 ,, ,,
Large ,,	4·0 ,, ,,	—
Large hyaline cells	5·5 ,, ,,	5·0 ,, ,,
Coarsely granular basophilic cells	2·0 ,, ,,	—
	100·0 per cent.	100·0 per cent.

By January 8, 1925, the patient's condition had greatly improved. The skin all over was more supple and of a much fainter colour, in addition to which the itching had abated and there was a profuse growth of fine hair over the scalp. There was

no fresh adenopathy nor were there any masses suggestive of mycosis fungoides, and from his non-improvement under X-rays it is almost possible to negative a diagnosis of pre-mycosic erythrodermia, while the clinical and hæmatological improvement make it difficult to allot the case to the group of lymphatic leukæmias.

Case of Lupus.

By G. B. Dowling, M.D.

Patient, a female, aged 58, has a nodule the size of a small hazel-nut on the right buttock. It began as a small pimple, and has gradually increased in size. It is somewhat soft and reddish-brown in colour. The section shows that it is lupus, although there is no tendency to cicatrization and healing.

Case of Granuloma Annulare.

By G. B. Dowling, M.D.

Patient, a little girl, aged 5, who has had these lesions five months. There is one ringed lesion on the ankle, and three on the fingers, over the proximal interphalangeal joints. One is the size of a pea, and the others are slightly larger. I think such a wide distribution of granuloma annulare is unusual.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Schamberg's Disease.

By A. C. Roxburgh, M.D.

Patient, a boy, aged 12. Lesions present six months. Between knees and ankles only. Both legs. Original patches 1½ by 3 in. and 1½ by 1½ in., now fading in centres. Younger patches have outlying "cayenne pepper" pigment macules. Section shows epidermis thinned in places and basal layer broken up, with invasion of fine fibrous tissue and round-celled infiltration. Similar infiltration immediately beneath epidermis, also about hair follicles and blood-vessels, not around sweat glands. Pigment not visible.

A three-quarter pastille dose unfiltered X-rays applied to one patch two weeks ago. Little visible result.

Nothing in this case to suggest connexion with varicose veins or angioma serpiginosum.

I should very much like to know if anybody can suggest treatment to expedite the disappearance of the lesions.

Case for Diagnosis.

By E. G. Graham Little, M.D. (President).

The patient has lived in Burma since 1905. He has had malaria frequently, once of malignant type. The present eruption commenced last March with an attack of fever, 102° three or four days, after being bitten by a native fly, the bite of which causes blood-blisters. These feverish attacks recurred at intervals of ten days, accompanied by headache, diarrhœa, great lassitude and nausea. With each such attack a notable spread of the eruption was observed, until at the present time there is a nearly continuous sheet of eruption over the lower extremities. The eruption consists of punctate lesions rather like those of Schamberg's disease. Deep pigmentation ensued later, irrespective of arsenical medication. The eruption persists without involution.

Discussion.—Dr. G. Pernet said he considered this was a toxic purpuric condition common enough about the legs. Pressure on the skin of the left leg did not cause the lesions to disappear. Purpuric conditions were apt to be very persistent and leave a good deal of discoloration behind.

Dr. W. Dyson agreed with Dr. Pernet that the lesions were purpuric. The bites of the flies and the insanitary surroundings in a tropical country suggested that there was probably some septic focus causing the lesions.

Case of Tinea.

By H. C. Semon, M.D.

Patient, a lady, aged 52, contracted the infection from a dog, in October, 1923. When seen on November 11, the lesions consisted of an exfoliating plaque embracing the dorsal aspect of the left index finger, and spreading thence to the interdigital spaces on either side. From the small peripheral vesicles Dr. Embleton succeeded in demonstrating the mycelium and cultivating a trichophyton fungus; this he also

cultivated from some ill-defined erythematous papules on the left cheek and columella nasi.

The points of interest in the case are :—

(1) Failure to demonstrate the growth in potash under a one-sixth objective. Immediate success on staining with Gram, and examining with the oil-immersion one-twelfth objective. (2) Unusual type of eruption on face. (3) Extreme obstinacy to all the usual forms of treatment. The case was eventually cured by daily inunctions of a 40 per cent. salicylic acid.

Dr. J. A. DRAKE suggested that Dr. Semon should try a fairly strong solution of permanganate of potash. He had seen that remove obstinate tinea of the foot.

Case of Periarticular Fibromata of the Skin.

By J. M. H. MACLEOD, M.D., and J. E. M. WIGLEY, M.B.

PATIENT, a woman, aged 31, presented painless nodules on several fingers of both hands, which first appeared 2½ years previously, situated at the sides of the fingers near the distal interphalangeal joints. They were about the size of a split pea, cartilaginous to the feel, the skin over them being unaffected. When pressed they presented a white, almost chalky appearance. They were freely movable, and unconnected with the joints. A radiogram of the affected fingers showed no signs of joint or bone changes. According to the patient's statement, two of the nodules had disappeared as the result of being knocked, and, before they went, a whitish discharge, sometimes tinged with blood, could be expressed from them.

No history of rheumatism.

The lesions appeared to correspond to what Savatard has named "periarticular fibromata of the skin," and upon microscopical examination they proved to be small fibromata.

Possibly they are related to the lesions described as "synovial lesions of the skin."

Dr. W. DYSON said he had had a somewhat similar case, except that the lesion was on the dorsum of the finger, and was single. It felt to him to be slightly fluctuant. He pricked it and there exuded from it some glairy fluid, which was translucent and gelatinous. The patient feared it was cancer, and asked to have it examined with X-rays and the rays applied to it. After exposure to X-rays the lesions disappeared. Apparently the condition was not connected with the joint.

Erythema Multiforme associated with Post-scarlatinal Nephritis.

By J. E. M. WIGLEY, M.B.

PATIENT, an anæmic, nervous boy, aged 10½. Eruption present two months.

Eruption on nose, malar prominences, ears, skin behind ears, backs of hands and fingers, and front of knees.

Lesions on face and ears are persistent scaling erythemas suggestive of lupus erythematosus. No atrophy. Those on hands are purplish-red macules of erythema multiforme. Acro-asphyxia evident. Some itching.

Scarlet fever two years ago, complicated by hæmorrhagic nephritis. Now urine contains much albumin, casts, &c.

Condition described by Duhring as "multiform erythema in association with nephritis."

The similarity of the lesions, especially on the face, to lupus erythematosus is in accordance with a case reported by the late Sir J. Galloway and by Dr. MacLeod in 1903, when they drew attention to the possibility of a close resemblance between lupus erythematosus and erythema multiforme.

Dr. G. PERNET said that in his monograph on acute primary lupus erythematosus he had mentioned a case of that type,[1] which had given rise to much discussion in Berlin as to whether it was one of erythema multiforme (erythema exudativum multiforme), or of acute lupus erythematosus. In his (Dr. Pernet's) own case of acute primary lupus erythematosus, erythema, iris-like elements occurred on the palms (op. cit. infra, p. 24). He did not mean by these observations that Dr. Wigley's case came into the same category.

Case of Purpura Annularis Telangiectodes.

By S. E. DORE, M.D.

THE exhibitor, in describing this case, said he believed it to be the first of the kind shown in this country.[2]

Discussion.—Dr. H. C. SEMON asked whether this patient had any cardiac lesion. He said he showed a case four years ago, to which he gave the same name, and that patient had definite cardiac lesions, an association which had been described by other writers.

Dr. DORE (in reply) said that there were no cardiac lesions present. In July Dr. Whitfield had suggested that the condition might be embolic, but the patient had been examined by Dr. Tidy at St. Thomas's Hospital, and no cardiac or other visceral disease had been found.

Case of Lichen Plano-pilaris.

By S. E. DORE, M.D.

PATIENT, a girl, aged 14; the condition is of six months' duration.

Discussion.—Dr. GRAHAM LITTLE (President) said that the cases to which the late Dr. Pringle gave the name lichen plano-pilaris were those which had typical lichen planus lesions and the grouped lesions which were called lichen spinulosus. He rather questioned the identification of this present case with Dr. Pringle's group. He regarded these as lichen planus with acuminate lesions.

Dr. MACLEOD said he showed a case much like the present one in a child of the same age, in Dr. Pringle's presence, and Dr. Pringle said it was lichen plano-pilaris.

Case of Poradéno-lymphite or Lymphogranulomatose Inguinale Subaiguë.

By J. E. R. MCDONAGH, F.R.C.S.

SYPHILIS, 1910. Granulating, primary sore skin of penis. No treatment and no recurrence since. Multiple suppurating foci in granulomatous masses appeared in left groin in 1912. They persisted till 1918, when they were excised, having resisted all other treatment. In 1919 a similar condition affected the right groin. Patient now has large granulomatous masses which are perforated by suppurating channels. There is elephantiasis of the scrotum. Treatment by drugs has been unavailing. Nothing else abnormal found, excepting the following blood-picture (relative lymphocytosis): Polymorphs, 41·5 per cent.; small lymphocytes, 51 per cent.; large lymphocytes, 7 per cent.; basophils, 0·5 per cent.

Discussion.—Dr. SEMON asked whether Mr. McDonagh had considered the possibility of a malarial super-infection doing any good in such a case.

Dr. MACLEOD asked whether the exhibitor thought this a general infection with a local manifestation. It did not seem advisable to use the above nomenclature unless one knew it was connected with Hodgkin's disease. This seemed to be an inflammatory disturbance, and it might be due to some local infection.

[1] Pernet: "Le Lupus érythémateux aigu d'emblée," Paris, 1908, Koch's case, pp. 55 *et seq.*, also "Remarks on Differential Diagnosis," pp. 102 *et seq.*

[2] A full account of this case will be given in the *British Journal of Dermatology and Syphilis*.

Mr. McDonagh (in reply) said that he had considered the treatment of this case by means of shock-therapy or of the induction of malaria, but thought the simpler method of X-rays would probably suffice. He thought the case belonged to the lymphocytomata and that the lesion presented was only one of the signs of a general condition which might be of varied ætiology. The diagnosis of ulcus molle serpiginosum suggested by Dr. O'Donovan could be clearly ruled out, as this case in no way resembled that condition clinically and it had proved resistant to large doses of iodine and injections of tartar emetic.

A Case of Porokeratosis of the Leg.

By George Pernet, M.D.

A man, aged 58. In outline the patch is extraordinarily like the case of porokeratosis of the hand of Grover Wende (Buffalo) figured in Sutton's fifth edition of "Diseases of the Skin" (page 485, fig. 384).

Discussion.—Dr. H. C. Semon said that in 1917 he had seen a case of porokeratosis (Mibelli) which in its clinical features certainly resembled the case now shown by Dr. Pernet. It had spread in the course of years from the back of the hand to the shoulder, leaving superficial atrophy in its trail, and not reacting to any of the remedies that were prescribed for it by more than one dermatologist.

Dr. Graham Little (President) said he hoped that a section from the case would be forthcoming, as he felt doubtful about it, and he did not think a diagnosis could be made in the absence of a section. It seemed to him a case of flat rodent ulceration.

A Case of Melanotic Growth of the Temple exhibited to show Result of Excision and Grafting.

By George Pernet, M.D., and Neil Sinclair, F.R.C.S.

Showing condition before operation.

Patient, an engine driver, aged 60. Duration, twenty years. When first seen, two and a half years ago, the growth was very black and irregularly starred in shape about left temple. The growth was widely excised by Mr. Neil Sinclair followed by skin-grafting.

Extensive Vascular Nævus of Face, treated by Radium.

By R. H. BROWNE-CARTHEW, M.D.

THE nævus, being so extensive, affords a good opportunity of observing whether the contraction of the vessels which has been caused by the application of radium will continue further, and whether it will be permanent.

Discussion.—Dr. H. G. ADAMSON said he did not recommend treatment of such nævi by radium. The result of radium treatment of "port wine" nævi might be apparently quite good for a year or two, but in his experience it frequently happened that the good cosmetic result was afterwards marred by the appearance of telangiectases. It was exceedingly difficult if not impossible to judge exactly the dose which would destroy the vessels of the nævi without leading to subsequent telangiectases in the scar.

Dr. S. E. DORE said that in order to get results from radium treatment in these cases, it was necessary to obtain destruction of the superficial tissues, and although the primary result might be good, there was a risk of chronic dermatitis and atrophy ensuing later, as in the treatment of hypertrichosis by X-rays. He thought that treatment with a small plaque of radium was very tedious, and better results could be obtained by light stippling with the galvano-cautery.

Dr. W. J. O'DONOVAN said he had seen another complication which would make him chary of using radium in this manner if a patient was likely to live some years. This had occurred in a girl who was photographed and treated at London Hospital eleven years ago for a similar nævus to that now shown. Early this year she returned with an ordinary impetiginous scab on the scar, and it took four months to heal. Tissues subject to radium were devitalized for years to come, and in a young person such a therapy should be avoided if other means of treatment were available.

Dr. W. DYSON said he had had a fair experience of treating such nævi with radium, and good results could be obtained; in all cases the appearance was improved. It was necessary to give small doses, at long intervals, and extending over a considerable time—two or three years. Distinct paling was produced, and he had not seen ill-effects from the treatment. No doubt radium exercised a greater selective action on blood-vessels than did X-rays.

Dr. BROWNE-CARTHEW (in reply) said that in using radium in these cases it was necessary to proceed cautiously in order to avoid the danger of tissue destruction. Devitalization of the tissue from too prolonged an application might certainly have an unwished-for result. But that also applied to the application of CO_2 snow. This particular nævus had been treated in a London hospital with CO_2 snow. The snow had had a destructive effect, with the result that the skin was slightly scarred in two places. The application of radium could be more definitely regulated than that of CO_2 snow. He had used in this case 50 mgm. of radium bromide, equal to about 37 mgm. of pure radium. He had found the "cosmetic" results obtained by radium to be good. Eighteen months ago, at a meeting of the Clinical Section, he had shown a case of rodent ulcer of the face, $1\frac{3}{4}$ in. across, and involving the ala of the nose. It had healed up in a very satisfactory way. Now, three years after treatment, it was not easy to detect where the ulcer had been. Only on a very cold day did the place show up at all, being then only very slightly paler than the surrounding parts.

Case of Pityriasis Lichenoides Chronica.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT, a policeman, in whom an eruption very similar to the one above described has been noted, commencing within the last six weeks and spreading over the legs, thighs and buttocks. There is slight itching, but he is otherwise unaffected by it.

Case of Post-salvarsan Melanodermia.

By W. J. O'DONOVAN, M.D.

THIRTY years ago this man had a sore on his penis. Recently he discovered that the Wassermann reaction was positive. He was given eight injections of 0·6 grm.

of novarsenobillon, and an eruption which was regarded as eczema developed. He continued under the same treatment, and after three other injections he came to my clinic with a moist eruption, which was scaling freely. In hospital he improved slowly without blood changes, but as the scales cleared off there was this dappled appearance of small and large white spots with black pigmented background. Face and mucous membranes are free. On the back of his head was a small bald area. The blackness is diminishing, leaving larger white surfaces. It has been suggested to me that there was a former condition of vitiligo. I think the pigmentation is an arsenical phenomenon, and that there is no need to postulate a coincident disease to explain its distribution.

Discussion.—Dr. F. PARKES WEBER thought that the patient must have had vitiligo (whether he was aware of it or not) before the arsenobenzol treatment was given. The dermatitis that followed the treatment had doubtless increased the pigmentation of the skin, excepting in the areas of vitiliginous leucodermia. This doubtless first became obvious when the dermatitis subsided. The patches at the back of the man's hairy scalp, which resembled patches of alopecia areata, were only part of the general vitiligo. The leucodermatous patches over the lower part of the back, with their somewhat symmetrical distribution and curved outlines, were too large for any other kind of leucodermia than vitiligo. Time would finally settle the question.

Dr. ADAMSON said he regarded this case as a leucodermia in which the leucodermic areas had become more noticeable in contrast to an arsenical pigmentation of the normal skin. The leucodermia might have been previously unobserved by the patient, just as in other cases the leucodermia was sometimes not noticed until the patient became sunburnt, and the white areas showed up in contrast with the sunburnt normal skin.

Dr. DYSON said he wondered whether the white patches became densely pigmented like the rest of the skin. Leucodermia was associated with a loss of power to form pigment in that part. If the white patches did not become pigmented at the same time as the rest of the skin, it was proof that the patient was suffering from vitiligo.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Epithelioma Adenoides Cysticum.

By F. Carminow Doble, L.R.C.P., M.R.C.S.

This is a somewhat unusual case. The condition did not start until the patient was aged 23. There is no family history of such a disease. He does not appear to have suffered from the lesions more than two years. Colonel Perry kindly cut sections from the growth for me. They show the condition typically.

Dr. A. Whitfield said he did not diagnose the condition when he saw the patient, and someone else was in the same position. It must be a very rare disease indeed, as he had had charge of an out-patient skin department for twenty-eight years, and had never seen an instance of it there. His first view was that it was sebaceous adenoma. He thought this was not only a tricho-epithelioma, but a tricho-epithelioma *plus* a sweat-gland epithelioma. In one place in the admirable sections shown he thought there could be discerned the hyaline line of the sweat-ducts in the centre of one of the masses, and proliferation proceeding from that. The lesions might arise from both sources.

Case of Tuberculosis Cutis.

By F. Carminow Doble, L.R.C.P., M.R.C.S.

As the microscope shows, this is a case of tuberculous disease of the skin. The buttock was probably infected by the bacilli passing through the rectum, but there is no sign of tubercle bacilli in the case.

Case for Diagnosis.

By J. E. M. Wigley, M.B.

Patient, a healthy girl, aged 7.

First seen two months ago, with a scaling eruption over the whole scalp, spreading about half-an-inch beyond the hair margin on to the glabrous skin, of about six months' duration. The dry silvery scaling and the sharply demarcated red base suggested psoriasis.

The eruption has steadily spread until at present the scalp, whole of the face, upper part of the trunk, upper parts of the limbs, especially extensor surfaces, are affected. Hands and feet entirely free.

The lesions are red patches, and over the upper part of the chest, shoulders and face are fused into large scaling areas. The limbs and abdomen have a roughened feeling, and there are islands of normal skin between the areas. The scalp is covered with greyish, dry scales.

Is the diagnosis of psoriasis still tenable?

Discussion.—Dr. Graham Little (President) said that this patient reminded him of one almost similar in age, who came to the Children's Hospital, Shadwell. He took the case into the wards, and the child improved enormously under thyroid treatment.

Recently Dr. Winkelried Williams showed a case of what was supposed to be definite pityriasis rubra pilaris, and that improved contemporaneously with, if not because of, the administration of parathyroid. He (the President) saw the case before and after treatment, and the improvement was astonishing. He did not know whether other Members had had

experience of cases of pityriasis rubra pilaris improving very rapidly; as a rule the condition was not very tractable. One case he notified for exhibition at the International Congress, and when the patient showed himself at the meeting there was nothing of the disease to be seen.

Dr. A. M. H. GRAY said he considered the condition in this case to be pityriasis rubra pilaris.

Dr. H. SEMON said he had at the moment a characteristic case of pityriasis rubra pilaris, with loss of hair on the scalp; last summer he took the patient into hospital with the view of observing progress. While there, he developed a somewhat acute double irido-cyclitis. He handed the patient over to the ophthalmic surgeon, who had a Wassermann test carried out. The reaction was found to be doubly positive. He (the speaker) was still certain of his diagnosis of pityriasis rubra pilaris, but, in view of the positive reaction, he prescribed the ordinary anti-syphilitic treatment, under which the eruption improved very much indeed. The patient was almost well, though he still had slight follicular lesions all over the body, especially on the thighs and buttocks. It was hoped to demonstrate these at the ensuing meeting of the Section.

Dr. GRAHAM LITTLE (President) said he showed before the Section, during Dr. Pringle's presidency, a man who had suffered from dermatitis herpetiformis for a long time, and for which he was having large doses of arsenic. He developed an eruption, which Dr. Pringle said he felt certain was pityriasis rubra pilaris, and which seemed to have developed while large doses of arsenic were being taken.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Late Post-X-ray Pigmentation.

By Haldin Davis, F.R.C.S.

The patient is a woman who was X-rayed for hypertrichosis thirteen years ago. She received a dose of unknown quantity almost every fortnight for two years. The peculiar feature of her case is that the unsightly pigmentation of which she complains did not begin to appear till at least five years after the cessation of treatment, and she says that it is still increasing. In this case the pigmentation is more conspicuous than the development of telangiectases.

Widespread Streptothrix Infection.

By H. C. Semon, M.D.

The granulomatous lesions now present in this patient, a butcher, aged 67, first made their appearance above the right collar-bone, about two years ago. There has been an extensive spread of the condition over the back to the middle line, and as low as the angle of the right scapula, with a marked tendency to suppuration and subsequent cicatrization. A recent lesion on the right cheek (about six months' duration) is raised above the surface and is soft in consistency, and is discharging a thick mucoid pus from numerous sinuses, thus recalling the appearances of actinomycosis. No grains have ever been demonstrated, but Dr. Embleton, using carbol-fuchsin and 25 per cent. sulphuric acid as his decolorizing agent, has discovered the acid-fast bacilli you have been able to see under a $\frac{1}{12}$ immersion lens. In appearance they are not unlike leprosy bacilli, but the clinical data make that diagnosis quite improbable, and it is suggested that they are the bacillary stages of a streptothrix group.

All attempts to grow the organism have failed. Considerable improvement has attended the administration of iodide of potassium (ninety grains per day).

Lichen Planus of Mouth and Lips.

By H. C. Semon, M.D.

Dr. Malcolm Simpson sent this patient to me a few weeks ago with typical circinate lichen planus lesions on the glans penis, and streaky white lesions on the buccal mucous membrane and on the lips at their cutaneous margins. The case much resembles that shown by Dr. H. C. Samuel a few weeks ago.[1] The lesions have cleared off the penis and are daily improving in the mouth under arsenical medication.

[1] *Proceedings*, 1924-25, xviii (Sect. Derm.), p. 23.

Case of Mycosis Fungoides of Fifteen Years' Duration.

By H. MacCormac, C.B.E., M.D.

THE patient, Mrs. S. S., now 52 years of age, was exhibited before this Section by the late Dr. Pringle in May, 1916.[1] The diagnosis of mycosis fungoides with positive Wassermann reaction was then made and generally agreed to. The disease has now been in existence for fifteen years, but in spite of the eruption, at times extensive, the patient has remained in relatively good health. In 1920 she came again under observation; there were then tumour masses over both shoulders. These and several other lesser lesions were treated by X-rays with success. She was admitted again in 1921 when considerable tumours were present over the left shoulder, the right side of the chest, and elsewhere. The lesions did not at this time respond well to X-rays, and the disease progressed, the patient becoming gravely ill. An attack of erysipelas then developed on the left leg, and following that there was at once a marked improvement both in the mycotic eruption and the general condition. Since that time it has been possible to keep the disease in check by occasional doses of X-rays.

In this case the attack of erysipelas seems to have had a very definite beneficial effect on the disease. Had this accidental infection not occurred, it is extremely doubtful whether the patient would have been alive to-day. Possibly a similar result might have been obtained by Coley's fluid, and it may be worth while considering treatment with this remedy in analogous cases.

In spite of the prolonged use of X-rays since 1920, amounting to more than forty-four full pastille doses, the blood-picture has remained normal. It may be pointed out that when using X-rays in this case, an attempt was always made to employ the smallest dose capable of removing the lesions, generally equal to a one-half pastille. This is, it would seem, an important consideration, as it saves the patient from being subjected to unnecessarily heavy dosage, with the possibility of damage to the blood-producing mechanism.

Dr. J. M. H. MacLeod said that some years ago he and the late Sir James Galloway reported a case of mycosis fungoides in which Coley's fluid was used, but it made very little impression on the condition.

Case of Lupus Pernio.

By H. MacCormac, C.B.E., M.D., and E. A. Cockayne, M.D.

THE patient is a young woman, aged 19, whose previous health has been good. There is no family or personal history of tuberculosis.

About two years ago a roughness of the nail of the right thumb was observed, and at the same time the left wrist became swollen and painful. What is described as a red patch also developed on the left cheek. Later, about eighteen months ago, there was a complaint of pain in the toes and of swelling and pain in the fingers now affected. A second cutaneous lesion developed below the left eye one year ago. The condition remained unchanged until about December last; then the pain in the feet ceased, but at the same time an aggravation of all the other symptoms occurred.

The present state of the patient may be summarized as follows:—On the right hand all fingers except the ring finger have a fusiform contour. There is marked striation of the nails of the little and middle fingers, also of the thumb, with erythematous patches over the dorsal surface of the terminal phalanges resembling those seen in lupus erythematosus. Some scarring can be observed in connexion with the erythematous patch on the middle finger. In addition, on the little finger there is a

[1] *Proceedings*, 1916, ix (Sect. Derm.), p. 147.

painful nodule the size of a pea deeply seated on the flexor surface of the proximal phalanx. It is stated that this, at times, assumes a reddish colour. The left hand presents in general a similar appearance, with striation of the nails of the thumb and index finger, and erythematous patches of the thumb, index and ring fingers. There is a fusiform deformity of all the fingers and thumb. Nodules are present on the proximal phalanges of the ring and little fingers on the flexor surface. On the feet erythematous lesions can be observed, although not of marked character, on the third and fourth toes of the right foot and the fourth and fifth toes of the left foot. There are also present the two erythematous plaques on the left cheek already noted. They appear to vary in degree from time to time.

Clinical examination of the patient has failed to reveal any evidence of tuberculosis. The radioscopic examination of the hands is of some interest as it shows the characteristic areas of rarefaction in some of the bones. In the feet this is not apparent in the radiograms. The radioscopic examination of the chest reveals enlargement of the hilus glands, indicating some abnormal condition in these structures.

Both tonsils were removed by Mr. Somerville Hastings, and microscopic preparations have been made from them by Dr. Baker, who reports slight fibrosis and evidence of chronic inflammation, but no circumscribed tubercles such as described by Schaumann. The cuti-reactions for both bovine and human tubercle are negative and the Wassermann reaction is also negative.

Urticaria Pigmentosa.

By G. B. Dowling, M.D.

A male infant, aged 5 weeks, presented at birth a bright-red erythema, covering the entire surface. A few days after birth the erythema began to fade in patches, leaving areas of normal-looking skin alternating with irregular patches of erythema. The erythematous areas were slightly infiltrated. A few days later, bullæ began to appear in large numbers upon the erythematous areas, especially upon the dorsal aspects of the hands and forearms, feet and legs, and about the scalp and face. A few appeared upon the trunk. Within about a fortnight of birth the bright-red colour had practically faded, and the lesions had become pale in colour, some of them with a faint bluish tinge. About this time innumerable dermic nodules were appearing and replacing the lesions just described. Some of the nodules were about the size of the split pea, others larger. They appeared in greatest numbers about the face, ears, forearms and hands, feet and legs. Many of them were capped by vesicles or bullæ.

The infant now exhibits an eruption, mainly nodular, accompanied by many vesico-bullous lesions, and in addition numerous flat, infiltrated patches of variable dimensions, especially about the buttocks and trunk. No part is free from the eruption. The colour is generally somewhat paler than that of normal skin with here and there the addition of the bluish tinge previously mentioned. The lesions obviously itch a great deal, and many of the bullæ have become infected from rubbing.

The general health seems unimpaired, and the infant is well developed and has put on weight normally.

The consensus of opinion appears to be that this is a case of urticaria pigmentosa. There is no actual pigmentation.

Discussion.—Dr. J. M. H. MacLeod said this case was much like one he showed before the Section two years ago, and of which he afterwards showed photographs. The interesting feature of the present case consisted in the bullæ, as when his case was exhibited he could find no other case mentioned in literature in which bullæ were present.

Dr. L. SAVATARD said he had a case which he had shown at a local society, not so extensive as this, in which bullæ were marked, and he thought that confirmed the diagnosis of urticaria pigmentosa.

Dr. H. G. ADAMSON said that this case reminded him of a case of bullous urticaria in an infant which he showed before the Section a few years ago.[1] A closely similar case of infantile bullous urticaria was pictured in the first edition of Dr. Sequeira's "Diseases of the Skin." Neither of these cases was lichen urticatus nor urticaria pigmentosa. A microscopical section should settle the question of the diagnosis of urticaria pigmentosa in Dr. Dowling's case.

Postscript.—Sections of this Case made after the meeting show dense infiltration of the derma with mast cells. The appearance is typical of urticaria pigmentosa.

Case of Pseudo-Pelade associated with Lichen Plano-pilaris.

By G. B. DOWLING, M.D.

PATIENT, a girl, aged 18. *History*: She had lichen plano-pilaris in 1922 for about nine months. She was exhibited by me in the absence of Dr. Barber at a meeting of this Section in 1922. Shortly after this occasion she developed about half-a-dozen bald patches on the scalp. These patches began simply as circumscribed patches of alopecia without any preceding follicular change. I diagnosed alopecia areata. At that time there was no atrophy.

She again came to the out-patient department at Guy's Hospital a few months ago with the condition now present.

The hair has never re-grown and there are now half-a-dozen bald patches of two or three centimetres in diameter scattered about the scalp, smooth and polished and slightly but definitely atrophied. I do not think the development of the alopecia is a coincidence, for the President, Dr. Graham Little, showed at a meeting of the Section in March 20, 1924[2], a somewhat similar case under the name of folliculitis decalvans et atrophians. This case, a lady, aged 55, had follicular lesions on the scalp resembling lichen spinulosus with a neighbouring area of cicatricial alopecia. This patient also had follicular lesions resembling lichen spinulosus on the forearm.

Dr. Little also stated the interesting fact that he had had a case with cicatricial atrophy of the scalp, lichen spinulosus and typical lichen planus, and suggested that all these manifestations might be forms of lichen planus. This he said had not been borne out by any other case unless the case he was then exhibiting might be said to further that view. He also drew attention to a published case of Schaumann's of perhaps the same nature.

Discussion.—Dr. GRAHAM LITTLE (President) said that four years ago he showed before the Section the first case of the kind published in this country; it was a patient whom Dr. Lassueur sent over to him. Soon afterwards, Dr. Beatty and Dr. Dore had similar cases. He believed those three were isolated cases for some time, until he (the speaker) had another in which there was definite lichen planus associated with lichen spinulosus, and with some cicatricial atrophy of the scalp.

Dr. S. E. DORE said he did not know whether the President's case showed definite lichen planus lesions, or whether Dr. Dowling's case showed lichen planus papules. [Dr. DOWLING: Yes.] If that was so, it seemed to prove that they were definite lichen planus cases, and that lichen planus could produce these atrophic areas on the scalp—a fact not generally recognized.

[1] *Brit. Journ. Derm.*, 1918, xxx p. 39.

[2] *Proceedings*, 1924, xvii (Sect. Derm.), p. 74.

Case of (?) Sarcoid.

By G. B. Dowling, M.D.

A. W., male, aged 45.

His history is that he began about five months ago to suffer from slight œdema of the dorsum of the right foot. This was shortly followed by the simultaneous appearance of two nodules, one on the anterior aspect of the left shin, the other on the plantar aspect of the right foot. A little later the middle toe of the right foot became somewhat swollen. These lesions have all gradually increased in size.

He now presents three swellings in the structures mentioned, and still some œdema of the dorsum of the right foot. The lesion on the left shin is a bluish-red node about the size of a nut, and is mainly hypodermic in situation. It is firm but not very hard.

On the instep of the left foot is a firm oval plaque, red in colour, quite hard, with a sharply defined margin.

The right middle toe presents a bluish swelling on the dorsum of the terminal phalanx. The whole toe is, in addition, somewhat swollen.

There is some aching and occasional shooting pain associated with the lesions. The instep lesion itches occasionally.

The Wassermann reaction is negative. Luetin test negative.

A radiogram of the right foot shows diffuse periostitis of the fourth metatarsal bone. I have made a diagnosis of sarcoid of the Darier type, and I do not consider it to be related in any way to Boeck's sarcoid, the benign lymphogranuloma type. A section of the shin lesion shows mild chronic granulomatous change in the derma, but nothing to help in making a diagnosis.

Discussion.—Dr. A. M. H. Gray said this case seemed to be a very unusual one; it did not seem to fit in with either of the sarcoid groups. He did not regard it as a hypodermic sarcoid of Darier, for he doubted whether the lesions in the case shown were ever entirely subcutaneous. On the other hand, they did not seem to fit in with the sarcoid of Boeck. In this case the lesions were very hard, whereas those in Boeck's sarcoid were very soft. The striking feature about the instep lesion was its hard nodular edge. It seemed to resemble somewhat closely the class of case described by Radcliffe-Crocker as erythema elevatum diutinum. It also bore some resemblance to granuloma annulare, and he believed that the tendency at the present day was to class these two conditions together.

Dr. E. Graham Little (President) said he believed Dr. Radcliffe-Crocker accepted erythema elevatum diutinum as being a form of granuloma annulare. Darier had suggested that granuloma annulare was a sarcoid. Therefore there was not much in dispute in this case.

Case of Lichen Nitidus.

By H. W. Barber, M.B.

Female, aged 23, lives on a farm. The eruption appeared first three years ago. There are typical lesions of lichen nitidus confined to the back of the left shoulder region; some of the papules are definitely umbilicated.

Dr. Beddard kindly examined the patient for me and reported that there was definite evidence of tuberculosis of the hilus type in the chest. This is confirmed by X-ray examination.

Investigations.—The von Pirquet test and complement-fixation test to tubercle were unfortunately not carried out owing to a misunderstanding, but they will now be done.

The patient has been treated on the balcony of the ward, and she has been given iodine and cod-liver oil internally. She has steadily put on weight, but no change is apparent in the lichen nitidus.

Dr. S. E. DORE said that the first case of the kind he had seen was that shown by Mr. McDonagh. He (the speaker) was not questioning the diagnosis, but it was worth mentioning that there was a form of linear nævus like this in which there were minute dotted lesions like pricks of a pin in a piece of cardboard. The present case was unilateral, and it suggested that form of linear nævus.

Scaly Eruption ? Syphilitic.

By H. W. BARBER, M.B.

J. C., MALE. This patient has a curious atrophic scaly condition of the buttocks, thighs and legs, the upper margins of the eruption over the sacrum being very sharply defined and bilaterally symmetrical. The eruption is said to have begun two years ago on the outer side of the left ankle at the level of the boot top, and has always been of the same character.

He also has vitiligo of the scrotum and penis, and leukoplakia of the lower lip, and, I think, slight tertiary glossitis.

The blood gave *a strongly positive Wassermann* reaction; the cerebro-spinal fluid gave a negative reaction and was normal.

Owing to the curious symmetrical distribution of the eruption on the buttocks and legs, I asked Dr. Symonds to see him. A careful chart of the skin lesions was made, which was very suggestive of an underlying nervous pathology. The area in the arm corresponded fairly well with the distribution of the internal cutaneous and musculo-spiral nerves, and in the lower limbs the relation to the nerve roots from the second lumbar downwards is again close. Apart from this, however, Dr. Symonds could find no signs of any central or peripheral nervous lesion.

Case of Papulo-necrotic Tuberculide.

By A. C. ROXBURGH, M.D.

BOY (W. J.), aged 6. Has always been pale. No cough. Takes food well. Whooping-cough and measles five years ago. German measles, December, 1924. No family history of tuberculosis. Came to St. Bartholomew's Hospital January 21.

About January 14, 1925, an eruption of multiple reddish-brown nodules appeared on arms and legs. Was in usual health at the time. Diagnosis lay between multiple lupus and papulo-necrotic tuberculide, but no definite apple-jelly nodules could be made out on diascopy, although a certain amount of brown staining seemed visible. He had some palpable axillary lymph-glands, especially on left side, and some small masses could be palpated in the left iliac fossa. No signs in chest except transient moist sounds at bases. Tonsils not enlarged. Wassermann and sigma reactions both negative.

Has had daily arc-light baths for two weeks (now 1¼ hr. daily) and cod-liver oil. Has gained weight each week. There is slight erythema and desquamation of skin owing to the light; many of the nodules have disappeared, leaving reddish scars, but several new ones appeared on buttocks and left elbow five days ago.

During the last three days some of the lesions, especially those on the buttocks, have become necrotic in the centre.

Case of Erythema Induratum (? Whitfield Type).

By A. C. ROXBURGH, M.D.

M. T., MARRIED woman, aged 61. Has always been healthy. Has one son, healthy. No miscarriages. No indigestion. Bowels open daily. No heart trouble. Slight varicose veins. Teeth mostly absent. One stump. Tonsils appear normal. Came to St. Bartholomew's Hospital, February 17, 1925.

For the last twelve months has had numerous nodules under skin of legs below knees, especially left leg.

The nodules are button-like discs, at first beneath skin, adherent to it, but movable on deeper tissues. Later they come to the surface and form flat, red-brown nodules, scattered irregularly. There is some desquamation of surface over the nodules, but they do not break down or discharge.

They are not painful except when picked up between the fingers.

Dr. A. M. H. GRAY said that if he had seen this case without learning anything about it first he would have named it the hypodermic sarcoid of Darier, but he had believed that the cases described by Dr. Whitfield were the same as Darier's hypodermic sarcoid. They varied greatly in the amount of pain and tenderness. In one case he had, the lesions were mostly in the buttocks. He believed Darier thought they were of tuberculous origin.

Case of Acute Sarcoids.

By E. G. GRAHAM LITTLE, M.D. (President).

THIS case is very like Dr. Roxburgh's, the patient coming to me from the tuberculosis officer at St. Mary's, who has been treating her for a long time for tuberculous disease of the hip. She has been under his observation continuously for several weeks. He says that all these nodules, some of them deeper, some very superficial, have appeared in the last three weeks ; this indicates a very acute onset.

Case of Erythematoid Benign Epithelioma.

By E. G. GRAHAM LITTLE, M.D. (President).

THIS is a case of erythematoid benign epithelioma, a single lesion on the right temple, of forty-four years' duration. She is subject to psoriasis, but there has been no psoriasis on the site of this particular patch.

Syphilitic ulcerating Granuloma of the Pudenda terminating with Amyloid Disease.

By W. J. O'DONOVAN, M.D.

F., AGED 25, single, has been sentenced for molesting men ; is hysterical and alcoholic ; she consorts with black seamen. Her mother has been in an asylum. The history is unreliable, but she says she has noticed sores of the vulva for six months. I admitted her into the London Hospital, January 7, 1925, and her condition is substantially unchanged. There is a crateriform ulcer in the mons veneris the size of half-a-crown, exposing the subcutaneous fat and exuding malodorous serous fluid. In the groins are small ulcers in large, ugly, pink, keloidal raised scars. Both labia majora are grossly swollen, red and superficially ulcerated ; this ulceration

extends backwards on either side of the anus. On both legs are small scars and numerous brown macules. The Wassermann reaction was positive, seven full doses of N.A.B. produced no clinical improvement. There is an irregular, low pyrexia of 99-100° F., no splenic enlargement, no albuminuria. She suffers no personal discomfort and would gladly leave hospital to-morrow. Blood-count, January 14, 1925: red blood cells, 4,900,000; hæmoglobin, 63 per cent.; colour index, 0·64; white blood-cells, 20,400; stained blood polynuclear neutrophils, 79 per cent.; polynuclear eosinophils, 0·5 per cent.; small lymphocytes, 15 per cent.; large hyaline cells, 5 per cent.; coarsely granular basophilic cells, 0·5 per cent. (H.A.A.). Films contained Gram-positive cocci and a few Gram-negative bacilli. There were large numbers of spirilla and bacilli resembling Vincent's (G. T. W.). She is at present receiving intravenous injections of antimony tartrate, and X-rays.

Postscript.—Death on April 6, 1925, of amyloid disease, confirmed post mortem.

Section of Dermatology.

President—Dr. E. G. Graham Little.

? Schamberg's Disease, or Angioma Serpiginosum

By A. C. Roxburgh, M.D.

E. W., aged 31, married woman. Sent to me at St. John's Hospital, by Dr. Theo. Murphy. About the middle of January, 1925, she noticed yellow patches on knees during convalescence from influenza. On February 17 when I first saw her she had yellow, slightly scaly patches over the front of both tibiæ just below the patella, about 3 in. long by 1½ in. wide. Patches of small dilated vessels were present over each great trochanter and similar patches were scattered over the buttocks. Blood could not be emptied out of the vessels by pressure. She says the affected areas ache. February 24: telangiectases on hips fading, leaving yellow-brown staining like that on knees.

Since I saw her she has developed one or two fresh areas on the back and abdomen, and there are some additional areas on the buttocks. The whole history goes back no longer than three months, and she never suffered from anything of the sort before. The Wassermann and Sigma tests are negative, there is nothing abnormal about the blood-count, and the section does not show much. There is thickening of the stratum corneum, and irregularity of the prickle-cell layer, with dilated vessels in the corium and some cellular infiltration in patches. No pigment visible. Certain Members think it is Schamberg's disease, others that it is angioma serpiginosum.

Case of Hemiatrophia Facialis of the Romberg Type.

By J. M. H. MacLeod, M.D.

Patient, a woman, aged 30. At the age of 9 she had an abscess under the first right upper molar tooth. This was followed by a scar on the chin, opposite the place where the tooth was removed. When she was aged 17 her doctor made an incision in the buccal mucous membrane to prevent this scar becoming adherent to the underlying bone. This was followed by atrophy, which gradually spread until it involved the right side of the face from the lower border of the inferior maxillary bone as far up as the malar bone. The skin in the atrophic area does not appear to be altered in texture from that of the other side, and there are no changes in sensibility, but the cheek is hollow from atrophy of the subcutaneous tissue, fat, and, to a less extent, of the muscles and bone. The palate is flattened on the right side, and several of the teeth are lost in the upper jaw; this necessitates the wearing of a dental plate.

This peculiar type of atrophy is believed to be a nerve affection connected with a loss of function of the sympathetic fibres of the fifth nerve, and it is possible that the abscess or the incision may have been responsible for it. The appearances are similar to those described by Romberg in a man as long ago as 1847. Romberg's case was subsequently recorded by the late Dr. J. F. Payne (*Trans. Path. Soc. Lond.*, 1881, xxxii, p. 306).

Discussion.—Dr. F. Parkes Weber said that at the meeting of the Section of Neurology, on March 12, 1925, Dr. J. P. Martin showed a young woman with one side of her face atrophied so that it looked like a death's head, while the other side looked like that of a normal well-nourished young woman. In that case it was difficult to decide whether the change was not confined to the subcutaneous fat and subcutaneous connective tissue. The fatty tissue in the region of one breast was also atrophied. The atrophic process in both regions had commenced at about 16 years of age, as in Dr. MacLeod's case.

Dr. WHITFIELD said that the case he showed at the first meeting of the Dermatological Association was diagnosed by Dr. Kinnier Wilson as typical Gowers' pan-atrophy. In that instance the skin was sclerodermic, and he agreed it was difficult to determine here whether there was any change in the true skin itself. It was only tied down because the subcutaneous tissue had disappeared. In the present case he did not think pan-atrophy was the right term to use; the correct title was that given to it by Dr. MacLeod.

Dr. MACLEOD (in reply) said that in this case the bone was affected, and that all of the teeth on one side were out. In Romberg's case all the tissues were affected. The radiogram showed but little atrophy of bone.

Case of Recurrent Erysipelas.

By H. MACCORMAC, C.B.E., M.D.

PATIENT, a woman aged 27, who gives a history of recurrent attacks of facial erysipelas extending over a period of two and a half years. She was first seen by me in November, 1924, when there was marked swelling and erythema of the cheeks, nose and forehead. The hair of the scalp had been almost completely shed. The condition proved at first rebellious to the forms of treatment adopted. She was given a course of seven injections of collosol manganese without effect. The improvement which has taken place may be attributed to a series of five injections of whole blood begun on February 17. It is proper to record that a similar course had been undertaken without marked benefit, but after the second series the inflammatory condition began to subside and the patient is now comparatively free from the condition. The attacks are rare, the swelling which accompanied them has markedly diminished and the hair is growing freely.

Discussion.—Dr. WHITFIELD said he thought that, in nine cases out of ten, these conditions were secondary to fissures at the anterior aspect of the nose, and they were not cured because the fissure was not healed. The moment the patient got a cold, the unsound scar gave way and re-infection occurred. This woman said she was never without a cold. If blood-injections would prevent her catching cold and obviate fissures, the œdema might die out. He had tried the treatment of destroying some tissue round the old fissure by means of the galvano-cautery, and in one case that seemed to do good; but as a rule he was not successful with these cases.

Dr. MACCORMAC (in reply) said he thought it was unnecessary to make any other diagnosis than that given. He did not pretend that the patient had been cured; but he was confident that considerable improvement had followed the second series of injections of whole blood. He admitted that in such cases it was common to find a fissure which might harbour a streptococcus. He had in such cases attempted to cure the condition with an autogenous vaccine without success.

Two Cases o Livedo Racemosa.

By H. C. SEMON, M.D.

IN the *Archiv für Dermat. u. Syph.*, 1925, cxlviii, p. 313, Perutz and Kaiser bring forward evidence, based on their experiments with adrenalin injections and amyl nitrite inhalations, to show that livedo racemosa and livedo calorica are two separate entities, and that the factors responsible for the vascular hypotonus in the former condition, attack the muscular walls of the blood-vessels concerned, i.e., the phenomena are not due to a neuro-vascular paralysis, as is the case in livedo calorica.

They also maintain that a constitutional predisposition to vascular hypotonus, e.g., acro-asphyxia, chilblains, &c., is commonly observed in both types of livedo, and that syphilis may be a causal or accessory factor in about 40 per cent. of cases.

The two cases shown—those of a mother and her daughter—seem to support the contention that a constitutional factor is involved, for we note here the typical network of venous congestion on the inner aspects of the shins. The mother has

observed the condition for six years, the daughter has noticed it for four months. The Wassermann reaction is negative in both, and Dr. Beattie, who kindly examined them for me at the Royal Northern Hospital, has excluded tuberculosis in the ætiology. The child suffers from chilblains and has marked acrocyanosis at the present time.

Dr. WHITFIELD said that whatever diagnosis was arrived at in these two cases, the size of the network in the two patients was about the same as that which was usually termed pigmentation ab igne. If so, it was difficult to understand on what grounds the two authors mentioned (Perutz and Kaiser) stated that pigmentation ab igne was capillary, whereas the other form affected vessels with muscular coats. If that were so the networks would be of different sizes, which was not the case.

Case of Generalized Lichen Nitidus.

By H. W. BARBER, M.B.

MISS E. P. First consulted me June 20, 1924, with the history that her eruption had begun over a year previously in the foot. When I first saw her she had a more or less generalized eczematous-looking eruption involving the wrists and forearms, the antecubital and popliteal spaces, the presternal region and submammary folds, the sacral region, thighs and legs. On the palms there were what appeared to be eczematous patches with deep vesicles showing through the horny layer. Some of the nails were rough and fissured. There was universal scaliness of the scalp. I was puzzled as to the nature of the eruption, but thought it was an atypical eczema.

In November, 1924, I saw her again and was struck by the resemblance of the lesions on the wrists and forearms to lichen nitidus. She was admitted to the private ward of Guy's Hospital for investigation.

Biopsy.—Microscopical section (shown) confirmed the diagnosis of lichen nitidus.

Blood.—Wassermann reaction and complement-fixation to tubercle-bacillus negative.

Fæces.—Coli-variants. Nothing else abnormal.

X-ray examination of chest.—Fairly dense fibrosis at the roots of the lungs, with calcified bronchial glands.

Case of Vitiligo, with (formerly) Alopecia Areata and Lupus Erythematosus.

By H. W. BARBER, M.B.

MRS. P. M., aged 32.

History.—Formerly used to suffer from severe attacks of sore throat until the tonsils were removed in 1912. Had severe oral sepsis, for which some teeth were removed in 1923.

In 1914 she had three patches of alopecia areata.

In 1916, *after a severe shock* which affected her profoundly, the pigmentation appeared, and she was under Dr. Arthur Hall, of Sheffield, who diagnosed Addison's disease.

First seen by me, at Dr. Debenham's request, November 14, 1924. Extensive vitiligo involving scalp, face, neck, back and chest, arms and legs. On the hands there were typical patches of lupus erythematosus, which have since disappeared.

Dr. E. PARKES WEBER remarked that certain French writers had claimed vitiligo as being frequently of syphilitic origin. That had not been confirmed by the examination of cases in London. Syphilis, however, produced, not very rarely, a disorder in the vegetative nervous system, a disorder which occasionally manifested itself by the appearance of either vitiligo or alopecia, or both. This, he believed, was the only point of connexion between vitiligo and syphilis.

Case of Persistent Circinate Erythema.

By H. W. Barber, M.B.

J. H. G., aged 40.

History.—In 1914 first noticed a "ringworm" on his left shin. Has frequently suffered from sore throats and at times from bronchitis. The present outbreak of the eruption began in October, 1924, on the right forearm. Since then he has not been free from the lesions.

Present Condition.—First seen by me a week ago. Characteristic case of persistent gyrate or circinate erythema. The lesions begin as raised, firm erythematous spots which form small, complete circles. The centre of the raised erythematous bodies is whitish, the edges are pink. As the rings extend at the periphery, part disappears, leaving $\frac{3}{4}$-circle figures.

The eruption is present as follows:—(1) Both gluteal regions. (2) Flexure surface right forearm, three lesions. (3) Right upper arm: one ring, flexor surface: two rings, extensor surface. (4) Left forearm, two rings, extensor surface. (5) Left arm, one ring, extensor surface. The patient has very severe oral sepsis. He has now been admitted to hospital for investigation.

Dr. H. C. Semon said the clinical appearances reminded him strongly of an exactly similar eruption in a girl, aged 10, first under Dr. Whitfield and subsequently under his own care at the Royal Northern Hospital in 1913. The centrifugally spreading rings of erythema with their smooth, raised borders would slowly extend over wide areas from the lower limbs to the trunk, and then as slowly disappear. The recurrent attacks, which were always accompanied by fever and subsequent debility, lasted about six weeks, and were favourably influenced by salicylates.

Postscript.—The infected teeth were extracted, and from them was cultivated a *Streptococcus longus*, which was agglutinated by the patient's serum in a dilution of one in 200. The eruption rapidly disappeared and has not since returned (two and a half months).

? Lichen Obtusus.

By H. G. Adamson, M.D.

Patient, G. D., a male, aged 73. He states that the eruption began twenty years ago with one "warty pimple" on the calf of the left leg and that others appeared later. The eruption now consists of numerous raised, flattened, dome-shaped, firm, dusky red nodules with circular outline. The nodules vary in size from $\frac{1}{16}$ in. to $\frac{1}{4}$ in. in diameter. They are irregularly grouped, some touching, but mostly discrete, over the middle $\frac{2}{3}$ of the inner and posterior surface of the left calf. Upon a nodule being pinched up it seems to be situated mainly in the epidermis and the superficial part of the corium. The nodules are smooth on the surface, slightly scaly, but not verrucose. There are two or three small horny plugs in each nodule. There are no typical angular, flat-topped lichen planus papules. There is no eruption elsewhere on the skin or mucous membranes.

On microscopical section a nodule shows: (1) Changes in the epidermis—broad processes of prickle-cells projecting into the corium. For the most part these have an intact basal-cell layer, but here and there the basal-cell layer is broken up. The stratum granulosum is everywhere well marked and there is thickening of the horny layer. (2) A sharply circumscribed, though not very dense, round-cell infiltration in the corium immediately below the epithelial projections.

The diagnosis suggested is lichen obtusus, in the sense in which this term was originally employed by Unna to designate a type of lichen planus papule which was obtuse or blunt and rounded, as opposed to the plane and acuminate papules. The

present case differs from those which were first named lichen obtusus corneus, by Brocq, in that the nodules are grouped in one area and not scattered more or less symmetrically over the limbs.

Discussion.—Dr. KNOWSLEY SIBLEY said this case did not represent his idea of lichen obtusus. Several of the lesions were coalescent, and there were masses of these lesions, and in the cases he had seen the irritation had always been considerable. There was no irritation in the present case, and the condition was on one side only. He did not agree with Dr. Adamson that the lesions were dome-shaped, and they were certainly not horny. In lichen obtusus corneus the lesions remained discrete. Could Dr. Adamson distinguish lichen hypertrophicus from lichen corneus under the microscope? He believed the present case was lichen hypertrophicus of very chronic nature.

Dr. WHITFIELD said he agreed with Dr. Adamson that this was a form of lichen planus, and that he regarded it as nearer to the lichen hypertrophicus class of case than the other more recently described lichen obtusus corneus. He noticed that Dr. Adamson had called the case lichen obtusus of Unna, and not lichen obtusus corneus. In this case each large papule had many plugs, in lichen obtusus corneus he had seen only one central plug and he thought the lesions were distinct.

Dr. A. M. H. GRAY referred to a case he had shown before the Section in December, 1921,[1] which was like the present case. He then described the case as lichen obtusus, but he believed Dr. Adamson refused to accept that diagnosis. He agreed with Dr. Whitfield that two distinct conditions had been described under the term lichen obtusus, and it had been used for the dome-shaped lesions of lichen planus and for the condition known as prurigo nodularis. Professor Pautrier, among others, was of opinion that prurigo nodularis had no relation to lichen planus, but that, histologically, it was a lichenification.

Case of Subcutaneous Nodules for Diagnosis.

By W. JENKINS OLIVER, M.B.

PATIENT, a boy, aged 3½ years, in whom the first nodule had been noticed at the age of 5 months; since that date the other lesions had gradually made their appearance. He is otherwise healthy and is the only child of the mother's second marriage. She has seven other children, all healthy, and has had no miscarriages; her father died of phthisis. The skin lesions are confined to the extensor aspect of the left forearm in the area just below the olecranon process of the ulna, and consist of five discrete, stony, hard, firm, intradermic and subcutaneous nodules grouped in an irregular ringed formation to the size of a florin. The nodules are roughly the size of a split pea, with two smaller gunshot-sized lesions within the circle, and they are all freely movable with the skin over the subjacent bone. The underlying skin is perhaps slightly stretched, and according to the mother's statement has appeared red at times so that she has applied iodine under the belief that the condition was ringworm. No history of trauma is obtainable to account for the lesions, which are not tender. A biopsy will be made.

Discussion.—Dr. GRAHAM LITTLE (President) said he did not think it would be possible to make a diagnosis in this case without a biopsy being done. From the feel of the nodules, he thought they might prove to be fibro-sarcomata.

Dr. PARKES WEBER said he thought this hard, subcutaneous plaque was more likely to be a kind of lymphangioma of nævoid origin. As an alternative diagnosis he suggested the possibility of xanthomyeloid tumour ("xanthomyeloma"), such as he discussed in a recent paper on "xanthomatosis."[2] A microscopical examination ("biopsy") would settle the point.

Postscript.—Section showed a granuloma of indeterminate nature.

[1] *Proceedings*, 1921-22, xv (Sect. Derm.), p. 13.

[2] F. Parkes Weber, "Cutaneous Xanthoma and Xanthomatosis, &c.," *Brit. Journ. Derm. and Syph.*, London, 1924, xxxvi, pp. 335-370. Most so-called xanthomyelomata (and xanthosarcomata) were of tendon-sheaths, but not all.

Case of Acne Varioliformis.

By E. G. Graham Little, M.D. (President).

Patient, a male aged 45, has had this condition for a year. At the present moment he has the typical eruption on his forehead and scalp—nowhere else.

Dr. H. C. Semon asked whether the acne lesions could occur elsewhere than on the scalp, temples, trunk and face. Recently he saw a bad case of what he took to be acne varioliformis, with prurigo-like papules on the backs of the hands, but as the distribution was so wide he felt doubtful of his diagnosis.

Case of Dermatitis Artefacta.

By E. G. Graham Little, M.D. (President).

Patient, a girl of 18, seen three days ago, showed large excoriations of very irregular outline, distributed upon the right breast and arm: they were polygonal areas, ranging from an inch to two inches across. She had been kept in bed at hospital for four days, with the affected side under bandages, and the condition had greatly improved. The agent used was not identified.

Discussion.—Dr. H. C. Samuel said it was usually stated that the lesions in dermatitis artefacta were seen to be within the reach of either hand. He asked whether Members had seen a case in which the lesions were caused by another party. He had had under care a patient whose case had puzzled the doctor for a long time. When he investigated the case he ascertained by cross-examination that the lesions had been produced by the mother, so that the girl should stay at home and help her with the house-work.

He (Dr. Samuel) said there were two kinds of cases:—

(1) The conscious cases, where the lesions were produced consciously and purposely with various conscious motives, such as freedom from work, compensation, desire for sympathy, &c.

(2) The unconscious type (hysterical in nature), where there were present other stigmata of hysteria, such as the anæsthetic palate, and other hysterical make-up in the mentality.

This unconscious type generally occurred in single girls and was of a similar origin to hysterical paralysis and anæsthesia and other hysterical phenomena. It often alternated with these. The lesions were produced unconsciously and yet with a definite motive, but if of an unconscious origin the motive was not a conscious one but was a physical replacement of a repressed complex of the mind with strong emotional tone. The whole process was as it were an outlet for the strong feeling-tone attached to the idea, which, owing to its incompatibility with reality, had to be put back into the unconscious but found its way out in these hysterical manifestations of various physical replacement phenomena (hysterical paralyses, anæsthesias, dermatitis artefacta, &c.). Freud regarded them all as derived from the sexual repressed instinct, but there was no doubt that other instincts might be the cause in other cases and all were not necessarily sexual in origin.

These cases, once diagnosed and temporarily ameliorated, should be handed over to the psychotherapeutist for suggestion treatment or psychological analysis.

Dr. Parkes Weber referred to the case of a woman with dermatitis artefacta that he had described long ago,[1] in which the patient had previously (i.e., before the dermatitis artefacta appeared) had four laparotomies done, without the cause of the abdominal symptoms being discovered. In that patient the history had been followed out for many years. On one occasion she had had fæcal vomiting, and a question of a gastro-colic fistula arose. An enema coloured with methylene blue was given, and soon afterwards, while she remained under observation, some of the coloured enema appeared in the vomit. Experimentally, it had been found in many cases of men and animals, by various observers, that lycopodium powder injected into the rectum might travel upwards so as to reach the stomach in a few hours.

[1] See F. Parkes Weber, "Two Strange Cases of Functional Disorder," *International Clinics*, Philadelphia, 22nd ser., 1912, i, pp. 125-138 (the second case).

Multiple Sarcoids.

By E. G. Graham Little, M.D. (President).

Patient, a nurse, aged about 40, shows a number of hypodermic swellings, with little or no alteration of the overlying skin. Distributed entirely upon the right leg and thigh; the longest duration is eight months. There is no history of tuberculosis.

Case of Pityriasis Rubra.

By E. G. Graham Little, M.D. (President).

Patient is a boy of 10. He has had a universal eruption for three years. There does not appear to have been any preliminary disease of the skin. The whole integument is now involved, the skin is extremely inflamed and exfoliative all over. There is marked ectropion. His present general health is fair, and he is an exceedingly intelligent boy.

Adenoma Sebaceum Tumours in a Boy aged 19.

By G. B. Dowling, M.D.

This is a case of adenoma sebaceum of the Pringle type which is peculiar in that it is strictly unilateral. I should like suggestions as to the best way of removing the tumours. I have thought of electrolysis.

Postscript.—Acting upon the advice of Dr. Semon I have since treated this case by application of the point cautery, with complete success.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Bowen's Disease.

By Professor P. L. Bosellini.

(Royal University of Rome.)

(Condensed from the French original.)

To apply the name of a person to a morbid entity is not free from objection, but may be held a necessity until it is possible to grant a name which will indicate how the affection is caused, and also mark its clinical and anatomical characteristics. If Bowen's original description is considered, one may recognize a peculiar morbid syndrome, and his case should be made the pattern for the investigation and criticism of other published cases.

In Bowen's case the affection of the skin was limited to one buttock, and appeared as an irregularly circular patch about 10 cm. in diameter. Part of the patch had a cicatricial aspect, part was occupied by papular and nodular elevations, almost round or lentil-shaped. These were placed especially at the periphery of the patch, and whether isolated or confluent conferred on the patch a particular stamp. These elevations were in colour dusky-red, in texture distinct from the surroundings, but not indurated. Their surface was papillomatous, and in some parts they were covered with scales, or scaly scabs, the consequence of exudation. These, when detached, exposed a moist and even bleeding area. The arrangement of nodules, towards the periphery in particular, together with the cicatricial aspect about the centre of the patch, produced a general resemblance to a serpiginous nodular eruption originated by syphilis or tuberculosis.

In Bowen's second case the eruption was limited to the skin of the right leg, and exhibited the same arrangement of scarring and of nodules as in the first case. The histological examination of the nodules, great and small, showed changes both in the epidermis and dermis. In the epidermis there was an alteration, constituting hypertrophy, in the corneous layers, changes named by Darier, keratosis, parakeratosis and dyskeratosis. Alterations in the rete Malpighii exhibited hyperplasia, a division of cells being marked by numerous mitoses. Also amitotic division was indicated by a heaping together of several nuclei, or the formation of enlarged humped nuclei within the cells. Besides, œdema had caused dilatations between the epidermal cells, along with some infiltration by leucocytes. This process had given rise to vesicles between and within cells, as in Paget's disease. Changes in the underlying dermis consisted in a mucoid degeneration with disappearance of connective tissue and elastic fibres as well as an infiltration by cells, plasma cells, lymphoid cells and polynuclear cells, especially around the blood-vessels.

Darier published a case in confirmation of Bowen's observation in which there was a patch on the left buttock, altogether like that in Bowen's case. But besides the large patch of a syphilitic type, there were two other patches presenting a papillomatous and warty surface—one in the right axilla, together with an enlarged axillary gland; the other on the back.

Darier's second case, as differing from Bowen's, showed multiple lesions. A patch over the right malleolus resembled those described above. There were, besides, half a score of eruptions in other regions, particularly on the trunk; some of these were red patches with sharply defined margins, covered with fine, very adherent scales, and an atrophy of the underlying skin. Some were yellowish scaly patches, also with atrophy underneath, others were brownish, not desquamating, with an epidermis divided into fine squares. Another case described by Darier and Danal presented lesions almost the same as the preceding one. Besides, there was on the forearm an oval, bleeding ulcer, which had appeared upon a cicatrix, along with a corresponding glandular enlargement in the axilla. Among the patches observed by Darier were some which were flattened, or papular, or nodular, or ulcerating. In the axillary gland just noted were found cells of the rete Malpighii which had undergone Darier's dyskeratosis. In addition, Darier observed a large patch on the thorax, irregularly circular, pale red, flattened, atrophic, desquamating, which, in some parts showed a papular margin, hard, white, and translucent, owing to ischæmia. The patient said that this patch had formerly resembled other more papular and nodular patches. Darier explained that the atrophic patches represented the result of a reabsorption of nodules, alternatively the seat of abortive nodules, destined not to develop further.

Besides these objective characters, cases of Bowen's disease are distinguished further by the patient's history. The eruptions commence in youth or manhood without appreciable cause; the development is very slow, extending over a score of years; whether restricted to a circumscribed patch at one spot, or presenting multiple eruptions, they do not trouble the patients; glandular enlargements occur seldom, and when they do appear, late.

A further characteristic is the occurrence of similar eruptions in other members of the family.

The Author's Cases.

Case 1.—A female, aged 56, was examined two years ago and the case was published by Dr. Sparacio. The father of the patient died, at the age of 70, of cardiac disease. He had had six patches disseminated over the body, altogether resembling those on his daughter, and to these he had paid so little attention that he could not say precisely when they had first appeared. His sister, still alive, has suffered from the same disease, viz., half a score of patches scattered over the trunk, which, so it seems, had spontaneously disappeared without any caustic application. The patient had nothing noteworthy in her history. The skin eruption had begun twenty years before on the right hypochondrium, as a small elevation hard, brown, covered with a scab, or rather perhaps a scaly scab, which, when detached, exposed a dry reddish surface. The patch spread slowly at its periphery until it reached the dimensions of a halfpenny. Meanwhile further patches appeared in succession, but these did not make the patient seek medical advice until fresh ones arose near the eye. Then a score of patches were to be seen scattered all over the body. In addition to one on the chest resembling a warty nævus, the majority were almost circular, nummular, sharply defined, in colour rosy red to reddish violet. Some were covered with adherent scabs, which, when detached, left superficial ulcerations. Some presented merely a superficial infiltration underneath; those on the front of the chest were small nodules of varying consistency. Besides the flat patches, there were on the face lenticular and fusiform nodules, brownish, having a warty or mulberry-like aspect, covered by a horny mass, which, on separation, exposed bleeding papules. In addition, there was a diffuse enlargement of lymphatic glands. A patch was cut out from over the sternum and a nodular one from the right lower eyelid. On microscopic examination there was found no substantial difference

between the two patches. The eruption appeared to have as the principal centre of its development the cells of the rete Malpighii, and to start in this zone without any appreciable alteration in the dermis underneath. By the time that a visible nodule had arisen, a mucoid degeneration had set in underneath, causing a disappearance of connective tissue and elastic fibres. The dermis was replaced by a mucoid network enclosing polymorphic cells, winged and stellate. These evidences of epidermal hypertrophy and of degeneration in the dermis were not accompanied by evidences of inflammation. The bunch of epithelial cells forming the nodule had a regular outline and an absence of signs indicating the penetration of an epithelioma. The epidermis was divisible into two zones—the deeper presenting an overgrowth of the Malpighian layer, tending to undergo keratinization, the superficial composed of granular and horny cells. In varying degrees the superficial corneous layer had become infiltrated by leucocytes and serum to form scaly scabs. At a more advanced stage the epithelial cells of the nodule exhibited signs of degeneration, resembling the dyskeratosis of Darier, and signs of inflammatory infiltration of the dermis. The general aspect of the eruptions was that of an epithelial overgrowth, benign in character, not tending to invade the subjacent connective tissue.

In a second case under the author an important side of Bowen's disease was more definitely exhibited, viz., the relation of the affection to nævi and senile warts.

Case 2.—Male, aged 55, had a sister and a nephew who presented similar lesions, although in a less developed degree, otherwise the personal history of the patients possessed nothing of importance. In him the disease had actually started thirty years before with small tumours on the chin. Gradually others appeared on the face, on the limbs, and on the trunk. During the preceding three years there has been a rapid multiplication of the eruptions. On examination there was found to be a remarkable number of eruptions all over the surface of the body, with the exception of the soles and palms. They varied in size from that of a lentil to that of a pea; in shape, rounded, sharply circumscribed; rosy red or brownish in colour; disc-like or hemispherical, the surface smooth or mammillated, or warty. Some were covered by small scabs which, when removed, left bleeding papillomata of an elastic or indurated consistency; whilst some spots tended to become pedunculated, others remained flattened discs; some, not elevated above the level of the skin, presented an increased pigmentation, others a loss of pigment. These last patches were tending to cicatrization and were stated by the patient to have previously been the seat of warts. The patches gave no trouble; there were no signs of senile warts, nor of seborrhœa. No enlarged glands were found.

The characteristics which support the contention that there is a morbid entity termed Bowen's disease are mainly clinical, including the history and course of the malady. The anatomical features do not definitely distinguish Bowen's disease from some other benign cutaneous overgrowths.

Turning to the literature of the subject, MM. Martinotti and M. C. Ducrez have collected lists of cases which cannot be taken without reserve as all belonging to the same class. Some benign eruptions may be dismissed offhand, such as molluscum fibrosum. The name of Paget's disease properly applies to the original description affecting the nipple. Difficulty arises when that name is transferred to apparently similar changes on other surfaces than the nipple. A further extension of the term "extra-mammary Paget's disease" has given rise to the term of "Pagetoid epithelioma." More clearly distinguishable from Bowen's disease are the rodent ulcers of Jacob, and the baso-cellular epitheliomas. The clinical feature of Bowen's disease is distinct from various dermatoses such as eczema, senile warts, erythematous lupus, and psoriasis, also from tertiary syphilides covered by encrusted papules.

More obscure are the distinctions in some epitheliomatous varieties described in recent years by Graham Little, Savill, Gray and Segnere. Whilst they cannot be well classed as rodent ulcers or baso-cellular epitheliomata, they may differ from Bowen's disease as described above. Graham Little would separate, under the term erythematoid epithelioma, a type different from rodent ulcer, epithelioma, Pagetoid epithelioma, and different from Bowen's epithelial neoplasm, owing to its special resemblance to lupus erythematosus.

Discussion.—Dr. J. H. SEQUEIRA said that the Section was indebted to Professor Bosellini for a very admirable paper. All Members of the Section had probably seen in their practice, especially in recent years, cases of multiple, superficial carcinomata of the skin; and he was particularly interested to know that Professor Bosellini took a wide view of this group; it was the view he (Dr. Sequeira) had been inclined to take himself, in order to include some conditions which, at different times, had aroused some discussion in this and other societies. Professor Bosellini had given a lucid description of his personal cases, and the photographs and sections shown illustrated and enforced his thesis.

Dr. H. MACCORMAC said he was extremely interested in what Professor Bosellini had said, because it seemed to him (Dr. MacCormac) that the wider view—that which the Professor took—was the more correct one. A short time ago, it would be remembered, he had brought a case to the Section in which he suggested the diagnosis of Bowen's disease. Different opinions were expressed, some of the Members—including the President—holding that it was a form of basal-celled carcinoma of the type which Dr. Graham Little, in particular, had described. One of the reasons for that view was that small downgrowths were to be seen in parts of the section. In a pre-malignant condition the presence of such downgrowths was an expression of the tendency towards malignancy and therefore a proof of the nature of the disease. He believed Darier had laid particular stress on the eczematoid character of these lesions, a point to which Professor Bosellini did not appear to attach any very great importance.

Dr. AGNES SAVILL said she did not know whether this was the occasion to refer to multiple, flat rodent lesions. The President would remember the case she showed some years ago, when Dr. Darier visited the Society; he had that day also shown remarkable microscopical sections of those and allied conditions. She had had an opportunity of following up her case of multiple flat rodent. When Dr. Darier saw it he had said that if the patient were given a 1-pastille dose of X-rays, the condition would be cured. The patient had developed paralysis agitans since then. Many of the lesions, those X-rayed, and those not X-rayed, were becoming definitely carcinomatous. They showed a raised, bleeding surface. When the X-rays were applied the lesions died away for a month or two, and then the patient returned with a recurrence. Whether there was any recognized relationship between these multiple, flat conditions and Bowen's disease she did not know, but this particular case seemed to be on the border-line.

Case of Granuloma Annulare.

By HENRY MACCORMAC, C.B.E., M.D.

THIS patient, a male aged 35, now presents a typical appearance of granuloma annulare. The interest in this case lies in the course taken by the disease. The condition began, it was stated, fifteen years ago on the left hand as a "lump under the skin"; subsequently other "lumps" appeared on the hand and then on the left ear. When first seen in 1920, the eruption was confined to a group of nodules on the left hand and on the left ear. The condition has recently altered in appearance, the lesions having flattened down and assumed the form characteristically seen in granuloma annulare. In the earlier stages the diagnosis of sarcoid was made, and the alteration in the character of the eruption would seem to show that a relationship exists between the two conditions.

Case of Leprosy.

By HENRY MACCORMAC, C.B.E., M.D.

THE patient exhibited, a boy aged 9, came to England from Brazil three years ago. It is difficult to determine exactly when the disease first appeared, but lesions certainly have been noted during the last two years. The present state is as follows:—There are numerous pigmentary patches of varying colour and dimensions, on the arms, legs and trunk, the larger of a deep brown colour with a slight degree of desquamation, the smaller of a lighter fawn tint, and smoother. One or two of the more deeply coloured areas have lost their central pigmentation.

In certain situations, especially the face and arms, there are small discrete nodules, some exhibiting an increased pigmentation, varying in size from that of a pin head to that of a small split pea. There is also conjunctivitis of the right eye, and a small ulcer is present on the front of the left leg. A history of bleeding of the nose can be obtained and there is now a slight nasal discharge. The lymphatic glands are enlarged and easily palpable in the groins, epitrochlear regions, and to a less degree in the posterior triangle. The superficial nerves do not show the characteristic thickening usually present in such cases. Investigation of the sensory condition of the pigmented areas reveals some degree of confusion between heat and cold and there is definite loss of the perception of touch in some situations. A slide made from the nasal discharge and suitably stained shows numerous acid-fast bacilli.

According to Rogers and Muir the incidence of leprosy in Brazil is 0·72 per thousand. From information received in a private communication, it appears that leprosy is common in Manaos, where the patient formerly lived, several hundred cases being registered.

Cutaneous Xanthosis, especially of the Palms of the Hands, in a Patient with Grave Diabetes Mellitus.

By F. PARKES WEBER, M.D.

THE patient, Mrs. E. T., aged 48, English, was admitted to the German Hospital on May 5, 1925, with grave diabetes mellitus and urinary acidosis, under the care of my colleague, Dr. E. Schwarz, by whose kindness I am able to show the case. She had had symptoms of diabetes mellitus for the last six months, and during the last four weeks before admission she had noticed that her hands were very yellow. The ochre-yellowness of her hands, especially of the palms, is still very striking, but not as much as it was on admission. There is likewise a (lesser) yellowish coloration of her feet (chiefly the soles) and (very slight) of the face and other parts of the skin; but there is no yellow tinge of the sclerotics as there is in true jaundice. The urine is free from bilirubin and excess of urobilin and urobilinogen; and it is not specially highly coloured. The patient's blood-plasma is abnormally yellow, but gives negative direct and indirect Hijmans van den Bergh's reactions for bilirubin. The abnormal yellowness of the blood-plasma is due to excess of a lutein-pigment, which can be (and in a specimen from this patient has been) extracted with ether. The blood-plasma

colour has been estimated (by Dr. F. E. Loewy, May 19) at 9, by Meulengracht's scale, whereas 3 is the physiological maximum in ordinary persons.

The cutaneous xanthosis since admission has certainly diminished, whether or not as a result of the patient's general improvement under insulin treatment; the urine still contains a little sugar and the tests for acetone and diacetic acid are weakly positive. It should be noted that during the last five or six months before admission the patient, though she had not eaten carrots, had been eating a large quantity of cabbage, which is likewise rich in carotin (a pigment allied to lutein). The present case is very similar to the one I showed at the meeting of the Section on October 16, 1924, which was described, together with a short consideration of the whole subject, in the *Medical Press*, London, 1924, clxix, pp. 471-473. The last-mentioned patient (a young man) is now doing well with insulin treatment under out-patient supervision.

Section of Dermatology.

President—Dr. E. G. Graham Little, M.P.

Eight Cases of Lupus Vulgaris treated with Pyotropin.

By H. C. Semon, M.D.

These eight patients, suffering from lupus vulgaris,have been treated by a new German local caustic called "Pyotropin."

Dr. Axmann, of Erfurt, writing in the *Deutsche medizinische Wochenschrift* of April 3, 1925, p. 560 describes very successful results in eighteen inveterate cases of the disease, and expresses the opinion that the new preparation is "better, cheaper and simpler in its application than all the other lupo-therapeutic agents put together."

There are two solutions ("1" and "2") and an ointment. Both the solutions contain an aqueous solution of potassium phenolate and free carbolic acid (No. 1 in stronger percentages), with a deposit of chalk. The ointment-base consists of spermaceti, glycerine and glucose, and contains equal parts of sodium salicylate and salicylic acid. The technique of application is simple, but requires care. Secondary sepsis is first eliminated and then solution 1 is rubbed into the affected area, or, if the area be extensive, into part of it, with a small cotton pledget, and allowed to act for a minute. Solution 2 is then applied in a similar manner, and a strip of linen coated with the ointment and cut to fit the lesions is fixed with strapping and *made into an air-tight dressing*, which is left *in situ* for four days. The copious discharge is then cleaned off with benzine and the area again treated with No. 2 and securely enclosed under the ointment for a second period of four days. The nodules of lupus tissue are found to have ulcerated out at the end of the second period, and the lesion may then be allowed to heal up under boric or zinc ointment. None of these patients has had more than one course as described above, and in two of the eight cases the lesions have healed without leaving a visible trace of the characteristic tubercles, while the others have certainly improved more rapidly than with any other method I have used.

It is of course too soon to form any definite conclusions, the method having been under trial only eight weeks. But the results in that short time seem to justify a demonstration of cases in order that others with the same abundance of clinical material may put pyotropin to the proof.

The preparation appears to have a more specific action than any other caustic application I have yet tried. The nodules seem to be picked out, at the moment of application, by a darkening in their colour, and on many occasions there was not the slightest inflammatory reaction of the surrounding skin, as is usually the case when other caustics are applied.

Discussion.—Dr. H. C. Semon (in reply to a question by Dr. MacLeod as to whether the preparation was painful) said that in one case the application of No. 2 solution was very painful. The amount of pain produced seemed to be proportional to the depth of the ulceration. The children said it stung them a little, but they were not distressed by it for more than a few seconds.

Dr. H. W. BARBER asked whether Dr. Semon had gathered from the original paper that the nodules came up again in the scars left by the treatment, and, if so, whether this treatment could be used for deep nodules in scar tissue. Also, was the method applicable to lupus in the mucous membrane?

Dr. SEMON (in reply to Dr. Barber) said that he had no personal experience, but that according to the instructions supplied, mucous membrane could also be treated with this preparation. In the case of the nose, one nostril was treated at a time, and the application was made in the same way, a strip of linen being soaked with the ointment and put into one nostril, which was then occluded by strapping. In the case of sites such as the epiglottis, in which the ointment could not be sealed on and the air excluded, painting with the No. 1 solution was said to be sufficient. He did not gather from Dr. Axmann's paper that he had had any failures with it. If the first and second applications were not sufficient, another treatment was given until the deep-seated nodules had been eradicated. In one of Axmann's cases the treatment had been preceded by scraping of the areas. In one of his (the speaker's) own cases (lupus verrucosus) he had previously applied salicylic acid and creosote plaster to get rid of the verrucose tissue, and he thought a combination of these methods might be good.

Dr. J. H. SEQUEIRA said that the principle of this treatment was evidently the same as that underlying the treatment of lupus vulgaris by creosote and salicylic acid plaster, which had been devised by Unna; it was also the principle of the brass-paste treatment. It depended on the application to the lupoid area of a substance which would be destructive to the non-vascular lupus nodules without damaging the other tissues. He asked whether Dr. Semon would later exhibit cases which he had so treated after a longer interval of time. Superficially, good results followed the brass-paste treatment, but further experience showed that the deep-seated nodules were not eradicated.

Case of Xanthoma Tuberosum Multiplex.

By G. B. DOWLING, M.D.

PATIENT, a male, aged 38, a Russian Jew, began to develop the disease abouta year ago. He presents a very large number of xanthoma tumours in the usual positions, including both upper and lower eyelids, and, in addition, several in various tendons. The extensor tendons on the dorsal aspect of the fingers and of the triceps are involved, and recently two small tumours have developed in the tendon of the peroneus longus behind the left external malleolus. The whole skin and the conjunctivæ have a slightly icteric tinge.

General examination reveals nothing abnormal in the cardio-vascular, respiratory or nervous system. Blood-pressure 130 mm. Hg. The spleen and liver are just palpable.

Investigation.—Dr. Archer, of the West London Hospital, has kindly carried out the following investigations:—Blood cholesterin, 900 mgm. per 100 c.c., about nine times the normal figure; blood-sugar and sugar tolerance curve are normal. The urine reveals nothing abnormal except marked excess of urobilin. The Van den Bergh test is negative and the lævulose test for liver efficiency is quite normal. There is no excess of fat in the fæces.

This a case of xanthoma tuberosum multiplex of the diathetic type, with marked hypercholesterinæmia. I have had three cases of this disease in Russian Jews. Presumably Jews are especially liable to it.

The points of interest are (1) the very high blood-cholesterol figure, (2) the involvement of many tendons, and (3) the general icteric tinge. The latter cannot be accounted for by the presence of bile-pigment in the blood, since the Van den Bergh test is quite negative. Perhaps it is due to excess urobilin in the blood.

Discussion.—Dr. F. PARKES WEBER said he thought the urobilin in the blood-serum ought to be investigated again. He did not know that it had been acknowledged that urobilin could colour the sclerotics. In the cases formerly spoken of as urobilin-jaundice, it was now known that the jaundice was due to bilirubin in the blood-serum. He asked what the

exhibitor thought of the prognosis in the case. His own view was that, with such a wide distribution of lesions, the man might develop some form of internal xanthomatosis. He understood from others present that in non-diabetic cases of xanthoma insulin treatment had been tried, but without success.

Dr. L. SAVATARD said the exhibitor mentioned that among other lesions were some on the eyelids. The speaker had shown to the Section a boy who had lesions on the lids as well as on the trunk and extremities, and he had hypocholesterinæmia. He had only been dieted, and this had exercised no influence on his condition. It was a question whether insulin would benefit him.

Professor DUBREUILH said he had at present a case much like the one now shown, but more pronounced. The patient had jaundice, and the urine contained urobilin. His condition had been improved after he had been given hepatic extract and calomel, but he (Professor Dubreuilh) did not know whether that treatment was the cause of the improvement.

Syphilitic Pseudo-elephantiasis.

By H. MACCORMAC, C.B.E., M.D.

PATIENT, J. S., a man aged 73, gives the following history: Seven years ago he was suddenly attacked with severe pain in the back, and after this his left foot, leg and thigh became swollen and œdematous. About three years later, what he describes as two ulcers—which were in fact gummata—developed in the left thigh. Afterwards the varicose condition noted over the left abdomen, thigh, scrotum and penis developed, indicating some further obstruction of the deeper veins in the thigh or pelvis. No other symptom occurred until about a year ago, when apparently the elephantiasis, lymphatic varices, and ulceration developed in the left leg.

The condition on admission to hospital was as follows: The enlarged veins were noticeably present on the left abdominal region, thigh and scrotum. Two pigmented scars marked the site of the former gummata; the left foot and leg were swollen, and lymphatic varices in the form of papillomata were present about the ankle. In this area there were two small ulcers.

The condition has improved considerably, as the result of rest, with treatment by bismuth injections and potassium iodide.

In this case, although no history of syphilitic infection was obtained, the Wassermann reaction is strongly positive. The exact nature and position of the venous obstruction are difficult to determine. The history of the sudden pain in the back, with swelling of the leg, suggests a thrombosis at that time, but the later and more considerable changes directly followed the gummatous development in the left thigh.

Dermatitis (? Artefacta).

By H. MACCORMAC, C.B.E., M.D.

THE patient, a man aged 52, a tailor's presser, states that the eruption began nine months ago, on the left upper arm, in the form of a blister. Subsequently other similar lesions appeared on the wrists, thigh, arms, and elsewhere.

He now presents numerous sharply demarcated pigmented areas, in some of which scar tissue has developed. All have originated, it is stated, in the form of blisters. Within some of the pigmented areas a smaller circular lesion can be distinctly seen. Although the eruption is also present on the back, every spot is within reach of the patient's hands. He states that he has been unable to follow his occupation on account of his disease, and that fresh areas develop directly he resumes work.

Discussion.—Dr. S. E. DORE said he found it difficult to believe that this was a case of artefact. The inaccessibility of some of the lesions on the back, and the characters of the eruption suggested a possible mycosis, and he (the speaker) thought that a microscopical section might throw further light on the case.

Dr. HALDIN DAVIS agreed with Dr. Dore. If the lesions were artefacts they were very cleverly produced, and he asked what the substance used was thought to be, as he did not know anything which could produce such an appearance.

Dr. J. H. STOWERS supported the diagnosis of dermatitis artefacta, and said that even in cases in which some of the lesions were so distributed as to be beyond the reach of the patient, it was not improbable that the aid of an accomplice had been employed.

Dr. SAVATARD said that he also considered the lesions to be artefacts. The eruption seemed to come out about two hours after the man began working, and all the lesions were within the patient's reach. It would be well to get him to re-start work under close observation.

Dr. GRAHAM LITTLE (President) said that artefact was less common in men than in women, but Dr. Savill and himself had recently had a case of a boy whose eruption was difficult to establish as an artefact. It cleared up absolutely, however, after many months' reproduction of it, on a threat that he would be taken into a home and kept there as long as he had any lesion at all.

Dr. AGNES SAVILL said that itching was complained of by the boy to whom the President referred; the itching occurred before each outbreak, and had led to the case being considered one of intestinal toxæmia.

Dr. MACCORMAC (in reply) said that he still suspected that the condition was an artefact. The man had told him that there was intense pain before the lesions appeared. All the lesions were within reach of the patient's hands and the distribution of the eruption was very striking. Several similar cases came under his observation in France during the war. Artefacts in men were produced with the intention of avoiding some duty, or obtaining some benefit, generally of a pecuniary nature, and in this they differed from those met with in hysterical women.

Case of Unusual Pigmentation.

By H. MACCORMAC, C.B.E., M.D.

THIS patient was first seen this afternoon, and therefore a full investigation of the case has not been possible. The child, a healthy male infant aged 1½ years, has been gradually becoming pigmented for the last twelve months. The condition is well marked on the scalp and trunk, in the latter situation suggesting arsenical pigmentation. It is definitely stated, however, that neither the parent nor the child has at any time taken this drug. Both the parents are of European descent.

Case of Hydroa Æstivale.

By H. MACCORMAC, C.B.E., M.D., and P. R. PEACOCK, L.R.C.P.Lond., M.R.C.S.Eng.

THIS patient was exhibited before the Section on a previous occasion, and is now brought again in connexion with some experiments we have made, with the object of protecting her skin against the effects of light. Substances that have the property of fluorescing in the ultra-violet light beam have also the property of protecting the skin, as has been pointed out by one of us (P. R. P.). This effect will be demonstrated presently, but it may here be said that quinine in solution fluoresces markedly, ceasing to do so when the solution is allowed to dry. An ointment containing quinine also fluoresces and is also protective to some slight degree. Lanoline possesses both qualities to some extent, and white vaseline may be placed in the same group as lanoline. Lard neither fluoresces nor protects. The most marked fluorescence is exhibited by yellow vaseline, which has also very considerable

protecting qualities, as the following experiment carried out with our patient demonstrates. An area on the forearm was smeared over with yellow vaseline, and then the whole forearm was exposed to bright sunlight for ten minutes. On the treated area no reaction occurred, but on the control region a typical eruption developed. This is shown very well in a photograph taken twenty-four hours after the experiment. This protection by yellow vaseline is of limited duration, and, as would be expected, the substance must be reapplied from time to time if its effect is to be maintained.

Lard, vaseline, and lanoline, singly or in combination, enter into the composition of most ointments; the property of protecting, or not protecting, from ultra-violet light, although it is not usually taken into consideration, is not a matter of indifference, and, in certain cases at least, the composition of the base would appear to be a matter of some practical importance.

Discussion.—Dr. H. C. SAMUEL asked what was the effect in this respect of calamine lotion. He had a case of hydroa æstivale which only responded to calamine lotion; it was not benefited by any greasy application.

Dr. A. M. H. GRAY said he believed that the experiments in the case shown had been made with direct sunlight; he would like to know what was the reaction of the child's skin to the ultra-violet mercury lamp. A year or two previously he (the speaker) had shown a case of hæmatoporphyrinuria congenita with marked hydroa. The patient's skin could not be tested with direct sunlight, as it was in the winter, but it was tested with the carbon arc and the mercury vapour lamp, and the reaction was no greater than that of the controls. Thus the suggestion was that the reaction was not due to ultra-violet radiations, but to rays in the visible spectrum. If the vesiculation was not due to the ultra-violet radiations, how did the exhibitors explain the action of vaseline in preventing the formation of these vesicles? The matter was an interesting one, and required working out from that point of view.

Dr. WILFRID FOX said it had been known since 1860 to those who did much mountaineering that yellow vaseline was the best thing to apply to prevent bad sunburn. He (the speaker) had learned this from the late Mr. Clinton Dent, who was a member of the old Dermatological Society of London, and also President of the Alpine Club. With regard to hæmatoporphyrin, he had never found it in these cases.

Dr. A. C. ROXBURGH said that in Vienna he had found two proprietary preparations largely used both for protecting patients' skins from ultra-violet light from the mercury vapour lamp and for protecting the skin during mountaineering. The more effective was the "ultra-zeozone," said to be made from the brown pigment in horse-chestnut buds. The other, "antilux," was said to be composed of β-naphthol in lanoline. Testing with the tungsten arc lamp there was little to choose between the two preparations, but the second named was much the cheaper.

Dr. H. W. BARBER said that this subject had been carefully studied by one of his assistants, Dr. Howitt, who chose it for his M.D. thesis. He confirmed what Dr. Gray had just said, that the juvenile type, at least, did not react to ultra-violet rays. Dr. Howitt found it was the violet and blue rays rather than the ultra-violet which caused the reaction. The same worker found that both adults and children with this condition fluoresced more than did normal persons. His own experience of adults who were light-sensitive was that they were usually sallow and hyperpigmented people. Red-haired people, who pigmented badly, usually developed a solar erythema, such as was seen in normal persons after they had been exposed to ultra-violet light, rather than the polymorphic eruption that was characteristic of acquired light-sensitiveness.

Dr. J. M. H. MACLEOD said that he had tried to produce a reaction in a vitiligo patch by means of a small tungsten electrode lamp and a mercury vapour lamp. With an exposure sufficient to cause a blister on normal skin only a slight erythema resulted.

Dr. PEACOCK (in reply) said that calamine lotion fluoresced, but not so brightly as yellow vaseline. The explanation of that was not entirely connected with ultra-violet rays. Lanoline cut out more of the visible spectrum than did vaseline. Pigmentation and fluorescence seemed to bear an inverse relationship to each other. In the child exhibited there was very little power of pigmentation in the skin, and that might be why she was badly protected.

Case of Circumscribed Sclerodermia associated with "White-spot Disease."

By H. W. BARBER, M.B.

E. C., FEMALE, aged 8½. Patches first noticed on left thigh last October.

Previous diseases.—Chicken-pox.

Present condition.—There was a series of patches of morphœa, with characteristic "lilac-rings" arranged in a linear fashion along the inner surface of the left thigh. On the upper part of the back are several slightly raised white spots.

Case of Lichen Nitidus.

By H. W. BARBER, M.B.

L. C., FEMALE. Eruption first appeared after vaccination eighteen months ago. Elbow first affected: then knees.

Present condition.—Symmetrical patches *palms*, *antecubital fossæ*, *elbows*, *knees* (disappearing), *popliteal spaces*, *groins* and *neck* (disappearing).

In these situations the eruption forms confluent, slightly scaly areas, but the papules can be seen at the periphery and when the skin is stretched.

With regard to the histology, in the first case I had of this type the section was quite typical; there were circumscribed areas of tissue lying just below the epithelium, rather recalling a tuberculous structure. The sections I have had from this case, though not quite so typical, showed the same type of cell infiltration just beneath the epidermis at various points.

Case of Lupus Erythematosus.

By HALDIN DAVIS, F.R.C.S.

THIS patient, a man aged 58, has spent most of his active life in China. The case is remarkable for the wide extent of the lesions which are present on the neck, the ears and the scalp, also on the chest and on the hands. The eruption has lasted five years. The patient had syphilis twenty years previously. The exhibitor thought that there was some risk that the condition might become generalized and acute.

Chronic Purpura, Cutaneous "Striæ," and Grave Endocrine Disturbance.

(ABSTRACT.)[1]

By F. PARKES WEBER, M.D.

THE patient, Mrs. E. B., aged 28, an Englishwoman, suffers from a "coarse," plethoric-looking type of obesity (but she weighs only 54 kilograms, her height being 159 cm.), chronic purpura, and large "striæ cutis distensæ" of the trunk and limbs. The purpura recurs from time to time in the form of "crops" of cutaneous petechiæ and ecchymoses. Constriction of the veins of the arm at once produces ecchymoses. The "striæ" are of different dates and vary correspondingly in colour, the newer ones being purplish, the older ones paler. The blood-pressure is high, the brachial systolic blood-pressure ranging from 205 to 230 mm. Hg. The urine contains a little albumin, but is practically free from tube-casts. The administration of 100 grm. of

[1] The full account of the case will be published, with illustrations, in the *British Journal of Dermatology and Syphilis*.

sugar causes the appearance of a little sugar in the urine. The blood-sugar, when fasting, is within normal limits, but the curve after the administration of sugar by the mouth reaches its maximum height only after two hours. The blood-count shows nothing special, excepting a moderate polymorphonuclear leucocytosis, possibly connected with pyorrhœa alveolaris; the thrombocytes are 180,000 to the c.mm. of blood. There is double optic neuritis, no tumour can be felt by abdominal palpation and no enlargement of the spleen or liver can be made out; by vaginal examination the uterus is like that of a nullipara; there is no sign of any intra-thoracic disease. By Roentgen-ray examination the *dorsum sellæ turcicæ* gives only an extremely faint shadow, but the pituitary fossa appears of normal size. There is slight bilateral exophthalmus. The basal metabolic rate (for the estimation of which I am indebted to Dr. Langdon Brown) is 20 per cent. above the normal. No disease of the sphenoidal or other air-sinuses can be made out. There is slight hairiness of the chin and upper lip. The Wassermann reaction is negative. In the examination of the case I am greatly indebted to Dr. F. E. Loewy, one of the resident medical officers at the German Hospital.

The *history* is that the patient was a twin, her fellow twin being born dead. Her father and mother are both living, aged 56 and 49 respectively, and the blood-pressure of each of them is high. They have had eleven children, of whom only four are living, namely, the patient, a pair of twins aged 20½, and a girl aged 17½. The patient used to have a rather thin face (according to a photograph) without any degree of exophthalmus. She was married in December, 1922, and has never been pregnant. Her menstrual periods ceased suddenly about September, 1921, but she had three slight periods again after her marriage. Since then (March, 1923) there has been complete amenorrhœa. About March, 1922, she already began to get fatter, especially in the face and abdomen. But it is only since about March, 1923, *with the onset of permanent amenorrhœa*, that her chief symptoms have gradually developed: headaches, pains in her whole body, attacks of dyspnœa (accompanied by a sensation of suffocation), feelings of sickness (for which she sometimes induces vomiting by putting her finger in her mouth), frequent slight epistaxis, the "coarse" type of obesity already mentioned, the cutaneous "striæ," the purpura, the slight exophthalmus.

The case resembles closely one recorded by Harvey Cushing ("The Pituitary Body and its Disorders," 1912, p. 217, Case 45—a "polyglandular syndrome") and H. G. Turney's case, in *Proc. Roy. Soc. Med.*, 1913, v, (Sects. of Neur. and Ophth.), p. 69.

Postscript (September, 1925).—The patient died on July 4, 1925, in an attack clinically resembling acute pulmonary œdema. She had previously had a similar attack on May 30, which had been apparently relieved by blood-letting. The post-mortem examination (Dr. E. Bock) showed that there was no tumour in the cortex of either suprarenal gland, but excess of the medullary substance. The left ventricle of the heart was hypertrophied. There was slight chronic interstitial nephritis. In the anterior lobe of the pituitary gland was a minute adenoma consisting of basophil cells. No disease of the thyroid gland or of the ovaries.—F. P. W.

Leucoderma and Melanoderma.

By S. E. Dore, M.D.

Patient, a female aged 55, has suffered from vitiligo for twenty-four years and now presents typical patches of the condition on both hands. About six months ago well-defined, symmetrical, circular, deeply-pigmented patches developed on the lower half of both ears and the skin of the cheeks adjoining them. The shape and size of these lesions correspond fairly accurately to those of the head-phones which

she has been using constantly for "listening-in" during the past two years; and though the melanoderma associated with leucoderma does not commonly occur at pressure points, the pressure of the head-phones in this case may have determined pigmentation.

Discussion.—Dr. F. PARKES WEBER asked about the possibility of Addison's disease in this case. In that disease there might perhaps be increased pigmentation over pressure or friction areas. In a few recorded cases of Addison's disease there had been vitiligo-like patches of leucoderma present.

Dr. DOHI (Tokyo) said that in Japan ultra-violet light was used for cases of leucoderma, and some of the cases improved.

Dr. DORE (in reply) said that no particular investigations had been made, but there were no obvious symptoms of suprarenal disease. He would be glad to know whether any observers present had had good results from suprarenal therapy in cases of leucoderma. He had tried both this and ultra-violet light and had never produced any improvement. His impression was that With had had similar disappointing results from ultra-violet light treatment.

The President (Dr. GRAHAM LITTLE) exhibited cases of (1) Parapsoriasis, (2) von Recklinghausen's Disease, (3) Sarcoid, (4) Lupus showing Spontaneous Resolution.

SECTION OF ELECTRO-THERAPEUTICS.

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Section of Electro-Therapeutics.

President—Dr. STANLEY MELVILLE.

On Respiratory Excursion of the Mediastinum and some Allied Phenomena.

By E. RIST, M.D. (Paris).

PERMANENT displacements of the mediastinum to the right or to the left as a consequence of various morbid conditions of the respiratory organs have been described even by the older clinicians and pathologists; the mechanism of such displacements and their diagnostic significance are well known, and insisted upon in all our text-books. But not until fluoroscopic examination of the chest became a clinical method did we suspect that there is such a thing as rhythmical excursion of the mediastinal organs, synchronous with the respiratory movements. The first and rather casual mention of the phenomenon occurs in a paper published by F. O. Jacobson in 1899. It was described independently with great accuracy the same year by Holzknecht of Vienna, and in 1900 by Béclère of Paris. Holzknecht, like Jacobson, considered that respiratory excursion of the mediastinum was conditioned only by stenosis of one of the main bronchi. Béclère showed that it occurred even more frequently in unilateral fibrosis of the lung. Holzknecht finally agreed with Béclère's opinion, and subsequently published several cases of respiratory excursion of the mediastinum in the absence of bronchial stenosis. Later on, Béclère also showed that the phenomenon could be observed in pneumothorax. Bronchostenosis, unilateral pulmonary fibrosis and pneumothorax are the three conditions in which respiratory displacement of the mediastinum can be observed. I know of no other.

Let us first briefly describe the sign. If we study on the fluoroscope the respiratory movements of a normal individual, we notice that the situation of the heart in relation to the median line remains unchanged during inspiration as well as during expiration. But in certain morbid conditions we see the shadow of the heart displaced *in toto* towards one side during inspiration and returned to its original position during expiration. A close observation shows that not the heart only, but the whole mediastinum, takes part in the displacement. But points nearer to the diaphragm have the larger excursion. The shadow of the mediastinum moves like a pendulum hanging down from the upper part of the chest. It is a very conspicuous movement. The displacements of the edges of the heart-shadow can often be measured by more than an inch.

One can easily understand how the phenomenon can be produced by a stenosis of one of the main bronchi: the inspiratory expansion of the chest determined by muscular action is equally powerful on both sides; the fall in intrapulmonary pressure is rapidly compensated on the sound side, where the calibre of the main bronchus is normal, and much more slowly on the side of the stenosis. The intra-

pulmonary pressure is therefore lower on the diseased than on the sound side, and consequently the mediastinal partition is pushed from the side of the higher to the side of the lower pressure. During expiration the conditions are reversed; the higher pressure is on the side of the stenosis; the mediastinum is displaced towards the sound lung. The amplitude of the excursion in each individual case is, of course, influenced by several factors, among which are the mobility of the mediastinum, the narrowness of the bronchial stenosis and the depth of inspiration.

If we ask a patient affected with unilateral bronchostenosis (be it due to an internal cause, like syphilis, bronchial cancer or foreign body, or to an external cause, like aneurysm or mediastinal tumour) to take first a deep breath and then to remain in apnœa, it is quite evident that after a short time the intrapulmonary pressure on the diseased and on the sound side will be equalized, because they will both be equal to the atmospheric. As a consequence, the mediastinum, which has moved towards the diseased lung, will, as soon as inspiration has come to an end, tend to resume its normal position and move back slowly towards the middle line. This is quite apparent on fluoroscopic examination. Béclère, who first made this little experiment, remarks that it may be used clinically in discriminating cases of respiratory displacements of the mediastinum produced by bronchial stenosis from cases in which the phenomenon is due to another cause. As a matter of fact, bronchostenosis is a rather uncommon occurrence, whereas respiratory displacement of the mediastinum is by no means exceptional.

But it is more ordinarily caused by one of the two other circumstances already mentioned. Let us first consider the case of unilateral fibrosis of the lung. It may be due to tuberculosis or to some other disease; those fibrotic conditions of one lung which may follow lobar pneumonia in the adult or bronchopneumonia in the child, and are so often complicated with bronchiectasis, are among the more frequent causes of the sign. It may be observed, also, after gunshot wounds and in many other circumstances, which are all characterized by the fact that the lung is shrunken, and that, though still elastic, it is much less easily distended than a normal lung; it has become rigid to a certain extent. In a normal chest, the action of the inspiratory muscles is transmitted to the mediastinum through the lungs. A certain amount of the energy thus transmitted is spent in distending the elastic lung, the traction on both sides of the mediastinum is the same, and the mediastinum remains therefore motionless in the median line. If one lung is fibrotic and therefore not distensible, it will transmit the muscular traction to the mediastinum almost integrally, whilst the sound lung absorbs part of the energy for elastic distension. Consequently the traction on the fibrous side will be stronger than on the sound side, and the mediastinum will be moved towards the side of the stronger traction, i.e., towards the diseased side.

It is by far the most constant and the most reliable sign of unilateral pulmonary fibrosis I know. It is often coincident with a permanent displacement of the mediastinum. The respiratory excursion is proportioned to the density of the diseased lung and to the amount of its shrinkage. It is quite independent of pleural adhesions; there are, of course, many cases in which fibrosis of the lung is associated with an adhesive pleurisy. But it is by no means the rule. The practice of artificial pneumothorax has shown that a lung may be very much shrunken and the mediastinum permanently displaced and subject to inspiratory excursion, whilst the pleural space remains perfectly free.

In spontaneous pneumothorax respiratory excursions of the mediastinum are often observed. In artificial pneumothorax they are almost constant. We must remember that artificial pneumothorax is a closed pneumothorax. During inspiration, the pressure on the sound side of the mediastinum is equal to the atmospheric pressure *minus* the elasticity of the lung. It is approximately constant, the lung

cavity communicating with the outer atmosphere through the bronchi and the trachea. On the pneumothorax side we have a gas-containing pleural cavity which is absolutely closed. The inspiratory expansion of the chest tends to lower the pressure in the pneumothorax cavity. The mediastinum is pushed from the side where pressure is constant towards the side where pressure is gradually diminishing. The phenomenon occurs whatever may have been the initial position of the mediastinum and whatever may be the average pressure in the pneumothorax cavity.

The physicians who treat pulmonary tuberculosis by artificial pneumothorax and who are accustomed to control the treatment by frequent fluoroscopic examinations have great opportunities for studying the respiratory excursions of the mediastinum. A careful observation will show them many other curious and interesting phenomena which are more or less intimately connected with it. The best known among them was described for the first time in 1898 by Kienböck (of Vienna) under the title of "paradoxical movement of the diaphragm." It has been the subject of many a discussion and has received widely divergent interpretations. Let us consider it for a moment.

What Kienböck originally described were cases of spontaneous pneumothorax complicated with a liquid effusion, or hydropneumothorax. The fluoroscopic aspect of such cases is very striking. The effusion gives a dark shadow the upper limit of which is strictly horizontal and exactly represents the level of the liquid. It remains horizontal, whatever position may be given to the chest. During inspiration the horizontal level is lifted upwards; during expiration it is moved downwards. The excursions of the level are therefore exactly opposite to the excursion of the diaphragm on the sound side.

Curiously enough Kienböck himself, and all the observers who after him studied this so-called paradox, implicitly admitted that it was due to a disturbance in the motion of the diaphragm on the diseased side; this preconceived idea that the primary trouble was an altered motion of the diaphragm has led to several explanations of the mechanism of the phenomenon, which, in my opinion, are erroneous.

Kienböck, for instance, supposed that under the weight of the fluid the diaphragm turned its convexity downwards into the abdomen, instead of maintaining it upwards into the chest. The inspiratory contraction of the inversely curved muscle would of course lift up the fluid. One could perhaps imagine the left half of the diaphragm being made convex downwards. But the right half of the muscle has to model its shape on the upper surface of the liver, which is convex, and no amount of fluid in the pleural cavity would be able to carve a hollow in the liver. Now the upward inspiratory motion of the fluid is a regularly observed phenomenon in hydropneumothorax, whatever side be affected. Furthermore, fluoroscopic examination ought to show us the strange deformity of the diaphragm presupposed by Kienböck's theory. On the right side the shadow of the effusion is not distinguishable from the shadow of the liver. But on the left side, whatever may be the quantity of fluid in the pleural cavity, the sharp dark line of the diaphragm is almost always visible, because of its contrasting with the transparent air-chamber of the stomach. I have never seen any alteration in the shape of the left diaphragm in left-sided pneumothorax, and I do not know of any published case in which such an alteration has been described.

Another explanation takes it for granted that the presence of fluid on the upper side of the diaphragm must determine a paralysis of the affected half of the muscle. Being in consequence unable to contract during inspiration, it is pushed up passively by the increased abdominal pressure caused by the energetic downward inspiratory excursion of the sound half-diaphragm. But such a paralysis seems very unlikely and has never been proved. It is very easy to prove on the contrary that the

normal movements of the diaphragm are preserved in hydropneumothorax. Let us consider the case of left-sided hydropneumothorax and examine it fluoroscopically. If there is a well formed gastric air-chamber, we shall see the diaphragm moving downwards during inspiration, whilst the level of the fluid in the pleural cavity is lifted upwards. There are also many cases of artificial pneumothorax in which the lower part of the lung is strongly adherent to the diaphragm and is therefore not collapsed. The pneumothorax is incomplete and does not reach the diaphragm. In such an incomplete pneumothorax a fluid effusion may collect. Its horizontal level very distinctly shows the upward inspiratory movement described by Kienböck, and yet the fluoroscope shows a perfectly normal downward inspiratory excursion of the diaphragm.

We may therefore confidently assume that the movement of the fluid level has no relation whatever with the diaphragm, and is absolutely independent of the diaphragmatic excursion. The "paradoxical movement of the diaphragm" is a misnomer. The phenomenon described by Kienböck is due to another mechanism. If in a closed cavity we have a fluid mass surmounted by gas, the level of the fluid can be heightened only if we shorten one or several dimensions of the cavity. Shortening of the vertical dimension is out of the question as we have just seen. But shortening of the transverse dimension occurs regularly and necessarily, as we have seen before, in pneumothorax. It is caused by the inspiratory lateral excursion of the mediastinum, and it is no paradox. I think this explanation, which is founded on facts only and does not require any auxiliary hypothesis, is quite satisfactory, but I dare not hope that it will put an end to the extraordinary theoretical discussions which have been aroused through the phenomenon described by Kienböck.

Now let us observe on the fluoroscope the movements of the collapsed lung. If the pneumothorax is a recent one, if there has been no fluid effusion and therefore no thickening of the visceral pleura, we shall often have the opportunity of seeing some very curious movements. Of course the lung participates in the pendulum-like respiratory excursion of the mediastinum: it is moved sideways to and fro towards the pneumothorax during inspiration, towards the sound side during expiration; but it also has movements of its own. We may observe, for instance, the inspiratory distension of the lung as distinctly as in Funke's classical experiment. When the inspiratory expansion of the chest lowers the pressure in the pleural cavity the elastic lung expands. Very often the tuberculous lesions which have led to the induction of artificial pneumothorax are localized in one lobe only—say the upper lobe. It is interesting to see that the diseased upper lobe which has lost its elastic distensibility does not seem affected by inspiration and remains entirely collapsed, whereas the lower lobe, which is sound, shows an evident inspiratory expansion. Of course the production of the phenomenon is more or less conditioned by the degree of the intrapleural pressure. In cases in which a pressure well above the atmospheric is maintained, one can see no inspiratory expansion.

There is also a systolic distension of the collapsed lung which is sometimes very conspicuous. It is only seen in incomplete collapse, and strikingly illustrates the influence of the systolic blood-wave on the lung volume.

And, lastly, one may sometimes observe a regular vibratory movement of the lung, the rhythm of which is about two or three times as fast as the heart rhythm. The semi-collapsed lung is an elastic body suspended freely in the gas-containing chest cavity. It is rhythmically struck by the heart-beats and responds by a rhythmical vibratory movement of its own.

All these different movements may be observed separately or conjointly; when two or three of them happen to be produced simultaneously the result is very complex and somewhat confusing; but one soon gets accustomed to interpret them. They well deserve attention, and their demonstration to a class of junior students would, I think, be valuable in physiological as well as in clinical teaching.

Now let us examine how the movements of the heart are affected by pneumothorax. Spontaneous pneumothorax is generally complicated by an effusion of fluid, the dark shadow of which obscures the edge of the heart-shadow and makes observation of its shape and motion very difficult or even impossible. But in the majority of cases of artificial pneumothorax there is no such effusion; the edges of the heart-shadow are very distinctly seen on the fluoroscope and are easily observed.

We notice at first view the pendulum-like excursion of the heart *in toto* which we have already mentioned. On closer examination we are struck by another phenomenon, which I described for the first time in 1913, and which, in my opinion, curiously illustrates the influence of the elasticity of the lung on the mobility of the heart-muscle. It can be briefly described in the following way:—

Suppose the patient behind the fluoroscope has an artificial left-sided pneumothorax. The right edge of the heart-shadow will be seen beating normally, but on the left edge the systolic contraction and the diastolic expansion of the heart are characterized by an extraordinarily increased amplitude, such as we never observe in a normal chest. The contrast between the action of the heart on the normal side and its enormously exaggerated action on the pneumothorax side is so conspicuous that it is hardly conceivable that it should have escaped notice so long. The phenomenon is reversed, but equally striking when the pneumothorax is on the right side. It is not affected by the amount of gas contained in the pleural cavity or by its pressure. One misses it only when the gas is prevented from penetrating between the heart and the lung because of adhesions of the mediastinal pleural space. As soon as the heart is separated from the lung by gas, the heart-beats become considerably amplified. This exaggerated amplitude of the heart-beats on the affected side is the best and most constant sign of a mediastinal pneumothorax. In the physiological experiment, when we observe the heart of a dog whose chest has been opened and whose lungs are alternately inflated and deflated by means of an artificial respiration apparatus, the heart-beats have the same exaggerated amplitude—an amplitude which greatly exceeds the normal amplitude which we can observe fluoroscopically in the normal dog. We must conclude, therefore, that the normal contact of the lungs with the heart acts, so to speak, as a brake on the systolic contraction. This is further supported by some other observations.

Let us return to the study of our left-sided pneumothorax patient whom we left behind the fluoroscope. At the beginning of inspiration his heart is approximately on the median line; between its left edge and the partly collapsed lung there is an abnormally transparent space showing that the gas has penetrated into the mediastinal pleura. The heart-beats on the left edge are violently exaggerated. While inspiration goes on, the excursion of the whole heart towards the left becomes conspicuous; it is more pronounced than the similar excursion of the collapsed lung. Therefore, before the end of inspiration the heart again comes into contact with the lung. As soon as this happens we notice that the heart-beats are much reduced in amplitude; they are normal again. Then expiration begins. The collapsed lung is slower than the heart in returning towards the median line; it again separates from the heart, and immediately we see the heart-beats becoming widely exaggerated. On the right edge, on the contrary, the heart-beats remain normal all the time. The brake-like influence exerted by the contact of the lung on the contraction of the heart is very strikingly demonstrated by this little fluoroscopic experiment.

Now suppose we suppress the contact with the lung not on one side only, as in artificial pneumothorax, but on both sides. I do not think anyone would like to perform a bilateral artificial pneumothorax in order to see what would happen. But there is a rare condition of which we may profitably take advantage for observation, and this is pneumopericardium. I think it was Professor Petrén (of Lund) who for the first time noticed, when examining with the fluoroscope a case of spontaneous pneumopericardium, that the heart-beats had on both sides of the heart-shadow an extraordinarily exaggerated amplitude. But he did not attempt any explanation

of the phenomenon. He did not know of its one-sided production in the case of pneumothorax. I have twice had an opportunity of tapping a serous effusion in the pericardium and refilling the pericardial cavity with nitrogen. On fluoroscopic examination I saw a marked exaggeration of the heart-beats on both edges of the heart-shadow. After some days, the gas had been absorbed and the amplitude of the heart-beats was restored to normal.

How is this to be explained? Let it be understood first, that there is not the slightest functional disturbance of the hearts' action coincident with this strange phenomenon. I have studied a great number of such cases without being able to make out any change in the rhythm or in the mode of the heart's contraction. The electrocardiogram is constantly normal. There is no disturbance in the normal respiratory variation of the blood-pressure. Clinical experience proves that patients can be treated for years with artificial pneumothorax without ever showing any sign or symptom of altered function of the heart. But we know that the elasticity of the lung is a permanent force responsible for the negative intrathoracic pressure. We know that the elastic traction of the lungs exerts itself on the chest-wall and also on the mediastinum, on the heart and on the large intrathoracic vessels. The part played by the increased elastic traction of the lungs during inspiration in facilitating the return of the venous blood to the heart is a classical example of the physiological correlation between circulation and respiration. Marey, and later on, Tigerstedt, have insisted upon the idea that the inspiratory distension of the lung must to a certain extent counteract the systolic contraction of the heart. In the fluoroscopic experiment which I have just described this brake-like action of the elastic lung is made clearly visible. As soon as the lung is separated from the heart by the interposition of a layer of gas, the brake is suppressed, and the heart begins to beat, as if it were isolated, with its proper amplitude.

Before concluding, I should like again briefly to discuss the case of respiratory excursion of the heart caused not by pneumothorax, but by fibrotic degeneration of one lung. You will remember that the fibrotic lung, being more rigid and less distensible, attracts the whole mediastinum during inspiration. What would happen if *both* lungs were fibrotic? It is a condition not frequently met with in clinical experience, but we all know of its existence. In some circumstances tuberculosis of the cirrhotic type is the cause of a generalized fibrotic degeneration of both lungs. Prolonged exposure to mineral dusts also produces a similar degeneration, and we know that it usually ends fatally for the heart. The common explanation given in order to account for the heart-failure in such cases is that the blockage of the lung-capillaries is an obstacle to a normal pulmonary circulation, and that it increases the work of the heart-muscle, which finally becomes unequal to its task. This theory has always appeared to me very unsatisfactory. I admit that fibrosis may obliterate quite a number of lung-capillaries; but there are always compensatory dilatations of other capillaries. Moreover, it seems very likely that in artificial pneumothorax the lung-circulation is almost entirely blocked in the collapsed lung, i.e., in one-half of the respiratory apparatus, and yet it never brings about dilatation and failure of the heart, even when the collapse is maintained for many years. One would also expect that an increase in the work of the heart-muscle due to circulatory blockage would first result in a compensatory hypertrophy of the right heart. But we know that it is not so. There must be some other circumstance to account for the heart-failure which, in the long run, so invariably follows generalized fibrosis of the lungs.

Let us examine such a case by means of the fluoroscope. The increased inspiratory traction is now exerted by both lungs on both sides of the heart. It does not result in a lateral excursion of the heart as in one-sided fibrosis. The heart remains unmoved in the middle line. But, when the chest-wall expands, something

inside the chest has to give way. It cannot be the lungs: they are cirrhotic and much more resistant to distension than in a normal person. *It is the heart which gives way.* And therefore we may observe on the fluoroscope the phenomenon of the *inspiratory distension of the heart.* I have had several opportunities of seeing, and even of measuring, the inspiratory increase of the area of the heart-shadow in cases of generalized fibrosis of the lungs. It is clearly seen in the frontal view; but if one examines the patient obliquely or laterally, it is also quite noticeable. We may assume, therefore, that the distension is produced along all the diameters of the heart.

Probably it may be regarded as the exaggeration of a normal phenomenon. Tigerstedt considered that there is always a certain amount of inspiratory distension of the heart, as a result of the normally increased inspiratory elastic traction of the lung. But, on fluoroscopic examination of normal persons, the lateral distension of the heart is more than compensated by the vertical elongation of the heart due to the downward movement of the diaphragm. In the case of generalized fibrosis of both lungs on the contrary the phenomenon appears very distinctly. The increase thus produced in the brake-action of the lungs results in an ever-recurring increase in the muscular work of the heart, and one can easily understand that it finally imposes upon the heart an effort which it is unable to sustain. I venture to suggest that this explanation of heart-failure in chronic generalized fibrosis is more satisfactory to the questioning mind than the generally adopted traditional theory.

I should be gratified if I have succeeded in showing that fluoroscopic examination of the chest is a useful auxiliary in diagnosis. Its uses indeed are still wider than that. It opens up new ways for clinical and also for physiological and pathological investigation. It raises new problems and helps us to solve them. The clinician and the radiologist are, too often, still accustomed to work apart, and to correspond only by written reports. Medicine would greatly profit, I am sure, if they met personally, and, I venture to add, there should be no harm in their meeting in one and the same person.

Section of Electro-Therapeutics.

President—Dr. STANLEY MELVILLE.

The Radiological Investigation of Infantile Pyloric Stenosis and Pylorospasm.

By CECIL G. TEALL, M.B.

THE object of this paper is to record the experience gained at the Children's Hospital, Birmingham, in the radiological examination of infants suffering from pyloric stenosis and the allied condition, pylorospasm. During the past three years I have examined the majority of the patients admitted to the hospital for these conditions—in all 120 cases. Almost all of them have been under the care of Dr. Leonard Parsons, the Senior Physician to the hospital, who includes the radiological examination in the routine investigation of these cases.

It should be understood at the outset that I am not suggesting in the majority of the cases that radiography is necessary to establish a diagnosis; this can usually be done on clinical grounds. The value of the radiological examination has been very differently estimated by writers on this subject. Variot [1] even goes so far as to say that it constitutes the most valuable single piece of evidence in a case of infantile pyloric stenosis, while Dr. Fildes and Dr. Forest-Smith, in their recent communication to the Section for the Study of Disease in Children [2], say that it is unnecessary in cases where the condition can be recognized clinically.

It is now generally accepted that the best treatment for infantile pyloric stenosis is surgical, and the operation of choice that associated with the name of Rammstedt. Working in conjunction with Mr. Seymour Barling, all Dr. Parsons' cases are so treated, with very gratifying results. In view of the fact that the diagnosis of pyloric stenosis involves what is, for a child so ill, a very serious operation, it is felt that while a clinical examination will almost always suffice to establish the diagnosis, it is essential to make use of every method which will make this more certain, especially as the allied condition of pylorospasm does not call for surgical treatment. It is for this reason that the radiological examination is made, and in my opinion should always be made, whenever possible. Dr. Parsons, in his Goulstonian Lectures this year [3], made the statement that "even in ordinary cases it will tell us more than we can discover by clinical examination." While I cannot agree with Variot, I feel that the radiological examination constitutes a most valuable means of confirming the diagnosis. In only one case sent for operation at the Children's Hospital during the last three years was no evidence of stenosis found. In this case the radiological examination had not been made, and I am convinced that had the child been so examined the fact that no stenosis was present would have been established beforehand. If only on rare occasions the X-ray examination will serve to correct a diagnosis, in my opinion that in itself is sufficient to justify its use in every case.

Dr. Poynton, Mr. Higgins and Dr. Brydson, in a paper published in the early part of the year [4], said that "we do not as a rule give bismuth meals, because our experience has been that they may do harm to an infant who has no reserve power." I quite agree that were this so it would be a very strong argument against the radiological investigation, but I think their experience must have been unfortunate. In no case in the whole series examined at the Children's Hospital, in Birmingham, has any untoward result been observed, and I am quite certain that with ordinary care there is no reason why the child should be any the worse for the examination.

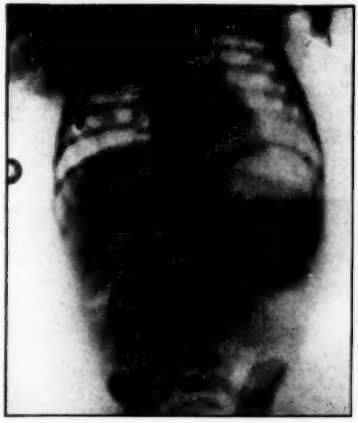
The method I have adopted is as follows: The meal takes the place of an ordinary feed, and the child is given about a teaspoonful of barium sulphate in an ounce of milk and an ounce of water. No attempt is made to fill the stomach with barium; sufficient only is given to act as an indicator of the rate and regularity of emptying. The size of the stomach can quite easily be seen after an ordinary feed by the air content, and it is unnecessary to give barium if it is required merely to settle this point. At first the patients were bottle-fed, but with this method I found there was a tendency to vomit and it was soon given up in favour of spoon feeding. Since this has been done I have had no trouble with vomiting and I have experienced no difficulty in giving the necessary amount.

As soon as the meal has been taken a radioscopic examination is made with the child lying face downwards. The appearance approximates to that seen when the child is held upright, in which position I have not found it practical to examine such tiny patients. Either holding them up by hand or binding them to a board which can be held vertically appears to cause a good deal of upset, with no advantage so far as I can see.

In a case of pyloric stenosis the stomach is seen to be enlarged. It is possible to watch the exaggerated peristalsis, such as is observed clinically, and, further, to see that in spite of active peristalsis with deep waves none of the barium passes through the pylorus. The rate and regularity of emptying are observed by making a series of films at a quarter of an hour, one hour, two hours, three hours, and four hours after the feed (fig. 1). Sometimes a film is made the following morning, about twenty hours afterwards. When I commenced to examine these children I made a number of observations as to the rate at which the stomach emptied in normal infants. I found that this varied a good deal, but the stomach was usually empty two hours after the meal. Using this as a standard of comparison, at first I watched the patients suspected of having pyloric stenosis for two hours. This standard appears also to have been adopted by other observers, but quite early in my series I had a case which made me realize that this time was not long enough. One of the children sent for examination showed practically complete stasis two hours after taking the meal, and I diagnosed pyloric stenosis. Clinically, the case was not one of stenosis and was not sent for operation. Shortly afterwards the child died, and the post-mortem examination showed clearly that there was no stenosis at the pylorus, nor was there anything else abnormal to be found in this region. There is no doubt that the case was one of pylorospasm, and in the light of further experience I think that if films had been made up to four hours after the meal the stomach would have been found to be empty.

In infantile pyloric stenosis there is always definite delay in the emptying of the stomach; not only at two hours, but at three and four hours after the feed most of the barium will still be retained, and there is often quite a large residue twenty hours afterwards. I am convinced that it is essential to make observations for at least three and preferably four hours.

It is not at all uncommon to find in this condition that some of the barium begins to leave the stomach quite soon after the meal has been taken. I have seen barium pass through the pylorus twenty minutes after the meal in a case which showed stasis forty-eight hours later. The important point, I think, is that if the barium begins to leave the stomach early it continues to do so more or less regularly throughout the whole period of the examination, but of course very slowly, so that only a very small proportion has passed through the pylorus in four hours. Some cases of stenosis being less severe than others show a less marked degree of stasis, and it has been suggested by Strauss [5] that by means of the radiological examination it might be possible to discover which cases could safely be treated by medical means. I am quite certain that the radiological examination is of no value in this



(*a*) Immediately after the meal.

(*b*) One hour P.C.

(*c*) Two hours P.C.

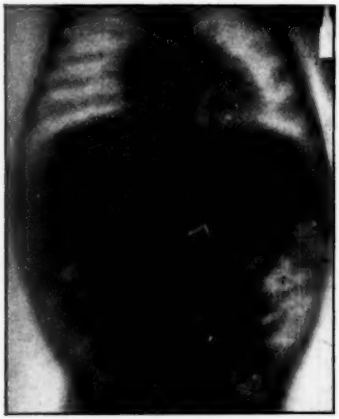
(*d*) Three hours P.C.

(*e*) Eighteen hours P.C.

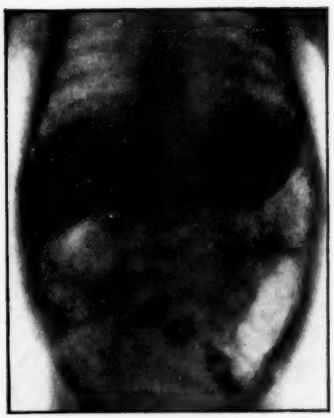
FIG. 1.—Pyloric stenosis. Showing stasis up to eighteen hours after a barium meal.

respect. When stenosis is diagnosed the only treatment which holds out a reasonable prospect of cure appears to be surgical, and it is necessary to realize that one of the reasons why the stomach in a case of infantile pyloric stenosis empties more quickly, may be that the case is a severe one and the child extremely ill and suffering from a terminal diarrhœa.

(*a*) One hour after the meal.



(*b*) Two hours P.C.



(*c*) Three hours P.C.

(*d*) Four hours P.C.

FIG. 2.—Pylorospasm. Showing very little emptying up to three hours after the meal, with rapid emptying commencing between three and four hours afterwards.

It is essential to distinguish between pyloric stenosis and pylorospasm, as I have already said, because the treatment in the two conditions is so different. This can be done clinically in most cases, but Dr. Parsons tells me that in suspected cases of pylorospasm the radiographic examination is especially helpful. The stomach in the case of pylorospasm is not so large as in the case of stenosis, nor is the increased

peristalsis so marked. It is in the type of emptying, however, that the difference between the two conditions is most evident. Frequently during the first two hours there is less emptying than in a case of stenosis, and at this time stasis may be complete, as in the case already mentioned. If the examination is continued to four hours after the feed, it will be found that the stomach, which for two hours has held up the meal, will quite suddenly begin to empty quickly, and before the examination is completed all the barium will have passed through the pylorus (fig. 2). This temporary delay, followed by a rate of emptying which is normal or rather more rapid than normal, completely rules out the possibility of stenosis at the pylorus.

Some authorities have said that pylorospasm as a clinical entity does not occur, but there is abundant evidence to hand that, while in a case of pyloric stenosis there may be some superadded spasm at the pylorus, nevertheless pylorospasm certainly does occur as a separate condition, and further that it is not an early stage of pyloric stenosis. Two of the cases of pylorospasm at the Children's Hospital, Birmingham, have come to post-mortem examination, and it has been possible definitely to establish the fact that there was no pyloric stenosis and also that there was no other organic lesion. In view of the difficulty in separating the two conditions clinically, and in the absence of radiographic examination, it seems probable that many of the cases of "pyloric stenosis" treated successfully by medical means were in reality cases of pylorospasm. That in stenosis there may be superadded spasm is possibly illustrated by one of the cases examined, in which quite half the meal had passed the pylorus in three hours. The child had been treated medically previous to admission but without success, and at the time of examination was very ill. He was suffering from the diarrhœa which in these cases precedes a fatal issue. In this case operation was refused. The child died and the post-mortem examination proved it to be a definite case of pyloric stenosis. An explanation as to why so much of the meal had left the stomach in three hours seems to be that while the child was so desperately ill the pylorus offered no more obstruction to the passage of the meal than was actually caused by the stenosis, and the reason for the more marked degree of stasis in similar cases where the child is not so ill is that there is some superadded spasm at the pylorus.

The points I wish to emphasize are :—

(1) The radiological examination is a valuable method of confirming the diagnosis of infantile pyloric stenosis and should be made whenever possible.

(2) It is possible, radiologically, to distinguish between pyloric stenosis and pylorospasm.

(3) It is essential to make observations on all cases for at least three, and preferably four, hours.

(4) The radiological examination is not followed by any untoward result.

In conclusion I wish to thank Dr. Parsons for allowing me to make use of his cases to illustrate this paper, and for his help on many occasions.

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Two Special Uses of Ordinary Electric Currents.

By JAMES MENNELL, M.D.

I CHOSE the two points that I propose to raise because they seem to me to be so essentially on the border-line between the more specialized branches of the work of your Section and my own work.

There are, of course, countless ways in which electrical treatment can help those who deal more particularly with other forms of physical treatment, and perhaps it is pardonable to say that the reverse is also true; there are many ways in which we can help, even though the more important place in treatment must be by electrical methods.

We have an enormous mass of ground to cover that is common to both of us, and I have always felt that the more we learn of each other's work the better it will be for our patients. I think you will find that the greatest trial of all who deal with physical treatment, whether by electrical or by other means, is that we are considered in many respects as the last resource of the destitute, and our departments tend to be used as the dumping ground for the hopelessly incurable elsewhere. Personally, I never resent this, as, although all too frequently we are unable to cure, we are able to alleviate when all others have failed; and this alleviation brings with it a gratitude from the patient which often exceeds that which we receive when, in more fortunate circumstances, we are able to cure.

(I) Fibrositis.

One complaint of this type is what, personally, I refer to as fibrositis, though cases afflicted in this way are most commonly sent to me described as suffering from muscular rheumatism or neuritis. I need, of course, hardly point out to you that any discoverable septic focus must be eradicated if we wish to cure, such, for instance, as septic tonsils, septic teeth; and—a point I have only learnt from our colleagues at St. Bartholomew's during the last few months—a septic focus in the cervix. But even if every possible avenue has been explored there remain a vast number of cases in which, in our present state of lamentable ignorance, we are unable to trace out the cause.

The sufferings of these patients may be very acute. They are always prolonged, and the disability, even apart from physical pain, is not infrequently distressing. By bitter experience, I have learnt to look for these painful deposits in various definite localities. Why they form there I can only hazard a conjecture, but with only slightly varying regularity they seem to appear in situations in which muscle fibre is passing over into aponeurosis. They may, of course, appear anywhere, but the pain referred to by the patient exhibits an amazing regularity in its distribution.

Starting from above downwards these deposits may be found directly under the scalp, though they seem to be rare except in the temporal region and over the occipital bone. We find them next deep in the muscles of the neck close below the occiput, and then not usually until we reach the level of the third and fourth cervical vertebræ. Though they may occur in any part of the erector spinæ below this, usually near the outer margin of the muscle, they are comparatively uncommon until we reach the lower border of the twelfth rib, and then we seem to skip again to somewhere about the level of the sacrum. Those in the cervical region are very frequently to be found in patients who complain of occipital neuralgia or headache; those over the sacrum produce symptoms usually attributed to lumbago; those in the angle formed by the twelfth rib and the outer border of the erector spinæ produce the curious story that a patient can get up, dress, leave the house, and set out to walk in perfect comfort, but after having travelled two or three hundred yards he begins to suffer pain which, if he perseveres, effectually prevents him from proceeding further. He then has to sit down or stand quite still, perhaps holding on to the palings, for a few minutes, before he is able to proceed again.

Another favourite spot is near the insertion of the levator anguli scapulæ. For some reason I am quite unable to explain, this is frequently to be discovered in patients who complain of musculo-spiral neuritis. In cases of median neuritis, you will constantly find these fibrositic deposits between the body of the scapula and the chest-wall, while pain down the inner side of the arm is usually associated with

a deposit which you can best discover by placing the hand on the opposite shoulder, then pressing in deeply against the chest-wall at the base of the spine of the scapula. Associated with this, as a rule, is an intensely sensitive spot in the third intercostal space in the mid-clavicular line.

It is wearisome to mention all the other places where these deposits may be found. I would therefore only mention three: one, near the front extremity of the ninth or twelfth rib, the former being constantly mistaken as a sign of chronic indigestion. Then there is the gluteal region, not at the crest as is usually taught, but some two fingers' breadth below it and near the great sacro-sciatic notch.

These deposits are of importance because they are the *fons et origo* of what is usually called sciatica.

Then, finally, I would mention deposits in the calf of the leg immediately behind the posterior border of the tibia, as pain from this source is so often confused with the pain of arthritis of the knee. Constantly I have seen patients who have been receiving treatment for the latter, with no benefit for weeks, or it may be months, who have been relieved in a fortnight when these spots are adequately dealt with.

The remedies to rely upon are three in number. The first is drink, and I believe lack of adequate fluid intake to be the commonest cause of the presence of these deposits. Constantly you will find these patients give a history of an intake of fluid of perhaps a pint or a pint-and-a-half a day. The second remedy is heat, though this is useless, or at best palliative, by itself.

There was once a time when these deposits were excised. Those under the scapula have been found, I believe, to be calcareous; but, in the recent state, I gather that they are deliquescent crystals which have lost their crystalline character before they can be examined under the microscope. The only satisfactory method of dealing with them, so far as I know, if you really wish to eradicate them, is by deep friction. But we must emphasize the word "deep"; no ordinary massage has any effect on them whatsoever.

In many cases the spots are far too sensitive to allow of this treatment, and occasionally even heat will accentuate the symptoms. I know little of the effect of diathermy on them, but ionization here finds a definite place, though only if used with a special technique.

When I began to deal with these cases in bulk, I could not fail to recognize that our predecessors, who were so familiar with what they referred to as suppressed gout (which I take it is the same complaint), invariably recognized the value of counter-irritants, usually in the form of blisters or the actual cautery. For obvious reasons these two remedies rendered subsequent treatment by frictions impossible for a considerable time. One realized, of course, that with a galvanic current it is possible to blister a patient, and I conceived the idea that if we could manage to pass a current locally over one of these spots with sufficient intensity, so as just to fall short of actual blistering, we should become possessed of a very valuable form of counter-irritation. This led to the adoption of a definite technique, which was worked out stage by stage at the B.R.C.S. clinic in Kensington.

The point of maximum tenderness is discovered by palpation. Around it on the skin we used to draw a very thin circle of vaseline, the size of the circle varying, but rarely exceeding an inch in diameter or about the size of a shilling, though the size of the hole varies from that of a threepenny-piece to that of half-a-crown. Latterly we have discontinued the use of the vaseline. We then cut a corresponding hole in a sheet of double-sided mackintosh, though if this is not available it is important to use vaseline round the edge of a hole cut in jaconet. The hole in the insulating material is then placed over the spot already marked out on the patient. Care must be taken that the edges of the ionizing pad do not protrude beyond the

outer limit of the mackintosh, and that there are no jagged edges round the hole. Taking scrupulous care to get an absolutely flat surface in contact with the patient, we cover this hole and surrounding mackintosh with the ordinary large ionizing pad, which is soaked in warm tap water. We use twenty-four layers of lint or sixteen of towelling, and greatly prefer the latter. Any convenient spot is selected for the neutral pad, which is usually large, though two discs, as we call them, can be applied at the same time, e.g., one on each side of the patellar ligament or upon opposite shoulders. It seems to be immaterial which pole is used for "discing." On the whole I favour the positive, and the current is passed in the ordinary way. Thus, through a hole an inch in diameter, in favourable circumstances, we advance to giving the patient anything up to 40 milliampères for twenty-five minutes, though in one case we reached 50. The usual current employed is from 15 to 30.

Needless to say, this is not a treatment to play with, as the eye must never leave the galvanometer: but it is often efficacious when, so far as I know, all other remedies fail—even diathermy. There is, of course, no need to remind you of the use of the galvanic needle, but I think you will find that this process of "discing" will render it unnecessary as a rule.

(II) Œdema.

The other point which I thought might be of interest is a brief exposition of my views on massage as a remedy for œdema, generally speaking from whatever cause, except during the early stages after fracture.

You all know how widespread is the belief in hand massage in the treatment of œdema. I do not want to belittle the value of this massage, but I am compelled to say that as a means of getting rid of œdema I have found it very disappointing. So much so is this the case that I have now almost entirely given up the prescription of hand massage for this type of trouble and have adopted what appears to be a much more rational line of treatment. When we remember that œdema is due to lack of adequate circulation of the lymph, and that in ordinary life the flow of lymph is maintained by voluntary muscular contraction, the *rationale* of what I propose will become evident. We wring out one towel in tap water, and bandage it tightly, say, from ankle to knee, generally using a cotton elastic bandage. Underneath the bandage, and over the towel, we apply a gauze electrode, a hand's breadth in thickness, all round the limb just below the knee. We then wring out a second towel, and bandage it in the same way from just above the knee as high as we can up the thigh; and we usually apply the largest size plate electrode to be found over the front of the thigh. We then connect up the electrodes with a faradic coil, and surge the faradic current as strongly as the patient can tolerate.

Compared with the results obtainable by hand massage the results of this treatment are often very striking. All it means, of course, is that we are causing the muscles throughout the limb to contract and relax underneath an inextensible skin; and, if you care to try it, I think you will find that the effect on the lymphatic flow will far exceed expectation.

It will be realized, of course, that the technique of applying the pressure is subject to endless variation according to the part we propose to treat. I would only say that it is essential that the proximal bandage should, if it is possible, extend well above the highest level of the œdema.

Dr. A. R. Friel read a paper entitled "Zinc Ionization in Relation to Chronic Otorrhœa."

Section of Electro-Therapeutics.

President—Dr. STANLEY MELVILLE.

Treatment of Gonococcal Infection by Diathermy.

By E. P. CUMBERBATCH, B.M.Oxon., and C. A. ROBINSON, M.B.Cantab.

THE work in the Electrical Department of St. Bartholomew's Hospital on the treatment of gonococcal infection by diathermy was commenced in 1913. It originated in an investigation on the therapeutic action of the new form of treatment of various forms of infective arthritis. It was found that gonococcal arthritis responded admirably to diathermy, and this observation led to further inquiry into the action of the same treatment on gonococcal infection of other parts, namely, the cervix uteri, Fallopian tubes, ovaries and urethra in the female; the epididymis, testis and vas deferens, prostate, seminal vesicles and urethra in the male. We have applied diathermy to the eye in two cases of gonococcal iritis. By the end of 1923 the cases treated had reached a total of 106. The youngest patient was 2½ years of age and the oldest was 60.

The particulars of the cases and the details of the work will appear in a monograph which is now in the hands of the printers. At the present meeting we shall state the results of our work, describe the methods which we now use and demonstrate the electrodes which we have devised. At the same time we shall make some reference to the subject of the influence of heat on the gonococcus and the use of diathermy in heating the infected tissues.

SENSITIVITY OF THE GONOCOCCUS TO HEAT.

We have perused various works on bacteriology in order to ascertain the temperatures most favourable for the growth of the gonococcus and the minimum degree of heat which will destroy this organism. In Hewlett's "Bacteriology" it is stated that the optimum temperatures for the growth of the organism are from 95° to 98·4° F., although it can grow between 77° and 100·4° F. We have taken the temperature of the anterior two inches of the male urethra in normal subjects and have found that it varies between 93° and 94° F. When chronically infected the temperature does not exceed 95° F. In the female we have found that the average temperature of the urethra is 99·2° F. These facts may account for the greater severity of gonococcal infection of the urethra in males than in females and may also explain why it is possible for the application of diathermy to end the infection of the urethra in the female far more quickly and easily than in the male (as will be described later).

With regard to the minimum lethal temperature for the gonococcus we find that different observers have given different figures. Most workers, though not all, have found that temperatures which are insufficient to damage the living tissues can destroy the gonococcus. Jordan's "Bacteriology" contains the statement that "according to most observers, cultures are injuriously affected by a temperature of 104° to 105° F." In MacFarlane's "Pathology" it is stated that the gonococcus is injured or destroyed between 100·2° and 104° F.

There is, however, another factor just as important as the degree of heat, and that is the duration of the exposure. This factor has not been stated by some experimenters. Santos found that in order to destroy the gonococcus an exposure of seventy-six minutes to a temperature of 109° F., or of fifty-four minutes to a temperature of

111·2° F., or an exposure of thirty-seven minutes to a temperature of 113° F. was necessary. For high temperatures short exposures were necessary, and vice versa.

The above-stated figures, while they show that the treatment of gonococcal infection by heat is a rational procedure, do not afford much guide to the treatment of human subjects. A temperature as high as 115° F. cannot be tolerated, and in certain regions it cannot be attained or even approached. We have nevertheless been able to obtain success with temperatures considerably lower than 112° F., and without prolonging the exposure to fifty-four minutes, or even thirty-seven minutes. We have therefore reached the conclusion that the destruction of gonococci by diathermy is not wholly due to the action of the heat on the organisms themselves, but in part to the action of heat on the invaded tissues. The tissues possess their own methods of dealing with infecting organisms and it is reasonable to assume that these methods are aided by heat. We think that when the heat reaches a high degree its action is more on the organisms, but when high temperatures cannot be attained the heat acts more by increasing the defensive powers of the invaded tissues, although, at the same time, it diminishes the vitality of the invading organisms.

A few words on the diathermy current may not be out of place. This current, oscillating as it does with an enormously high frequency, is unable to stimulate the excitable tissues or produce chemical (electrolytic) action in the tissues. It can therefore be passed through the body and its strength increased to a value sufficient to heat the tissues to a high degree, just as non-living conductors are heated by passing a strong current through them. The oscillations are *sustained*, and they produce a greater quantity and higher degree of heat than that produced by the current from the original high-frequency apparatus in which the oscillations were *unsustained*. Currents which oscillate with high frequency do not behave like other currents with regard to the path which they take between the electrodes. There is much uncertainty regarding the path which they take in the tissues of the body. The haphazard application of electrodes in relation to a part which it is desired to heat is likely to result in failure. Certain rules, learnt by experience, must be followed, and we have relied on the use of thermometers, whenever possible, in devising our methods of applying diathermy to various parts. The details of these methods will be described shortly. Wherever possible we heat the infected tissues to the highest temperature consistent with safety and give relatively short exposures. When high temperatures cannot be attained we lengthen the exposure.

One other fact must be mentioned. The amperemeter gives no indication of the degree of heat reached in the tissues and is quite untrustworthy in the matter of dosage. As will be shown later, dosage must be measured by the patient's sensations, the thermometer and the clock.

Results.

In all our cases of gonococcal arthritis (numbering thirty-nine by the end of 1923) we were able to obtain abolition of pain, reduction of swelling and increased range of movement. These results were obtained, whether the cases were acute or chronic, ambulatory or confined to bed, whether one joint or many were affected or whether the cases were in young children or old people. If the cases were of relatively short duration without permanent organic changes in the joints, all symptoms and signs disappeared. In more advanced cases some swelling and restriction remained, but again there was no more pain. In these cases the disease was arrested ; and it was possible by the application of added physical treatment to procure further usefulness of the joints. This additional treatment did not cause a return of the symptoms. In one patient the partially ankylosed wrist was forcibly moved under anæsthesia, but there has been no return of the arthritis.

In our cases of epididymitis and orchitis (thirty-four at the end of 1923) the results were even more striking. Pain began to subside during the first application of diathermy. It was rarely necessary to give more than three applications. At the end of ten days there was no pain, or tenderness, or swelling. In many of the cases, though not in all, there remained some thickening of the globus minor, but without tenderness on pressure. In advocating the advantages of diathermy in the treatment of the testis, epididymis and vas, we call special attention to the *celerity* with which the new form of treatment brings the signs and symptoms to an end. If the inflammation and infection are quickly brought to an end there is less likelihood of obliteration of the vas taking place, with sterility as a result.

In the cases in which the cervix uteri and female urethra were infected by the gonococcus (without metastasis) we obtained the following results:—

In order to study the effects of diathermy in the treatment of the cervix uteri and female urethra in which these parts alone were infected (without metastasis) we selected fifteen of the cases sent to us from the Centre for the Treatment of Venereal Disease. In all these cases gonococci were present in the cervix or urethra, or both parts, before diathermy commenced. Both parts were submitted to the treatment, and after the course was completed they were examined bacteriologically. In eleven cases gonococci were not afterwards discovered; repeated examination was made in most of the cases over intervals of weeks or months, but the organisms were not found. In the remaining four cases gonococci were found, but the report furnished by the Centre contained the statement that re-infection had probably taken place. The husband of one of these patients was attending the Centre for treatment of gonorrhœa at the time when his wife was undergoing diathermy. He completed his treatment and was finally discharged as cured. The continuation of the diathermy application to his wife was then successful, and gonococci were not discovered. In another case the patient obtained a separation from her husband; diathermy was then successful and gonococci were not found after the treatment was continued. In the two remaining cases the patients came again for treatment at later periods; diathermy was applied and was successful. Apart from bacteriological evidence, there are other indications that it is possible for diathermy to free the urethra and cervix from infection by the gonococcus. Pain and irritation, when present, are always relieved. The cervix regains its normal appearance, and no discharge, pathological in appearance, is seen at the external os if the infection is gonococcal only. In a few cases in which a discharge, purulent in appearance, persisted at the os, organisms other than gonococci were found. In these cases the discharge disappeared after treatment by ionization.

We have applied diathermy to a number of cases of gonorrhœa in women, in whom the Fallopian tubes were infected. The treatment was followed by abolition of the pain and disappearance of the swelling, or its reduction to a small size without tenderness.

In gonococcal infection of the prostate and seminal vesicles, diathermy undoubtedly possesses therapeutic power. This is shown by the fact that metastatic infection is brought to an end after submitting these parts to the treatment; this has been our invariable experience. We have also applied diathermy to the prostate and vesicles in chronic cases of gonococcal infection of these parts, in which treatment by massage and vaccines had failed to remove the discharge or the gonococci contained in it. In these cases, with one exception, the organisms were not found after the application of diathermy. In some cases there remained a morning gleet; in others the discharge disappeared.

The details of all the cases of gonococcal infection of all regions which we had treated by the end of 1923 will be found in the monograph to which reference has been made. During 1924 a number of new cases have been treated, and the results

have been similar to those described. We think that we are justified in stating the following conclusions:—

(1) That diathermy applied to the urethra and cervix in women, or to the prostate and vesicles in men, will bring to an end metastatic infection of joints, bursæ and fibrous tissue. Cure or arrest has been invariable.

(2) That the application of diathermy to the prostate and vesicles and scrotum will rapidly terminate gonococcal orchitis and epididymitis.

(3) That the application of diathermy to the urethra and cervix uteri in women will free those regions from all symptoms and signs of gonococcal infection. If there is no re-infection we believe that diathermy can effect a cure.

(4) That gonococcal salpingitis can be terminated by diathermy if there are no complications and no pent-up fluid in the tubes.

(5) That in gonococcal prostatitis and vesiculitis the application of diathermy will free many cases from discharge and the contained gonococci. In view of the absence of a positive test for absence of gonococci in the infected tissues we are unable to make a conclusive statement before more cases have been treated.

Technique Used in the Treatment of Gonorrhœal Infections by Diathermy.

By C. A. Robinson, M.B.Cantab.

At the commencement of the investigation of the treatment of gonorrhœal infections by diathermy other methods than those about to be described were employed. What follows is an account of those in use at the present time.

Cervicitis and Urethritis.—The great difficulty to be encountered in the treatment of the cervix by diathermy is that of the cervix being insensitive to heat. A temperature high enough to cause coagulation of the tissues in immediate contact with the electrode is reached without sensation of either heat or pain on the part of the patient. These sensations do not occur until the temperature of sensitive structures in the neighbourhood has been sufficiently raised. It follows that a rise of temperature sufficient to cause extensive sloughing may result before any warning is given by the patient.

The temperature reached may be estimated by means of thermopiles or thermometers. The use of these was considered, but was rejected for the following reasons: Thermopiles are unsuitable for clinical work of this description where the failure to record the temperature accurately might be followed by serious destruction of tissue. Thermometers are unreliable and tedious to work with, though we have used thermometers for the last three years for purposes of demonstration and investigation. They are tedious to work with on account of the smallness of the column of mercury making it difficult to read the temperature registered, and they are unreliable on account of the fact that part of the column of mercury sticks and is left behind when the rest of the column recedes on the temperature falling.

We have made use of the sensitive urethra to furnish us with the standard of current which will give the temperature required when applied to the insensitive cervix.

The urethral electrode consists of a piece of thin metal tubing $\frac{3}{16}$ in. in diameter, rounded off and closed at one end. A vulcanite button is fixed $1\frac{1}{2}$ in. from this closed end. The electrode is inserted into the urethra until the button is pressed against the urethral orifice. The end of the tube outside the urethra is open in order to allow of the insertion of a thermometer, and part of the wall of the tube is removed so that the temperature registered by the thermometer may be read. A piece of rubber tubing is slipped over the part of the electrode outside the urethra, which would otherwise be in contact with the inner surfaces of the vulva.

Treatments are given with the patient lying on her back, and a stand placed between the thighs serves to fix the electrode in position. The stand consists of a circular metal base, to the centre of which is fastened a vertical metal rod by means of a piece of vulcanite, and it is thus insulated from the base. Vertical adjustment is obtained by means of a clip sliding on the upright rod. To this clip is brazed a short horizontal bar carrying two short pieces of tubing fastened together at right angles. One of these pieces of tubing has a metal rod sliding through it to provide horizontal adjustment and it is capable of being fixed by a binding screw. The other piece of tubing revolves on the horizontal bar attached to the clip so as to give an adjustment of the angle at which the horizontal sliding rod may be fixed. This also is fixed by a binding screw. A screw terminal for attachment to the diathermy machine is provided. It is placed at the base of the upright rod so that the pull on the cable does not tend to upset the stand.

The indifferent or distributing electrode consists of a metal belt of sheet lead 3½ in. in breadth and several inches longer than is necessary to encircle the waist or pelvis. It is made of two-pound lead, which is thin enough to be easily moulded to the curves of the body. It is fastened by means of a clip made of two pieces of stout brass wire, something like a large safety-pin. Each end of the lead belt is passed through the loop of the clip and is fastened by being doubled over on itself. To the free end of one of the pieces of wire forming the clip is fastened a screw terminal for the attachment of the belt to the diathermy machine. In treating the cervix and urethra the belt is placed round the pelvis.

When the belt and urethral electrode are in position and connected to the machine, the current is switched on and increased until the patient says that it is beginning to produce pain. The thermometer placed in the electrode then records a temperature of just short of 115° F. The more complete the co-operation with the patient in obtaining the point at which the sensation of heat becomes one of pain, and the greater the care taken by the operator, the nearer will 115° F. be reached. Sometimes there will be no difficulty in actually reaching 115° F.

The intensity of current required to produce this temperature varies with different patients and varies with the same patient from day to day. A temperature up to 115° F. causes no symptoms subsequent to the treatment. If this temperature is exceeded there may be some pain on micturition the day following the application. Coagulation of the proteins of protoplasm does not occur till 131° F. is reached.

We have aimed at getting the physiological effects of heat without those of inflammation. No inflammatory effects are produced by heating to a temperature of 115° F. This is shown by the absence of objective and subjective symptoms after the application. After heating to more than 115° F., symptoms of inflammation begin to appear. Micturition may be painful on the day following such a treatment.

As coagulation of the proteins of protoplasm does not occur till 131° F. is reached, there is a margin of safety of 16° F. Seeing that no anæsthetic is used, it is difficult to see how harm can result if the sensations of the patient are thus used as an indication of the temperature reached. This method has been employed for the last five years without any harm having been inflicted in any case.

The treatment of the cervix is always preceded by a treatment of the urethra. The current required to produce the temperature at which the sensation of heat changes to that of pain is noted. It is not sufficient, however, to note this at the first treatment only, and use that current as the standard for subsequent treatments.

A speculum is passed, and the cervix is made to engage in the middle of its upper end. The cervical electrode is then inserted. This cervical electrode consists of a metal rod $\frac{3}{16}$ in. in diameter and long enough, when inserted into the cervical canal, to reach two or three inches beyond the end of the speculum outside the vulva. It is convenient to have the electrode made of soft copper, or better still, of

70 per cent. tin and 30 per cent. lead, so that it can be bent at the end of insertion to a suitable angle at the time of using it. A rubber tube is slipped over it in order to insulate it from contact with the speculum or vaginal wall. The electrode is notched for some inches from the external end at spaces of half an inch, and the notch at the end nearest the end of insertion is marked zero. When the electrode has been inserted into the cervical canal the rubber tube is made to slide down it until one end comes into contact with the external cervical os. The other end of the rubber tube then records the length of electrode inserted. Both the urethral and cervical treatments last ten minutes.

We had been making use of this procedure for two years, estimating that the temperature obtained by this means in both the urethra and cervix was 115° F. before we used thermometers in either case. We then had thermometer electrodes constructed. The urethral thermometer electrode has already been described.

The cervical thermometer electrode consists of a metal tube which contains the thermometer. The part to be inserted into the cervix is set at a suitable angle for this purpose and contains the bulb of the thermometer. It is so arranged that the thermometer can be removed from the electrode so that the latter can be sterilized. Part of the wall of the electrode is removed from that part which will lie outside the speculum so that the reading of the thermometer may be seen. The electrode is inserted and fixed to the stand in the same manner as the cervical electrode previously described. The use of the thermometers with the urethral and cervical electrodes demonstrated that a temperature of 115° F. was actually obtained in each case.

It is not our custom to use thermometers in routine work. The cervical instrument is tiresome and unhandy to work with, for it must be taken to pieces and the thermometer removed for sterilization. In addition, the angle at its proximal end is constant, which is a disadvantage. It has served, however, to show that the temperature in the cervix is remarkably close to 115° F. when the method described is being used. It may be a little above 115° F.—up to perhaps 117° F., but it is often exactly 115° F. This accuracy is somewhat surprising considering the rather inexact estimation of the length of electrode inserted into the cervix.

Although the temperature of the tissues of the cervix in contact with the cervical electrode is known, we do not know exactly to what temperature the rest of the body of the cervix is heated, but at least we know that it is considerable. When a thermometer is inserted into the rectum, during a cervical treatment, so that it is in contact with the wall of the rectum opposite the cervix, it registers a temperature of about 104° F.

Prostatitis and Vesiculitis.

In treating the prostate and vesicles an electrode consisting of a tube of metal, oval in section and ½ in. in its widest diameter, is used. Passing down this tube is another tube, fastened to its anterior surface. Down this tube a thermometer may be passed. The electrode is passed for 4 in. through the anus. The external end is then depressed on to the couch and kept in position by a piece of sheet lead. The anterior surface of the electrode is thus pressed against the anterior rectal wall.

The indifferent electrode is a lead belt similar to that already described in the treatment of the cervix.

The current is switched on and increased until the patient says that pain is beginning to be produced. It is then turned off slightly. The thermometer will now register about 114° F. or somewhat less. These treatments last for twenty minutes. Often before this time has elapsed, the patient will complain of an ache in the region of the electrode. When this occurs the current is reduced until he is again comfortable.

The temperature reached throughout the prostate during this treatment is not accurately known. If, in a woman, a rectal electrode be inserted, and the current applied as for prostatic treatment in a man, a thermometer in the cervix registers 108° F., while the rectal thermometer registers 114° F. For this comparison, the anatomical relationships of the cervix and prostate to a rectal electrode may be considered the same, and it may be inferred that the temperature of the prostate varies from 114° F. in that part nearest the electrode to 108° F. at its periphery.

We believe that this method of treatment is to be preferred to prostatic massage. The gonococcal infection is intra- and intercellular. The infection may be deep in the organ and in its lymphatics. It is not confined to the surface of the cells lining the ducts and glands. Massage can only remove the infected material collected in the ducts and glands. That massage is liable to cause metastasis is shown by the frequency with which it is followed by epididymitis. Diathermy heats the whole organ without the liability of any such accident occurring. It has been said that prostatic massage acts by causing an auto-vaccination. Our view is that diathermy acts in the same way and more effectually than massage, and without risk of metastasis.

Epididymitis.

In treating epididymitis both electrodes are active. One is the rectal electrode already described, and the other is applied to the scrotum on the affected side. Lint soaked in strong saline is first applied, and then a conveniently shaped lead electrode is placed over this. We have also treated epididymitis by applying the current to the prostate by means of the rectal electrode and belt round the waist. Recovery is as rapid under the use of this method as by treatment with an electrode applied to the scrotum, but the relief of pain is not so immediate. If there is pain and swelling in the inguinal canal a pad and lead plate should be placed over this situation, and treatment given in this way as well as the treatment described for the inflamed epididymis.

As just stated the epididymis may be effectually cured by treating the prostate. In the same way salpingitis is effectually relieved by treating the accompanying cervicitis. The diathermy of the cervix is in all cases an essential part of the treatment of a gonorrhœal salpingitis.

During diathermy of the cervix pain will sometimes occur in the lower abdomen. It may be severe and may last for a week. If pain comes on in this way cervical diathermy must be stopped and an application made with the vaginal electrode.

The vaginal electrode is a cylinder with a rounded end of such a diameter that it fits the vagina comfortably, and long enough to project beyond the vulvar orifice when fully inserted. The end in contact with the cervix is composed of metal for 1 in., the rest of the cylinder being constructed of vulcanite. The metal end is connected down the centre of the vulcanite by a copper rod with a screw terminal.

The belt-distributing electrode is placed round the waist. At the first sitting 1·5 amperes of current are passed for twenty minutes. Subsequently 2 amperes are used. After about four applications in a fortnight the cervical treatment may in most cases be conducted without producing pain. The increase of pain during cervical application seems to be dependent on the presence of pus or fluid pent up in the tube and not on the acuteness of the salpingitis.

We have treated children from 2½ years of age upwards suffering from gonorrhœal vaginitis and urethritis. A lead belt of suitable size is applied round the pelvis. Diathermy may be carried out by either a rectal or vaginal electrode. The latter is to be preferred, and with this an accompanying urethritis is also cured. Sometimes the vaginal orifice and vulva are so inflamed that the pain caused by the introduction of the electrode prevents its use. In this case the rectal electrode may be employed. Great patience is required. The electrodes are applied and the child

is then allowed to settle down; sometimes she goes to sleep. The current is applied gradually, the child being carefully watched. Any crying or evidence of pain is a signal for the reduction of the current. The treatment of these cases is very successful. Up to the present we have not had to give more than five effectual applications.

In the case of any new and successful treatment such as this there is a tendency for it to be taken up by persons not properly qualified to practise it. If this happens either the results will be unsatisfactory or disasters will occur.

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Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

Progress in Preventive Medicine:

PRESIDENT'S ADDRESS.

By JOHN C. MCVAIL, M.D., LL.D., F.R.F.P.S.

MY first duty is to thank the Section of Epidemiology of the Royal Society of Medicine for the high honour of the Presidentship. When that distinction was proposed to me by Dr. Reece, I accepted, relying on his presence and help at our meetings. All who knew him deplore his tragically sudden death in the vigour of manhood and in the midst of his usefulness. This Section and I personally will sorely miss his genial and stimulating membership.

In 1901, in a presidential address to the Incorporated Society of Medical Officers of Health, I took for my subject the Ethics of Preventive Medicine. Under a similar mental impulse my thoughts turn in the same direction to-day, and I propose to speak to you on Progress in Preventive Medicine. Notwithstanding the remarkable extension of average life in this country, there comes a time when a man tends to dwell more on the past in which he has had some small share, than on the limited period which lies before him. But the world continues though individuals depart, and it is well to look forward as well as backward.

The saving of human life since the modern sanitary era began, and the fact that life is more worth living, is a constant and justifiable theme of congratulation in the present day. As compared with the time when William Farr began his great work at the General Register Office, the mean duration of life has been extended by more than a decennium, with, it may be noted, the incidental practical result that insurance companies are moving towards the lowering of annuities or increasing of charges.

At the other extreme of life, infant mortality has wonderfully diminished, and at last in this country, every age period is getting some share in the benefit. Though increased length of days does not necessarily mean in every individual better personal health or greater physical fitness, yet, broadly speaking, it is safe to assume that the causes which delay the ageing of the bodily tissues, not merely defer death, but diminish suffering and raise the capacity for work, which done willingly, is not a curse but a blessing. There is indeed no delusion more deplorable and with a worse reaction on the individual than that happiness is to be found in sloth. At the same time that death is being deferred and working power increased, science, by control of diseases like malaria and yellow fever and hookworm, is extending the boundaries of the habitable world, and by devising new methods of food preservation and transport, by bringing fruit from California and meat from the Antipodes, is making provision for the larger population.

Science in Peace and War.

All this is claimed as due to the co-ordinated action of science and philanthropy Humanity rejoices in every fresh conquest of disease, the most recent notable example being diabetes, the control of which by insulin is due to the great work done by Dr. Banting and his fellow workers in the laboratories of the University of Toronto. But there is, alas, another side to the picture. Madly and ironically the same world which is striving so hopefully and strenuously to preserve life, is equally striving to discover means to destroy it. In the Great War, of which we still are and for a generation or more will be, suffering the after-effects, the number of lives lost by all the forces involved has been authoritatively estimated at between nine and ten millions. The share of the British Empire was nine-tenths of a million, and its wounded in addition were well over two millions. If that proportion be applied to the total of the forces the wounded would be more than twenty millions. A statistician will find interesting material in trying to calculate the balance of gain and loss as between the science and art of war in destroying life and the science and art of peace in preserving it. For example, in forty years the average annual death-rate in England and Wales has fallen by about a third—from 21 per 1,000 per annum in 1871-80 to 14 in 1911-20. The data for Scotland and Ireland are not so different as to require separate statement. If the rate of 1871-80 had still prevailed in 1911-20 (including civilian mortality only) then the annual deaths, instead of being under 520,000, would have been nearly 780,000. The saving thus was 260,000 lives a year in the United Kingdom. Its total war deaths having been well on to 750,000, life-saving science and philanthropy thus in three years considerably more than wiped out the loss of life sustained by this country in a war of four years—the greatest war in history. But this comparison is of the crudest. The lives lost were those of men in their prime, when their value is greatest, while the lives saved in the community were spread over all ages. As regards the wounded, who numbered (for the United Kingdom) nearly 1,700,000, no comparison, even so rough and uncorrected as that just attempted, can be usefully made. Yet the improvement in general health which is claimed as having accompanied the diminishing death-rate can, in some degree, be set against the damage to health and efficiency caused by the war.

So much for the past results of science as a life-saving force on the one hand and a life-destroying force on the other. What, however, of the future? It has taken forty years to make the progress claimed in this rough calculation. The powers of destruction have been advancing at a terribly more rapid rate—by means of long distance artillery, explosive ammunition, poison gases, burning liquids, and other such devilish devices applied not only on land and sea, but in the air and under the water. Indeed the modern pessimist asks whether, at no distant date, it may not be possible for whole armies or whole peoples to be exterminated by whatever nation chances first to discover the necessary means. Past civilizations have disappeared and their remains are discovered in the desert places of the world. The pessimist may here lead himself to think that a like fate awaits existing civilization. Well, without being able to submit any reasoned defence of the faith that is in me, I cannot subscribe to this doleful doctrine. The world will by and by recover from the calamity of the Great War, and peace and progress will reassert themselves. Life will attain higher levels, and advancing knowledge will include in its results the further prolongation of human existence. Urban conditions which have led to physical deterioration are being, and will be, ameliorated. The death-rate of cities is now lower than was that of Dr. Farr's "Healthy Districts" of a previous generation, and to the *rus in urbe* is being added the *urbs in rure*.

Holding these views, I proceed not merely to retrospect but to prospect.

Cleanliness.

It fell to my lot to take part in the establishment of a sanitary service in the country districts of Scotland under a Local Government Act passed in 1889. The medical officers then appointed had a fallow field in which to plough and sow and reap. Nothing had been done; everything was to do. The County Councils and District Committees did not know what their new duties were to be, and their medical officers had to instruct them. To begin with, we had to forge our own ploughshares and reaping-hooks. The powers under the then existing Health Acts were quite insufficient. New statutes needed to be devised, and Parliament had to be got to pass them. The very elements of a healthy life were to be provided for the population of Scotland living outside the burghs—a population amounting to about one-third of the whole. Schemes for water supply, drainage and scavenging, had to be established; the long struggle for improvement in the conditions of housing was entered on, and hospitals had to be erected throughout the country for isolation of infectious disease. The joy of life was felt to the full in the midst of such activities. There were no qualms or doubts as to the value of our labours, and every new success was the starting place for further effort.

At that time sanitation had very little help from the undeveloped science of bacteriology. That subject formed no part of the examination for the certificate in sanitary science of the University of Cambridge in 1885. But in all the new work the guiding principle was cleanliness. It was the principle which had inspired Chadwick and Simon in the previous generation—cleanliness of air and water and food—cleanliness personal, domestic, municipal, and national.

In the light of newer knowledge various errors are inevitably exposed. There were needless fears of sewer air and drain gases as means of conveyance of infectious disease, and efforts at disinfection were not always wisely directed. Also there were serious errors as to the value of isolation hospitals. The unquestioned assumption was that when a patient suffering from enteric fever or scarlet fever or diphtheria had entirely passed the acute stages of his malady, and disinfection had been duly attended to, he could be discharged without risk of harming others when he returned home; and if, as occasionally happened, scarlet fever was spread by such a convalescent, the occurrence was believed to be solely due to some mistake or carelessness in connexion with the dismissal of the patient. In short, the doctrine of chronic carriers of enteric fever or diphtheria had not been evolved. Yet, if the old practice of cleanliness be tested by the new knowledge it comes, on the whole, remarkably well out of the ordeal. In typhus fever, which already had largely disappeared when county sanitation began, reliance was placed on thorough cleansing, disinfection, and ventilation of the dwelling. When a typhus patient was removed to hospital, the opinion was that the carrying distance of the infection could not be more than ten feet, and that it could be cut off by the fresh air from an intervening open window. Now it is known that body lice are the vehicle for conveyance of the undiscovered organism assumed to be the *causa causans* of the disease. But if, experimentally, body lice are placed in a situation where on one side there is fresh air and light, and on the other side foul air and darkness and dirt, the lice straightway proceed towards the latter and away from the former. Similarly when plague invaded Glasgow, before the part played by rats and the *Bacillus pestis* was known, a great campaign of cleanliness was instituted in the whole district around the harbour. Constant removal of refuse and garbage, cleansing and lime-washing and disinfection were its features. But these proceedings were manifestly inimical to the rat population, and in fact whether exactly on that account or not, the disease speedily disappeared.

So also in the domain of surgery the great merit of Listerism was that it established a scientific doctrine of surgical cleanliness. As I have shown elsewhere,

an old-fashioned surgeon, practising cleanliness instinctively and empirically, could achieve a wonderful measure of success. But the principle which inspired Lister's work has permeated the whole practice of surgery, so that all can now achieve a success undreamt of before his time, and in regions of the body which no man had dared to penetrate.

While all this and more can be claimed for cleanliness it is possible to exaggerate its value. Environmental sanitation, by establishing in a community conditions favourable to health and life, strengthens resisting power to disease in general, including infectious disease. But it does not prevent epidemics, nor ward off liability to attack. Though some acute infections—scarlet fever and whooping cough—have become less prevalent and less virulent, influenza and pneumonia epidemics have increased exceedingly, and appear to defy sanitation. The ways of epidemics are peculiar, and one would need to live a thousand years to study them duly. It would indeed be a great gain if the conditions of life could be made so wholesome and the natural resistance of the human body be so strengthened as to prevent entirely the invasion of infectious disease. But there is no evidence in the history of the world that these conditions, either of environment or health, have ever existed, and notwithstanding all that has been accomplished there is no indication that they will be attained. And even if such an achievement were thought to be ultimately possible, it would be quite unjustifiable to refrain from using measures that are available now. For a generation the securing of a pure milk supply has been an ideal of the sanitarian. The problem has been attacked from all sides—the health and housing of the cattle, the extermination of tubercle, the cleanliness of milkers and milking, the safe-guarding of milk transport and storage. But the end aimed at has not been achieved and so recourse is had to pasteurization as a short cut to safety, while every effort is continued in other directions. Similarly, in addition to a good environment, individual immunization against infectious disease is practised to an increasing extent in the present day. Modern science has demonstrated its value against various dangers, and it would be immoral not to utilize immunization whenever it can save life or protect against illness.

Evolution of Health Policy.

The discovery that isolation hospitals were not so certain to prevent spread of infection as had been supposed did not cause their abandonment, nor even any lessening of resort to them. That fact introduces reference to a remarkable development in public policy. In the early days, isolation was practised not for the benefit of the isolated individual but to prevent spread of infection in the community. Incidentally, it was the duty of the public health authority to do its very utmost, in the way of treatment and nursing, for any child who had, in the public interest, been removed from the charge of the parents intended by nature to be its protectors, and taken to an institution where only hired services would be available. On that account the hospitals were well provided for and well staffed, but still the primary object was the public, not the patient's welfare. Contemporaneously, however, with the gradual realization of the limits of hospital isolation in protecting the public, there was developing a feeling of communal responsibility for the welfare of the individual. Training of nurses had made great advances; a modern hospital had far better equipment than even a good home, let alone a poor or slum home, for the treatment of disease, and so, on humanitarian grounds, the hospital is resorted to as freely as before, and accommodation, greater or less, is now being provided for a disease like measles which admittedly cannot be prevented from spread by removal to hospital.

Similar development of view has taken place throughout the whole sphere of social effort. At first the ideal was to give every human being a chance in life, and

then to leave him to work out his own career. The chance in life was to consist of elementary education, a healthy environment, and protection against epidemic disease. These conditions being established the individual and the family were to fight their own battle, each reaching such a level as his natural ability, perseverance, character and opportunities might enable him to achieve.

It was a fine ideal so far as it went, and it went very far, but it did not give due heed to one factor of essential importance. That factor was the education of the parent in the rearing of the child. With the best will in the world, in a healthy house in healthy surroundings, the average mother in the poorer classes, and even in the better-off classes, though she has her full share of natural affection, has not the intuitive knowledge as to how to feed, and guide, and train her offspring. Environment therefore is not enough, and public awakening to this consideration has been the basis of modern progress.

But it was not the statutory health authorities, at least outside the centres of population, who were the first to realize the implications of this fact. These were immersed in important work affecting the welfare of the general community committed to their charge. They were not dealing with persons but with conditions. The education authorities, however, were in a very different position and had a very different outlook. They were wholly occupied with individuals—namely, the school children. And when they found that some of these could not be properly educated owing to bad health or want of food and bodily comforts, they set about the provision of what was necessary. They indeed attended to the health of the children as an adjunct to their education. Logically, the health authorities should have been doing the health work, but not being in contact with the children and being otherwise fully occupied, they were not alive to it. Now, fortunately, in England the anomalous position has been rectified by the same local authorities being made responsible both for health and education. In Scotland, alas, the two bodies remain quite distinct, though through their officers a large measure of co-ordination is maintained.

Civilization looks back to-day in amazement and shame at the cruelties of the traffic in negro slaves, and at the methods and extent of child labour in the industrialism of a hundred years ago. Modern welfare activities, especially as to children, constitute a marvellous recoil from the abuses of the past. If ever it were to fall to my lot to need to preach a sermon, I think my text would be the saying of Christ, that it were better for a man to have a millstone hanged about his neck and to be cast into the sea, than that he should offend these little ones. The offence may be physical or mental or moral, and the offender may be parent or nurse or doctor or schoolmaster or public authority or politician. How easily the discourse could be developed, by reference to faults in cleanliness, feeding and clothing; to ophthalmia neonatorum, syphilis, rickets, tuberculosis; to school training, not only undertraining but overtraining, physical and mental, especially undue stimulation of the growing brain; to failure to educate in self-respect, thrift and foresight, so that the child should not, either by example or precept, be led to rely on charity or public assistance where personal exertions and prudence should suffice to meet personal needs. In all such directions it is possible to offend the little ones.

Credit for the advances that have been made in prevention of disease and prolongation of life cannot be claimed solely by any single agency or by any half-dozen agencies. The medical officer of health has played a large part in the work. He has often been not merely the instrument, but the initiator of progress, and as (commonly) a whole-time official he has been the administrator to carry out policy determined in large measure by his advice. But as an administrator he has not had, and could not have had, any large part in laboratory research, as to which his duty has been to appraise the discoveries of the scientist and to apply them with discretion. In addition to the research worker, who is often outside the profession of medicine, the

general medical practitioner is coming more and more into the ambit of hygiene, and the profession of nursing has its own departmental sphere. But, most important of all, the public conscience is now awake as it never has been before, to its duty towards the individual, and part of the awakening consists in realization of the value of modern discovery in its applications to bodily and mental health. Moreover, all that is being done is affected by the better general education of the community at large, by the higher standard of life in respect of food and cleanliness, soon, I trust, to be supplemented by better and healthier housing. The results achieved and to be achieved are therefore the outcome of a complex of forces, each bearing its own share in the general advance.

The Principles of Public Assistance.

Great progress in any sphere of activity is almost inevitably attended by some kind of drawback or disadvantage. I have heard a distinguished physician say that Laennec's introduction of the stethoscope, and its adoption throughout the practice of medicine, resulted in some loss of skill in observation of the general signs and symptoms of respiratory and cardiac diseases, and perhaps the same remark may apply even to the clinical thermometer. In modern schemes of public assistance, there is risk of diminution of the sense of personal and family responsibility and the fostering of a readiness to rely on imperial taxes or local rates for services which should be provided by individuals for themselves, and by parents for their children. Illustrations of this tendency came before me very early and forcibly in my official experience, but they need not be enlarged upon here, and the tendency is so obvious as to be open to every observer. The important question is in what direction prevention or remedy may be sought and found. It is not to be looked for in curtailing the work already being done towards raising the standard of life and health. On the contrary this work should be extended, but always on safe lines. Sound health and good character ought to be complementary to each other. Health should strengthen character and character should safeguard health. Success, it seems to me, should be sought by adherence to three principles, as follows:—

(1) Insurance in the provision of services which the individual cannot be safely left to secure for himself. The application of that principle is the most valuable feature of the Health Insurance system. Definite personal contributions to a fund created beforehand for meeting the cost of contingent outlay on behalf of individuals is incomparably better than resort to current rates and taxes.

(2) The firm establishment in practice of the principle of conditional relief as distinguished from unconditional; the refusal of relief where it is not needed, and exercise of due discipline over beneficiaries.

(3) Persistent and effective education in personal and family health, especially of parents in the rearing of their children; education in self-reliance and foresight and in the responsibilities of citizenship.

Periodic Health Examination.

Having stated these principles, I proceed to refer to two directions in which further progress may be made in the improvement of health and the prolongation of life.

The first is routine medical examination of individuals apart from illness. In 1907 at the request of the Royal Commission on the Poor Law I investigated Poor Law medical relief in England and Wales. In a memorandum appended to my Report I urged that such systematic medical inspection should be established in respect of all ages of life. The family doctor should, not too frequently but at reasonable intervals, offer to make examination of those for whose health generally

he is or ought to be responsible. His course of action might be somewhat as follows :—

(1) He would acquaint himself with the general family history. He would note tendencies requiring to be checked—tendencies to general disease like tubercle, gout, rheumatism, diabetes, obesity, cancer, insanity, alcoholism or the like.

(2) With regard to the children, he would note their physical condition as a whole, their state of development in relation to age ; indications of rickets, or tubercle or glandular disease, enlargement of tonsils, defects of hearing and of sight, as bearing both on health and education. He would give advice as to diet, bathing, fresh air, exercise and the like. By prevention of rickets in female children he might succeed in hindering pelvic deformity with its serious results belonging to the child-bearing period of life. In connexion with the occurrence of actual illness, he would be careful to note whether there was any threatening of dangerous sequelæ, such as endocarditis after the acute infectious diseases ; or ophthalmia or otorrhœa, which last, if neglected, is often followed, sometimes after an interval of many years, by fatal inflammation of the brain. He would note whether the child's nervous system was being properly developed on the one hand, and its functional activity sufficiently restrained on the other. As regards the school child, he would see whether school work was being overdone, whether there was headache, or insomnia, or night terrors ; whether the child was being taught to practise steady application in any specified task ; whether it was being permitted to read books of too exciting or too advanced a character, whether it was being sent sufficiently early to bed and made to rise early enough in the morning ; whether it was being punished unwisely for faults due to its physical and mental condition.

(3) As the child continued to grow into youth, the medical man would note whether athletic exercise was excessive, whether injury to the heart or other vital organs was being threatened ; whether cycling was being overdone ; whether, on the other hand, the youth was leading too sedentary a life and was unduly given to reading. When the time came he would advise as to the most suitable occupation, looking to the general physical and mental health and condition. Amongst girls, he would note tendencies to anæmia, gastric ulcer and the like, and again the question of insufficiency or excess of exercise would arise. Here also he would advise as to occupation with relation to length of hours and healthiness of employment, whether the work would be too sedentary or require too much standing ; whether, as in the case of a teacher, it might involve too severe mental strain or voice strain ; whether neuralgia and insomnia would result from the life proposed to be led.

(4) Amongst women he would see that due care was taken to prevent mischievous sequelæ of childbirth, such as uterine displacements or inflammations.

(5) Amongst men he would observe any tendency to trade disease ; whether their employment was such as made special calls on any of the vital organs, and, if so, whether these organs were reacting properly to the stimulus or were threatening to break down under it. In most cases the important organs would be the heart and the lungs. Heart strain would be looked for, and bronchitis and emphysema and the like.

(6) Independently of age and sex and occupation, the inmates of a household would be offered a general examination of their vital organs. In presence of heart lesions advice would be given in order to prolong the duration of compensation and to limit the calls made on the heart, so as to give it only so much work as it could properly perform.

(7) In old persons attention would be paid to the condition of the arteries in relation to apoplexy and to parts likely to be attacked by cancerous disease.

Though periodical medical examination did not commend itself to the Royal Commission, there has been since that time remarkable development on the lines

indicated. Infant welfare, child welfare, school medical inspection and treatment carry out the principle as regards the young, though the work is not done by the family medical attendant, but by school doctors and nurses. Even as to adult life much progress is now being made. In America[1] several of the large life insurance companies provide and pay for periodical medical inspection of the insured. They do so, not on grounds of benevolence, but of economy and finance. It profits them to defer death so that the premiums may continue and payment of the life policy may be postponed. Large claims are made of savings in life and capacity for work under this scheme, and data are given in support of the claims. The work is done by medical men employed not directly by the Insurance Companies but by a Life Extension Institute. It is asserted that "Evidence is accumulating rapidly to show that this common-sense method, generally applied and followed by necessary remedial treatment or regulation of personal hygiene, will do more to prolong health and vigour than any other measure." During the administration of President Roosevelt the system was introduced into the army, in respect of commissioned officers, and it is reported that "this policy is of great value in conserving health." A circular issued by the War Department in 1923 urged the necessity of the examinations being carried out with thoroughness and attention to detail, so as to enable the department "to apply practically, on a large scale, the principles of disease prevention and health preservation." The Metropolitan Life Insurance Company, between 1914 and 1921, authorized nearly 95,000 physical examinations and reports. "It has been established to the satisfaction of this Company that periodic medical examination is justified by the benefit in added life expectancy to the policy-holders examined."

In this connexion a most important question is the qualification of the average medical attendant to make skilled examination for the very beginnings of disease. No doubt different skill would be possessed by different men, but if such a system were set on foot there would be steady improvement in fitness for the work. And now the whole medical curriculum has been revised with the definite object of permeating the entire period of study by the principle of prevention; every subject, from applied physics, chemistry, biology and anatomy, up to the clinical courses is to be taught with a view to the maintenance of health as well as the cure of disease. The teachers themselves will perhaps have to develop a new orientation and the outdoor department or dispensary will often be a better field for instruction than the hospital ward.

In my own opinion no sphere of medical work is more certain to be helpful than this of periodic examination of individuals in the preservation of health and the prolongation of life—always however, let me repeat, with the consent of the individual—the adult for himself, the parent for the children.

Health Authorities and the Poor Law.

Next, in its bearing on the health, bodily and mental, of the general community, I wish to call your attention to an important Poor Law question. In reporting to the Royal Commission I supported the view that there ought not to be two separate sets of local authorities dealing with health, one with the health of the community at large, the other with that of the pauper class. The health functions of the Board of Guardians should be transferred to the local authorities under the Public Health Acts. The question whether, if that transfer took place, what remained of the Poor Law would require the continuance of the Guardians, or whether these could not be entirely dispensed with, is outside the scope of this address. The majority report, however, was against the policy of "break-up" of the Poor Law including the

[1] "Health Building and Life Extension," by E. C. Fisk, M.D. The Macmillan Company, 1923.

transference of health functions. Recent reconsideration by a specially appointed body has resulted in a reversal of the majority conclusion on the subject, and leading members of the old majority have concurred in that reversal. My present object is to urge the importance of the health authority taking charge of the health of the pauper class, not only in their own interest, but in that of the population at large. Excluding lunatics and vagrants, paupers number, perhaps, only about 25 per 1,000 of the population, varying regularly according to season and irregularly according to the state of trade and to other influences. Pauperism, though destitution is its legal criterion, connotes far more than mere poverty. Poverty may be due to sheer ill-luck or ill-health, or mental or moral deficiency. But whether it results from tuberculosis or alcoholism or instability of character or other cause, the public interest requires that paupers be under most skilled and broad-minded control in respect of health and character, and training in well-being and well-doing. Where a widow and her family have become destitute owing to the death of the husband and father from tuberculosis, or where children have been deserted by a dissolute or drunken parent, or where there is a history of insanity or criminality—in all these and similar conditions the offspring should be reared under the control of an authority which neither in law nor in fact is limited by any principle of mere relief of destitution. Reared wrongly such a child may develop wrongly in health and character and be a curse to itself and the community; reared rightly it may lead a normal healthy and happy life.

I quite recognize that many Boards of Guardians go beyond the limits prescribed to them, and on the other hand that the area and population under many a public health authority is far too small for efficient administration. But the situation should be frankly faced, and a great reform in the whole scheme of local government be undertaken. No doubt it would be costly, and we live in times of the most appalling need for public and private economy, but "there is that scattereth and yet increaseth." Once more on this point, let me make clear my conviction that public assistance should constantly be safeguarded by the three principles already premised as its basis—insurance, conditional instead of unconditional relief, and education in self reliance and character.

Conclusion.

Concerning modern activities in maternity and prematernity, and infant and child welfare, I have nothing to suggest beyond perseverance on the lines already followed. Perhaps I might refer, however, to the fears sometimes expressed, that all the health work, environmental and individual, of recent times will lead in the end to the propagation and rearing of a weakly race which would be better suppressed by the stern measures of unassisted nature in separating the fit from the unfit. Let me conclude this address by repeating an illustration which I used on a similar occasion many years ago. Taking a million births in Great Britain let it be assumed that in inferior health conditions one-tenth of these will during the first ten years of life fall below the line of viability—will cease to live. That, it may be urged, is a good thing, as they are better dead, being unfit to remain. Let us suppose that the environment is improved, so that this tenth remains above the line of viability, survives into manhood and womanhood. That, it may be said, is bad, as it tends to propagate an unhealthy race. But, let it be observed that when this tenth is raised from non-viability into viability, all the superimposed nine-tenths of the million are raised through a similar space in our imaginary scale. The lowest tenth, which formerly rested on the level of viability, is now one place higher, and so throughout the column. The gain is nine times the loss. And if, in obstinate determination to get rid of that unhealthy tenth, we return to the old and inferior conditions of life, it is true that the lowest tenth goes below the line, but the second

lowest takes its place. If, pursuing this course, we determine to get rid of the second tenth by making still harder its struggle for life, no doubt we succeed, but then the third tenth takes the place of the second, and so on, until, endeavouring to improve the species, we exterminate the race. Fortunately, in this matter science and natural affection, social economy and philanthropy, go hand-in-hand. The quality of mercy is not strained, and the same conditions which strengthen the weakest make also for the survival of the fittest.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

A Brief Survey of Public Health Organization in the United States of America.

By T. CARNWATH, D.S.O., M.B.

YOUR secretaries have thought that some account of a recent visit to study public health organization and achievement in the United States would be appropriate to a Section whose object is to occupy itself with questions of state medicine as well as epidemiology.

The journey, of which this paper is the outcome, was undertaken in the latter part of 1923, when it was my good fortune to be a member of one of the health interchange parties organized under the auspices of the Health Section of the League of Nations and financed by the International Health Board of the Rockefeller Foundation.

The tour was planned by the United States Public Health Service, and some twenty-five health officers from fifteen different countries participated in the visit.

We assembled at Washington on September 10, and were received by the Surgeon-General of the United States Public Health Service, General Cumming, who welcomed us on behalf of the Secretary to the Treasury. We spent a fortnight in Washington, learning something of the Federal health activities and of the relations of the Public Health Service to the State health departments. On leaving Washington, we separated into three groups. One group visited Virginia and Massachusetts, the second North Carolina and New York, and the third group, to which I was attached, visited Alabama and Pennsylvania. There were seven members in my group—a Frenchman, a Belgian, a Russian, a Brazilian, a San Salvadorian, a Greek, and myself. After leaving Washington, we proceeded to Montgomery, Alabama, where we had a most cordial reception from the Governor and from the Chief of the State Health Department. After spending a week in Montgomery, listening to lectures from various members of the health staff and visiting places of interest in the immediate neighbourhood, we continued our journey to Troy, a small town which lies about forty miles to the south of Montgomery. There we were shown a typical county organization. From Troy, we journeyed southward towards the malaria and hookworm belt, and finally ended up at Mobile, in the extreme south-west corner of Alabama. When there I took an opportunity of visiting New Orleans. After spending a few days in Mobile, we travelled north again to Birmingham, one of the largest industrial centres in the Southern States. This city has a population of about 200,000, and is engaged mainly in the steel and coal industry. It is the headquarters of the Tennessee Coal and Iron Company, an organization employing some 26,000 men, and one of the most important constituent members of the United States Steel Corporation. After five days in Birmingham, we travelled eastward to Talladega, where we were shown another county health organization, and finally north to Huntsville, where we bade good-bye with reluctance to Alabama.

On our way north to Pennsylvania, the journey was broken at Baltimore to give us an opportunity of seeing the Johns Hopkins School of Hygiene. From Baltimore we proceeded to Harrisburg, where we again received a very cordial reception from the local dignitaries, including the Commissioner of Health, Dr. Miner, and his colleagues. Later, we spent a week in Allentown and ten days in Philadelphia, studying health work in a large and a small city. The members of the three parties finally met in New York, where we were shown the quarantine arrangements and the methods of examination of aliens.

Briefly, this was the extent of our tour. A glance at the map, however, will show what a small area we covered compared with the total area of the country, but we were given to understand that these two States represented a fair average of American achievement in the sphere of public health. Alabama would in this respect be included among the more backward States. Pennsylvania, if not one of the most forward, has accomplished in certain directions a great deal during the last twenty years. Both States are about the same size as England. Alabama is somewhat larger, and Pennsylvania slightly smaller. The population of the former is 2,200,000, of whom 901,000 are negroes. The population of Pennsylvania is 8½ millions, but with a large foreign-born element (15 per cent). There are 350,000 negroes in Pennsylvania.

The area of the United States is approximately 3,000,000 square miles, and the population is estimated at about 110,000,000. Like "all Gaul," the country is divided into three main parts, each with a more or less distinctive political character. In the east, the watershed of the Appalachian Range, extending from New York down to Alabama, separates the eastern seaboard from the great middle-west. On the west the range of the Rockies separates the Mississippi basin from California and the other States on the western seaboard. Our experience is limited to the eastern and southern States, but there is little doubt that it is the great middle-west which will determine the future of the United States. One has only to consider the immense valley of the Mississippi, 4,000 miles long, and observe the numerous centres of industrial activity that are to be found within its confines, to realize what an important influence this great community will ultimately exercise on world affairs. It links the staid, cautious, in some respects old-fashioned, New England States with the more adventurous peoples of the west.

The Union consists of forty-eight independent States, each with its own legislature and executive dealing with all matters of purely State concern. The Federal Government can only intervene where the interests of more than one State are affected. Each state is divided into counties. Thus, there are sixty-seven counties in Alabama, and a similar number in Pennsylvania. The counties are further sub-divided into cities, boroughs, and townships. Thus there are thirty-nine cities, 933 boroughs, and 1,400 townships of the first and second class in Pennsylvania. Cities and boroughs enjoy a very large measure of autonomy. They exercise not only administrative but also legislative functions. They can make ordinances relating to community affairs, and these ordinances do not require the confirmation of any superior authority. For example, suppose a borough or city wants to adopt milk regulations, the first step is for the city commissioners, acting in their legislative capacity, to pass an ordinance empowering themselves to make regulations. These regulations come into operation when the required formalities have been observed. Again, suppose a city wants to borrow money for some purpose, say the construction of an abattoir, the commissioners pass an ordinance authorizing the matter to be submitted to the electors. If the vote of the electors is favourable, bonds are issued and provision is made for the payment of interest and redemption, in accordance with the requirement of the State constitution. No city can borrow more than 7 per cent. of its assessable value without an appeal to the electors. Exceptions are made of the larger cities. For example, Philadelphia has borrowing powers up to 10 per cent. of its assessable value. This limitation does not apply, however, to productive activities or to works of public utility, for example, water-supply, tramways, and matters of that kind, the return from which is expected to provide for the payment of interest and sinking fund. In this connexion reference should be made to the system of rating which obtains in the United States. Rates are not based on the annual income from property, as they are here, but on capital value. This is assessed at intervals of from three to five years by committees specially appointed for the purpose, and rates are levied, as a rule, on 60 per cent. of the gross value.

The form of government varies from city to city and borough to borough. In Philadelphia the governing body is the mayor and town council. In Allentown, Harrisburg, and most of the other cities which were visited, the governing body is a board of five Commissioners, one of whom is entrusted with the charge of health matters. These Commissioners are usually professional politicians and receive a salary for their services. Townships of the first class are managed by three Commissioners, one of whom is chairman. The second-class townships, i.e., areas with a population of less than 300 to the square mile, are managed direct by the county supervisors. The mayor or chairman of the Board of Commissioners appoints the Board of Health, a body consisting of five members of whom one must be a physician of two years' standing, and the Board of Health appoints the Medical Officer, generally from amongst the local practitioners. In addition to these local Medical Officers there it also in each county a County Medical Director, who is appointed by and is directly responsible to the Commissioner of Health for the State. With one exception all these officers are part-time. The head of the State Health Department is the Commissioner of Health, who is a member of the Governor's cabinet. This officer is liable to be changed with a change of Governor.

A somewhat different arrangement obtains in Alabama. There the State Medical Association has been entrusted with the administration of the Health Acts. It is as if the British Medical Association in this country were made the Board of Health. The State Medical Association functions through an executive Committee of ten members. This Committee acts (1) as a Public Health Committee; (2) as a State Board of Examiners; and (3) as a Board of Censors. In its capacity of Public Health Committee the Governor of the State is chairman, and its decisions are not subject to review by the Association as a whole. A similar arrangement obtains in the counties, where the County Medical Association appoints five of its members to act as a Board of Health. The State Board of Health appoints the State Health Officer and the County Board of Health appoints the County Health Officer. By the Alabama Public Health Act all part-time health officers were abolished. Only those county boards are recognized for purposes of grant which have established a minimum whole-time Health Unit, i.e., a health officer, a sanitary inspector, a nurse and a clerk. Twenty-two of the sixty-seven counties in Alabama are thus organized. Those counties in which a Health Organization has not yet been established are administered directly from the State Health Department.

In attempting a survey of American public health organization and effort, in the compass of a single paper necessarily somewhat random in its observations, two courses are open. One might begin from the periphery by attempting to describe the work of the local authorities, and show how this is co-ordinated with the activities of the State and Federal Health Departments, but this would be unprofitable on account of the difficulty of the primary task. It might, indeed, be said that outside the larger cities few of the local health authorities accomplish anything. In the boroughs—and it will be remembered that there are 933 boroughs in Pennsylvania—practically all that is attempted is a desultory quarantine. Few of the Boards of Health have either the wit or the money to do useful work. Their advisers are for the most part untrained and inexperienced. The sanitary inspector is usually a gentleman whose chief qualification is that he deserves well of his political party. Under such conditions progress is inevitably slow. In the rural areas sanitary organization hardly exists. Of 2,085 counties in the United States only 231 have a full-time health officer.

A more profitable method of approach is from the Federal side, and this I propose to adopt.

Environmental Conditions.

But first a word or two on certain aspects of environmental hygiene. Speaking generally there is a spaciousness in the planning and arrangement of the houses

and a standard of domestic accommodation which must react favourably on the health and the character of the inhabitants. In the poorer parts of the large cities, however, where the negroes and low-class alien elements congregate, one observes conditions which are far from satisfactory. There is much overcrowding, and in Philadelphia alone it is estimated that there is a shortage of 20,000 houses. In Alabama many of the dwellings of the negroes are of the most rudimentary description—dirty, ramshackle, wooden huts without any proper sanitary accommodation or water supply. The material used in the building of the houses varies according to the locality. In most of the large cities the use of non-combustible materials is compulsory, but in the smaller towns and in the country villages one finds a great many half- or whole-timbered dwellings.

Scavenging, except in the larger cities, is usually a matter of private arrangement. In Philadelphia collection of refuse is undertaken by the city authorities. Householders are required to classify their refuse into putrescible and non-putrescible material. The putrescible garbage is collected three times a week and taken to a large installation near the mouth of the Schuylkil River, where the fat is extracted. and the residue is sold as manure. The ashes are collected weekly and used for filling up low-lying sites on the outskirts of the city. In Harrisburg an incinerator has lately been erected and was just completed when we were there. There is a very active cleansing department in Allentown, which prides itself on maintaining the cleanest city in Pennsylvania. Elsewhere the disposal of refuse is generally left to the householder, who makes what arrangements he can. The State Health Department has issued some by-laws on the subject and a fairly successful attempt is made to prevent gross nuisance.

Much excellent work has been done in improving the condition of the *water-supply*. There are 650 public water-supplies in Pennsylvania, supplying about 6,000,000 people, out of a total of 8½ million. Seventy per cent. of these 6,000,000 are supplied with filtered water, 22 per cent. with chlorinated and 7 per cent. with untreated water derived for the most part from upland streams. The filters are of various kinds—pressure filters, slow sand filters and rapid gravity sand filters. Philadelphia has a slow sand-filtration plant, but most of the other cities combine filtration with chlorination. Ths administration of the law in regard to water-supplies is entrusted to the State Health Department, and in 1923 a forward step was made by the formation of a State Water Board consisting of heads of departments interested. The first work of this board has been the survey and classification of all waters in the state, and in future no waterworks can be installed or extended without the approval of the Commissioner of Health. In the rural districts the population depends for the most part on wells.

Similarly, in Alabama the State Health Department has complete control of all water-supplies in the State. All plans for new waterworks and for extension of old works must be submitted to and approved by the State Health Department. Samples are sent to the department for examination every three months, a card index is kept of the results, and inquiries are made when examination shows that the sample is unsatisfactory. Any plant supplying over fifty persons is a public water-supply within the meaning of the law.

In the Mississippi Valley most of the towns derive their supplies from open rivers and lakes. That there has been a very considerable improvement in the water-supplies of the Ohio region is shown especially by the decrease in typhoid. Of thirty-seven large cities none had a mortality less than 10 per 100,000 in 1900. Twenty-five had a death-rate of 25 per 100,000 and seven were over 50 per 100,000. In Pittsburg, the rate was 144 per 100,000. If we compare this with 1920, we find that of sixty-six cities with a population of over 100,000 none had a typhoid rate over 16 per 100,000; fifty-eight had a rate of under 10 and thirty-two were under 5. This change, of course, is not entirely due to improvement in the water-supply, but

it is generally admitted that this is the most important factor. Thus, in eleven cities in the Ohio Valley using an unpurified water-supply between 1906 and 1915, the death-rate from typhoid in 1906 was 76·8 per 100,000 : in 1915 it was still 74·5. On the other hand, in sixteen other cities where the rate was 90·5 in 1906, the mortality was reduced to 15 per 100,000 in 1915, mainly by the introduction of purification. The source of supply remained the same, the only difference being that it was purified.

As regards *sewerage*, in a survey of eighty-three cities made by the American Public Health Association in 1920, 85 per cent. were found to be provided with some sort of system. The remainder had only cesspools or septic tanks. In the smaller cities, such sewers as exist are for the most part unsatisfactory. In Pennsylvania there are 192 sewage disposal plants, some good, some bad, the majority indifferent. Philadelphia discharges its sewage into the Delaware and the Schuylkil Rivers. Allentown has an experimental plant dealing with only a small percentage of the sewage. In the Ohio Valley 72 per cent. of the urban population is served by a sewerage system of sorts. The predominant practice is to discharge the sewage without treatment into the rivers. Sixty-one cities discharge untreated sewage. In twenty-two some form of treatment is adopted, but in five or six of these the only treatment is screening. In short, only 20 per cent. of the population are living in cities where treatment is carried out. In the smaller towns the usual means of excrement disposal are the septic tank, the vault, and the pail privy. A vigorous campaign is now being carried out in Alabama to secure the provision of better closet accommodation in the villages and country districts, with the hope of controlling hookworm.

With this brief reference to the position of what we regard as the principal environmental services, I return to Washington. The chief Federal authorities exercising health functions are the United States Public Health Service (a Bureau of the Treasury Department), the Bureau of Animal Industry and the Bureau of Chemistry (of the Department of Agriculture), the Division of Vital Statistics in the Bureau of the Census, and the Children's Bureau of the Department of Labour.

United States Public Health Service.

In considering the health organization of the United States, one is impressed with the dominant position occupied by the United States Public Health Service. This unique organization had its origin in the Marine Hospital Service, which was established by Act of Congress in 1798 to provide temporary relief and maintenance for sick and disabled American seamen. With the inclusion of other beneficiaries and the gradual expansion of the Service in other directions, arose the need for co-ordination and supervision, and in 1871 the first supervising Surgeon-General was appointed, Dr. Woodworth, who re-organized the Service on a national and what is still practically a military basis, with a view to eliminating local political influence and introducing a regular system of promotion. Following the epidemic of cholera in 1873, the National Quarantine Law of 1878 was passed and later amended by the law of 1893 which still remains the substantive Act and gives authority for all existing domestic and maritime quarantine regulations. The administration of this law was entrusted to the medical officers of the Marine Hospital Service. About the same time the publication of a weekly health bulletin summarizing the reports of United States Consuls abroad was made obligatory, and soon after we note the establishment of a bacteriological laboratory in New York. This laboratory was later removed to Washington, where it ultimately formed the nucleus of the Hygienic Laboratory.

This expansion of activities led in 1902 to a change of the name to the " Public Health and Marine Hospital Service " and to the grant of more specific statutory powers. The Hygienic Laboratory was re-organized and statutory provision was made for an annual conference of State health officers. The primary purpose of this

conference was to obviate friction in the application of inter-state epidemic regulations, but it was an easy step to the discussion of other health matters, so that co-operation in the obvious soon led to co-operation in the less obvious health functions of the State departments.

In 1912 the name was finally changed to the United States Public Health Service, and more general authority was given to the Service to study and investigate the diseases of man and the conditions influencing their origin and spread.

It will be evident from this brief historical review that the functions of the United States Public Health Service are threefold: (1) To provide medical relief for certain designated persons; (2) to prevent the introduction of disease from abroad and its spread from State to State; and (3) to investigate human diseases and to act as an intelligence bureau on health matters. The staff numbers 9,357 (only 200 of whom, however, are fully commissioned medical officers) and the annual expenditure amounts to approximately ten million dollars.

Marine Hospitals.—In order to give some idea of the magnitude of the work under this heading, it may be stated that the Service operates 25 hospitals and 120 relief stations. During 1922, 310,000 out-patient visits were made to the hospitals and there were 1,190,000 relief days. The cost of treatment works out at 4·08 dollars a day inclusive, which compares with 2 dollars before the war. The increase is due partly to increased cost of materials and service, but mainly to increased demand on the part of the public and of the seamen. The total expenditure on this branch of the Service amounts to 5,000,000 dollars a year. Special hospitals are provided for tuberculosis, leprosy, trachoma and pellagra. It is estimated that there are 1,000 lepers in the United States, a considerable proportion of whom are said to have contracted the disease indigenously. They occur in groups, mainly in California, in the Gulf States of Louisiana and Florida, and in Minnesota. Formerly each State dealt with its own cases, but recently a Federal Home was purchased at Carville, in Louisiana, with accommodation for 174 patients, which it is proposed to increase to 500. It is worth recording that treatment with chaulmoogra oil and its derivatives has been somewhat disappointing. At first most optimistic accounts were received from Honolulu concerning the effect of this remedy, but subsequent investigation has led to a more moderate view being adopted. At present the supervision of this work is in the hands of experts from whom considered judgment will be obtained in due course.

Quarantine and Alien Immigration.—An important part of the work of the Service is in connexion with quarantine and the medical inspection of immigrants for the Department of Labour. Originally, quarantine law was administered by the individual States, but gradually the work was taken over by the Federal Government. The last State to give up its quarantine functions was New York in 1921. At the present time there are sixty-four stations in the United States, twenty-eight in Europe, and ten to fifteen in the insular possessions. Quarantine work begins at the port of departure. No vessel from a foreign port is allowed to enter the U.S.A. unless it is furnished with a bill of health from the consul or from the medical officer detailed to that port. Most of the precautions required by the quarantine regulations are now carried out at the port of departure—bathing, disinfection, vaccination, certification, &c. The company forfeits automatically 5,000 dollars if a ship sails without a bill of health. On arrival at the Quarantine Station at New York the ship is boarded by one to three medical officers. The bill of health, the ship's manifest, and the crew list are examined; one officer takes the crew, one the steerage, and one the cabin passengers. The steerage passengers are passed in line before the inspecting officers and one hundred of each sex selected for more intensive examination as to disease, presence of lice, &c. If more than 2 per cent. are found to be lousy, all the steerage passengers are taken to Hoffman's Island for disinfestation; if less than 2 per cent. are found to be so the passengers affected are

alone removed. The usual methods are adopted in cases of infectious disease. During 1922, of 3,125,000 persons examined at all the United States ports only nineteen were found to be suffering from quarantinable disease. If all is found correct, the ship is given pratique, straight or provisional. In the former case there is no detention. Provisional pratique implies that fumigation has not been carried out within the six months' period. Dr. Stock[1] has recently given you an account of the fumigation methods employed, so I will not detain you with any description of these. In the southern ports of entry, measures against yellow fever are based on stegomyia control. In most of the southern ports of the U.S.A. the stegomyia index is still above the danger line, i.e., 5 per cent. of houses infested, and owing to the close proximity of Mexico, where yellow fever is endemic, it is not yet considered safe to remove restrictions on the movements of passengers between the two countries.

The alien immigration work is carried out by the Department of Labour, assisted by medical officers of the Public Health Service seconded for this particular purpose. In practice, the medical officers engaged in the quarantine work also help in the immigration work, particularly as regards the first- and second-class passengers. All third-class alien immigrants are taken to Ellis Island for more detailed inspection. Ellis Island is situated in New York Harbour, and is shaped somewhat like the letter E, the three limbs being numbered 1, 2, and 3. No. 1 is devoted entirely to the medical examination and the inspection of papers. No. 2 is a general hospital, and No. 3 an isolation hospital. Aliens, whose admission is of course now regulated on a percentage basis (2 per cent. of the number of each nationality resident in the United States at the time of the 1890 census are admitted annually), may also be rejected on their arrival for medical reasons as well as on grounds of public policy. Idiots, imbeciles, insane persons, and persons suffering from dangerous and loathsome contagious diseases, such as venereal diseases, trachoma, and favus, are excluded by mandate. These are grouped together as class A. Class B includes diseases which affect ability to earn a living: for example, valvular disease of the heart, crippling, deafness, Bright's disease, &c. This class is referred to a Board of Special Inquiry, consisting of three non-medical Immigration Officers, who inquire into the social conditions of the immigrants and the nature of their occupation. For example, take a case of valvular disease of the heart; if the man is a book-keeper and has friends and some money, and is not likely to undertake hard work, he would probably be admitted, whereas a miner or person following an arduous occupation would probably be excluded. Class C includes those suffering from minor ailments, such as a limp, loss of little finger, &c. These defects are simply recorded. These medical certificates then go to the Immigration Officers who consider them, together with the other information relating to the immigrant, and decide on the evidence whether he shall be admitted or not.

Infectious Disease.—The measures taken to prevent the introduction of disease from abroad thus lie wholly with the Central Government. The control of infectious disease within the Union falls to the local authorities, except in so far as inter-state traffic is concerned or when an epidemic in one State threatens to become a menace to another. By a law passed in 1890 persons suffering from plague, cholera, and yellow fever were prohibited from passing from one State to another. In 1893 this prohibition was extended to a large number of other infectious conditions, and obligation was placed on the United States Public Health Service to secure co-operation of the authorities concerned. Where the local regulations and administration are inadequate, the federal authority is empowered actively to intervene but this it does only in the last resort. There is a special division for this work with a staff of ten medical officers and twelve sanitary engineers. The annual appropriation for the purpose amounts to 500,000 dollars. This staff is engaged mainly in developing bureaux for infectious disease and sanitary engineering in State and local health

[1] *Proceedings*, 1924, xvii (Sect. Epid.), pp. 9-31.

departments, and in promoting the establishment of whole-time county health units in rural areas. The Federal authority is responsible for securing a proper water supply on trains and boats, a function which gives the Public Health Service indirectly considerable control over State supplies.

As regards local action in the control of infectious disease the practice differs somewhat from that in this country. Practically all known infectious diseases are notifiable but only the more important of these are quarantinable. No fee is paid for notifications, and perhaps for this reason the reporting of cases is not as complete as could be desired. In the larger cities there is not much to complain of in this respect, but in the rural districts and smaller communities, I understand the requirement is more honoured in the breach than the observance. On receipt of notification the procedure differs in different localities. Thus, in Philadelphia, and I understand the same description applies to New York, there are forty-five part-time District Medical Inspectors, who are responsible for confirmation of diagnosis, placarding, isolation, or removal to hospital, and exclusion of contacts from school. Every house in which a quarantinable disease occurs is placarded, and susceptible children in the family are supposed to remain on the premises until the danger is over. No restriction is placed on the movements of adults and immune persons. On the occurrence of small-pox very drastic measures are adopted in Philadelphia. A cordon of police is drawn round the block in which the case occurs, and everyone who cannot show satisfactory evidence of protection has to submit to vaccination or detention. In other parts of Pennsylvania small-pox is not regarded so seriously. There are about 200 cases per annum, mostly mild. Every case is subjected to quarantine and there is the usual search for and vaccination of contacts. Few of the States have compulsory vaccination law, but, on the other hand, no child is admitted to school unless it is vaccinated, and school attendance is compulsory. Further, an adult contact, who refuses vaccination, may be apprehended as a potential danger to the community, and put in prison. Vaccination of all prisoners is compulsory.

Outside the more important cities, the hospital isolation of infectious diseases is practically unknown. There is an excellent isolation hospital in Philadelphia with nominal accommodation for 1,200 cases. It is run on lines similar to those of corresponding British institutions. An interesting feature is the Medical Social Service Department, with a staff of five social workers. This Department interviews the relatives. They preach the value of toxin-antitoxin treatment and explain to parents the after-care of the children and the need for isolation to prevent secondary cases, and generally give advice in regard to the maintenance of health. Terminal disinfection appears to be rarely practised except after small-pox.

Of the infectious diseases *tuberculosis* requires special consideration. The moving force behind the anti-tuberculosis movement is the National Tuberculosis Association. This Association was formed in 1904, but little was done until after the conference in Washington in 1908, when Sir Arthur Newsholme was largely responsible for crystallizing ideas on the subject of tuberculosis prevention. Following that conference the National Association bent its energies towards the formation of State and County Societies and conceived the idea of collecting money by the issue of Christmas seals. These seals are issued by the National Association to the State organizations who distribute them to the County Societies. The income from this source amounts to about 4,000,000 dollars per annum. After deduction of the expenses, which vary from about 8 to 12 per cent., the balance is distributed as to 5 per cent. to the National organization, 20 per cent. to the State Societies and 75 per cent. to the counties, but this allocation between State and county is a matter of local arrangement and varies somewhat from State to State. The main purpose of the Association is to induce State and local authorities to adopt proper schemes of prevention and treatment. The Society works in close co-operation with the Federal and State authorities.

In Alabama very little has been done so far. The sale of the seals realizes only 32,000 dollars per annum, but local authorities are supplementing this fund with increasing contribu-

tions. At present there are four small unsatisfactory sanatoria in the state, meagrely staffed and equipped. Pennsylvania, on the other hand, has been engaged in anti-tuberculosis work since 1907. At that time local health organization was practically non-existent and therefore the policy of State rather than county control was adopted. The original plan involved the establishment of a State-wide system of clinics, with a sufficient number of sanatoria to meet the demand for treatment of early cases. Since 1907 the State has maintained at least one clinic in each county. Three sanatoria have been built with accommodation for 2,000 patients. Efforts are being made to stimulate local authorities to build local hospitals for the care of advanced cases and of patients who have attained their maximum improvement from sanatorium treatment. The clinics, of which there are now ninety-five, are conducted wherever possible in close association with the local hospital, where X-ray and other up-to-date forms of diagnosis are available. The clinics act as admitting stations for the State sanatoria. For reasons partly of economy, partly of ultimate efficiency, the State is endeavouring to induce the local authorities to take over these State clinics. Philadelphia and Pittsburg have now done so and are running them at the expense of city funds.

With the purpose of proving the efficacy of modern preventive methods, a number of interesting experiments have recently been initiated. With that at Framingham you are probably all familiar, but three others have recently been started in New York State on a more ambitious scale. Three areas have been selected: (1) A "rural" area—Cattaraugus County, which has a population of 80,000 and includes within its boundaries one city of 20,000 population and another of 12,000; (2) a medium-sized city—Syracuse, with a population of 170,000; (3) a metropolitan area—the Bellevue-Yorkville district of New York, with a population of 250,000. In each area an attempt will be made to show what can be done by adequate application of existing knowledge to the problem of preventing disease and promoting health. Local health machinery will be utilized and the experiments will be continued for five years. The cost of the demonstrations will be borne by the Millbank Memorial Fund.

Another interesting experiment is the "Home Hospital," established in 1912, by the New York Association for the Improvement of the Conditions of the Poor, for the purpose of demonstrating the results which can be secured by housing the tuberculous patient and his family in a wholesome environment, by supplying the family with sufficient relief to provide for an adequate standard of living, and by providing the necessary medical and nursing care, not only to ensure the recovery of the patient, but to prevent the spread of the disease to other members of the family. In an analysis which has recently been made of the results obtained by this method it appears that the prospects of recovery of the patient are quite as good as those in the ordinary sanatorium, and that under proper management there is no risk of spread of the disease. It is true that the expense is, or appears to be, somewhat greater, but the human and social merits of the plan entitle it to a place in a well-balanced scheme of tuberculosis prevention.

Control of Milk-Supply.—Intimately related to the problem of tuberculosis prevention is the question of the control of the milk-supply. Here the United States has made noteworthy progress, a fact which is attributable in large measure to the autonomy enjoyed by American cities in matters of health. Every city is empowered to take such steps as it considers necessary to prevent the introduction of infectious disease, and the milk ordinance is designed with that purpose. In the U.S.A. the householder ordinarily buys the cheapest milk. It is stated, for example, that a city of 100,000 population will purchase only 200 quarts of certified milk at 10 cents per quart above the usual market price, or 1,000 quarts of grade A raw milk at an advance of 2 cents per quart. In almost any community without milk control an unknown distributor may enter the market and sell milk irrespective of its quality if he offers it at 1 cent per quart below the market price. America now accepts it as proved that the consumer cannot be relied upon to choose a safe, clean milk. The object of the various regulations is to classify the milk in such a way as to guarantee fair competition among the distributors. At first the grades were numerous, ranging from certified milk through grade A raw, grade A pasteurized, grade B, grade C and even lower, but latterly the tendency has been towards simplification, so that in, for example, Pennsylvania, there are now only two grades, grade A raw and pasteurized. In Alabama, there is grade A raw or pasteurized and grade B, all of which must be pasteurized. In addition there is certified milk which complies with the requirements of the National Association of Medical Milk Commissions, but the amount consumed of this grade is insignificant, being rarely more than 0·5 per cent. of the total consumption in any city. All grades must be sold in sterile, tightly closed receptacles,

plainly labelled. No one is allowed to sell milk without a permit and until he has signed an agreement to abide by the regulations. The powers as to condemnation and seizure are of a very summary nature.

In Pennsylvania the growth of control has been gradual, each advance being preceded by educational campaigns to show the necessity of the action proposed.

The pasteurization ordinarily required is thirty minutes at 145° F. but a small committee recently appointed by the Federal Government has reported in favour of a somewhat lower temperature, 142° F. for thirty minutes. They contend that a higher average temperature than 142° not only is unnecessary, but that it affects the taste of the milk and causes separation of the cream.

One consequence of the enforcement of these ordinances in America has been a gradual concentration of the whole business of collection, pasteurization, and distribution in the hands of a few large firms. Thus, practically the whole milk-supply of New York is controlled by three firms. In Philadelphia, five firms handle 80 per cent. of the business.

Another consequence is an increased interest among farmers, State and Federal authorities in the whole question of milk-production from the economic as well as from the health point of view, and an attempt is now being made by the United States Public Health Service to ascertain what are the minimum requirements of a milk ordinance and to see how these can be brought into line with economic considerations.

In the U.S.A., as in England, the great dairy problem is tuberculosis, although there it varies in urgency from State to State and from county to county. In Alabama and the Southern States it is rare to find more than 2 per cent. of the cattle infected, whereas in New England and the North-Eastern States the percentage may be as high as 30 per cent. Some dairymen talk of a time when the Southern States will furnish the whole of the milk for the Union. In Pennsylvania, the areas most affected are those in the neighbourhood of the older cities like Philadelphia, Lancaster, and Reading. Various attempts have been made from time to time to suppress or eradicate tuberculosis in the State, but none has been attended so far with conspicuous success. At present there are two plans in operation: (1) All cattle for dairy or breeding purposes imported into the State must be brought to one of three designated stockyards, where they are examined and tuberculin-tested and the reactors are slaughtered. (2) The "accredited herd plan," a co-operative arrangement into which the owner enters by signing an agreement with the State and Federal bureaux of animal industry. The requirements are uniform throughout the Union. The plan applies either to individual herds or to all the herds in a selected area. The entire herd is placed under the Government, and the tuberculin tests are made free of charge by an official agent. Reactors are slaughtered and compensation paid on a fixed scale, two-thirds by the State, and one-third by the Federal Government. The owner also gets the salvage value of the animal, but in no case must the aggregate amount to more than 90 per cent. of the market value agreed upon with the Federal agent. This plan may also be applied to selected areas such as a county or township, but up to the present only those areas have been selected in which the number of reactors is small. During the test no animals are admitted to the area and no milk sold except from cows proved free from infection. There are altogether 200,000 herds in Pennsylvania, numbering one and a half million cattle. 88 per cent. of these are dairy or breeding cattle and about 10 per cent. are reckoned to be tuberculous. Up to September, 1923, some 260,000 cattle were tested under the "accredited herd plan" at an average cost of 1 dollar 85 cents per head. The increased valuation of the cattle is conservatively estimated at 12 dollars per head. The cost of applying this plan to all the cattle in the State is estimated at 11 million dollars.

Intelligence Division.—The functions of the Intelligence Division of the Public Health Service are:—

(1) To collect and publish information with regard to the prevalence and geographical distribution of communicable diseases and other sanitary data essential to public health administration.

(2) The compilation, publication and distribution of health laws and regulations made by the Federal Government and by State and municipal authorities.

(3) The preparation and distribution of information suitable for the education of the public.

As far back as 1879 an obligation was placed on all consular officers to make weekly reports through the State Department to the Surgeon-General. The

information thus obtained was summarized in the *Public Health Bulletin*. In addition the voluntary co-operation of City Health Officers throughout the Union was secured, until to-day weekly reports are received from some 560 cities. Summaries are also received from thirty-eight State Health Officers. For the purpose of obtaining these returns the Public Health Service appoints in each co-operating State a State Epidemiologist (generally the State Health Officer) and a number of collaborating epidemiologists each of whom receives a salary of one dollar per annum which gives him as a Federal Officer the right to send his mail free of charge. There are altogether 4,000 collaborating epidemiologists sending in weekly returns at the present time. Each case of anthrax and leprosy is made the subject of a special report, and diseases of unusual interest and importance, like plague, are generally verified by an officer of the Public Health Service.

In view of the great credit which is always given to America for its efficiency in the education of the public in health matters, I was surprised to learn that in the States themselves there is some difference of opinion on the subject. Some of you no doubt are familiar with an article entitled "Truth in publicity," by Dr. Chapin, which appeared some eight or nine years ago in the *American Journal of Public Health*, and which urged the dangers of careless propaganda. The American people are tremendously interested in everything relating to health, and public health publicists are beginning to realize that if this trust is not to be abused more care will have to be taken to ensure the quality of the information supplied to them. In this respect the Public Health Service is setting an excellent example in publicity work by confining itself as far as possible to the distribution of information.

Education is given by means of lectures, popular articles, cinema films and broadcasting. The number of copies of popular articles available for distribution is at present limited to about 100,000 per annum, but by using the State and municipal authorities and the various social organizations as "relay stations," the information is widely disseminated. Special attention is paid to the school child, the motto being to "Catch them young." Most of the other work is subsidiary to this main purpose.

Most of the State departments and many of the large cities have also their publicity bureaux: and importance is attached to securing the co-operation of prominent local physicians in this educational work.

Research.

A special division of the Public Health Service is devoted to research. This includes the Hygienic Laboratory and a section for field research and epidemiological investigation.

Nowhere is the need for research more appreciated than in the U.S.A., not only research in the academic sense of the term, but inquiry into the best means of exploiting the results of academic research.

In 1912 the Federal Government authorized research work into the diseases of man without limitation as to time, place or manner. The only limitation is the amount of appropriation available. Two methods of investigation are employed—the experimental and the epidemiological. In general, the principle is to have the work done by full-time employees of the Government, frequently, however, with the advice of part-time outside experts. The officer in charge is made responsible for planning and carrying out the investigation to which he is detailed. A definite sum of money is allotted to him, but within the limits of that appropriation he is given a free hand. He has a central office, either in Washington or at some place strategetically situated with regard to his work, and is given such assistance as is required or can be afforded. As examples of the kind of work on which the division is at present engaged may be mentioned:—

Cancer.—One officer, with some clerical assistants, is now ascertaining what agencies throughout the world are engaged in research, and what lines of inquiry

are being followed. He is also examining the statistics of cancer, and ascertaining whether the disease is really increasing in America.

Child Hygiene.—Here an endeavour is being made to discover a "yard-stick" for measuring child health.

Diphtheria.—A considerable area in the city of Baltimore has been turned over to the care of the Public Health Service with a view to ascertaining the value of prophylactic inoculation of antitoxin, the Schick test, the immunizing value of toxin-antitoxin, &c.

Food-Poisoning.—An office is maintained for the study of botulism, outbreaks of food-poisoning, &c.

Industrial Hygiene.—A study of the factors which affect the efficiency of workers in various trades is being carried out. Attention is being devoted not to individual manufacturing processes, but rather to the investigation of various subjects, e.g., the effect of dust (animal, vegetable and mineral), the effect of posture, the illumination of workshops—and similar fundamental principles.

Leprosy.—Study is being made of chaulmoogra oil and its derivatives in the treatment of leprosy.

Malaria.—Twenty-five trained workers are engaged on this problem in eight or ten cities. Fifteen stations have been established with labour squads.

Stream Pollution.—A large laboratory has been established in Cincinnati with a number of branch laboratories for the purpose of studying the laws which govern the self-purification of rivers. Two rivers are being studied—the Ohio and the Illinois.

And so on, over the whole field of practical research—pneumonia, tuberculosis, goitre, milk-control, mental hygiene, pellagra, Rocky Mountain spotted fever, sewage disposal, and methods of public health administration.

An important function of this division is the administration of the law relating to the sale of vaccines, serums, antitoxins, and such like, the purpose of which is to protect the public from spurious, weak and non-potent drugs, and to prevent the exploitation of fake cures. This work was commenced in 1902. The control is limited to preparations intended for international or inter-state sale, barter or exchange. Six foreign and fifty United States establishments are now recognized for the manufacture of serums, vaccines, &c. The method of control is by inspection and licensing of establishments, and by examination of samples at the Hygienic Laboratory.

Apart from the work done directly by the Research Division, its example is a potent influence for good in State and county administration, and one which is being followed with results which are bound to react most favourably on the quality of local health work.

The laboratory organization in Alabama is particularly good. That in Montgomery, under the capable direction of Dr. Havens, has, in addition to the director, a staff of five technicians, and other assistance. That at Birmingham has a medical director, an assistant director, two bacteriologists, one chemist, one serologist, and clerical assistance. Other State Laboratories are situated at Mobile and Decatur, and two additional laboratories are shortly to be opened at Anniston and Tuscaloosa. The excellence of the Alabama Laboratory Service is doing more than anything else to create confidence in the value and efficiency of the Health Department amongst the public and medical men of the State. Every effort is made to assist the general practitioner in his everyday work, and to make him realize on his side that his co-operation is essential to the success of the department.

Maternity and Child Welfare.

Of the departments outside the Treasury engaged in health work, special reference must be made to the Children's Bureau of the Department of Labour. This Bureau was created in 1912 to investigate and report on all aspects of child welfare. Co-operation with the Public Health Service is secured by means of a Board of

Advisers, of which the Surgeon-General is a member. Attention was early directed to the question of the high infant mortality; and mainly as a result of a report by Miss Lathrop in 1917, in which reference was made to the good results obtained in Great Britain by the system of grants in aid, the Sheppard-Towner Bill was introduced and became law in November, 1921. This Act authorizes an annual appropriation of 1,240,000 dollars for five years, to be expended in the form of grants to States on certain conditions. These are that the State shall pass a Maternity and Infant Welfare Act and establish a Division of Child Hygiene; 5,000 dollars is given unconditionally to each State accepting the Act, and an additional 5,000 dollars if matched by an equal contribution from the State; the balance is distributed on a basis of population. With a few exceptions, all States in the Union have now accepted the Act, and a tremendous impetus has thus been given to this all-important branch of health work. It is gratifying to know that the inspiration came mainly from this country.

I had an opportunity of seeing some of the welfare centres at work in Pennsylvania and Alabama. There are two groups of infant centres in Pennsylvania: (1) those in which the State provides the nurse, and (2) voluntary centres, generally under the Red Cross, which co-operate with the State by sending monthly reports. There are altogether 412 centres, about half of which are directly under State control. A doctor attends at each centre once a week. The nurse records the observations of the doctor, and volunteer workers assist in following up cases and in inducing the parents to secure the necessary medical advice and attention. About 30 per cent. of the defects discovered are remedied in this way; 38,723 babies are enrolled at the centres, and the attendances during 1922 numbered 173,223. In the absence of any requirement as to early notification of births, the centres have to depend mainly on the volunteer workers for new enrolments. I was interested to note the enthusiasm at these centres for the toxin-antitoxin method of immunization against diphtheria. The Federal authorities have consented to the utilization of half, viz., 30,000 dollars, of the Sheppard-Towner Grant for this purpose, and as a result some 50,000 children have already been immunized.

A certain amount of pre-natal work has been done in recent years, partly at the clinics—twelve of the State centres are equipped for this purpose—and partly by the visiting nurses. There are upwards of 100 State nurses provided with apparatus for the examination of urine, blood-pressure, &c. Where any abnormality is discovered the mother is recommended to consult a doctor.

Although very considerable improvement has been effected in infant mortality practically no reduction has yet been achieved in the number of deaths of mothers. During 1922 there were 214,347 births in Pennsylvania, 1,257 mothers died in childbirth, and 367 of these deaths were due to puerperal sepsis. The infant mortality was 88 as compared with 167 in 1906.

The main hope of improvement as regards maternal mortality lies in the education of the midwife: nearly all the midwives are foreigners and for the most part quite untrained. About 1,500 are known but there are admittedly many others of which the State has no knowledge. A start has been made in this work by the appointment of a lady doctor to supervise midwife practice in four counties round Pittsburg, where there is a large alien population, and already as a result of her work, considerable improvement is noted.

School Medical Inspection.—What I saw of school medical inspection in the States did not impress me greatly. Medical inspection of all pupils in public schools in Pennsylvania is a statutory requirement. In school districts of the first, second and third class (i.e., all school districts with a population of more than 5,000) this is carried out by inspectors appointed by the local boards of school directors; in districts of the fourth class, by inspectors appointed directly by the State. Practically all these officers, of whom there are 550, are part-time, and work for the most part independently of the Health Officer. The usual fee is 6 dollars for the examination of the children in a one-roomed school, and for the larger schools in proportion. Inspection of children in the rural districts during the school year 1921-22 revealed that 75 per cent. were defective. Only 29 per cent. of these defects were corrected.

A feature of the school medical system is the Dental Hygienist—a young woman specially trained to clean teeth, and to teach the children in the primary grades *how*, *why* and *when* to use the toothbrush. About forty districts have introduced the Dental Hygienist as part of their school system.

As regards the school buildings, while some appear to be almost perfect, there is still a large proportion (almost half) of the one-room type.

Federal Meat Inspection.

The inspection of meat and meat products intended for inter-state traffic or sale abroad is under the Bureau of Animal Industry of the Department of Agriculture. There are 867 establishments under the inspection of this Bureau. The staff consists of 663 veterinary inspectors appointed after competitive Civil Service examination, 1,614 lay inspectors, twenty-six laboratory inspectors and a clerical staff of 2,423.

The system of inspection includes the sanitation of the premises, ante-mortem inspection of the cattle, post-mortem inspection and inspection of meat products and laboratory examination.

Plans of the establishment must be submitted in triplicate to the Bureau, with the application for approval. It is visited by an expert inspector, who indicates what alterations are necessary. When these are carried out, the establishment is approved and a Veterinary Inspector is placed in charge. In some of the establishments a staff of inspectors is employed, with a supervising inspector.

The inspection is most efficient. The cost works out at about 6 cents per animal, or a total of 3,891,000 dollars per annum. This sum is appropriated by Congress. The inspectors are paid for overtime by the packers. About two-thirds of the total meat consumed in the U.S.A. is subject to Federal inspection.

In addition to the Federal inspection most of the States and local authorities have their own officers for this purpose.

Food and Drug Laws.

The Federal Food and Drug Act, which is administered by the Bureau of Chemistry of the Department of Agriculture, has been in operation for sixteen years. It is not, primarily, a health measure, but it includes provisions of importance from the public health point of view. Its primary purpose is the protection of the consumer against fraud and deceit. It applies to imports and exports of food and drugs, and to inter-state traffic. It does not apply to food or drugs manufactured and sold within the individual states. The law defines adulteration and misbranding and prescribes penalties for these offences. The composition of the drug must be stated on the label, and particularly whether it contains poisonous ingredients. For the purpose of enforcement the Bureau has a staff of 606 chemists, bacteriologists, clerks, &c.: 320 of these have their headquarters in Washington, and 286 in other parts of the States. Of the 286, 111 are scientific workers and forty-six are engaged in the work of inspection. The country is divided into three districts, with a number of inspection stations in each. The staff at each station is assigned a definite territory, within which they must be familiar with all food manufacturing activities. The inspection is of the "hit or miss" variety. It is, of course, impossible to check every single drug. They therefore operate on what is known as the "project" system, selecting some particular food or drug for special attention each year.

There is close co-operation with the State departments, the State officer usually acting as an officer of the Bureau. The efforts of the Bureau are further supplemented by the State organizations and there is free interchange of information between the two authorities.

The food standards laid down by the department are not legally binding in the sense that failure to comply with these will result in prosecution. They are merely definitions which accord with reasonable and reputable trade practice, but they have much weight as evidence in courts of law. As the penalties imposed by the Act are not severe the practice is to seize and destroy the goods.

As regards patent medicines these are regarded as misbranded if they are labelled with false or fraudulent therapeutic claims. Unfortunately, the law does not reach advertisements in the newspapers, where all sorts of claims may still be made. The postal authorities give valuable assistance by prohibiting the transport of fraudulent matter, but manufacturers get over this difficulty by using the express companies.

Many of the States have very drastic laws with regard to the handling of food. Thus, in Alabama, if a café does not comply with the State requirements, it is simply closed. The proprietor has an appeal to the Board of Health, but the place remains closed while the appeal is being heard. The proprietor therefore finds it cheaper to comply with the requirements of the Department than to risk an appeal. The same applies to any place where food is handled.

TRAINING AND CONDITIONS OF SERVICE.

Some reference is due to the training and the conditions of service of health officers in the U.S.A.

In this respect the officers of the United States Public Health Service occupy a unique position. They hold the highest commission in the Union—being appointed by the President with the consent of the Senate—and their position and emoluments are secure. It is far otherwise with most of the local officials, although latterly there is observable a tendency to remove these appointments from the political sphere, and ensure a certain degree of security at any rate to the more important subordinate officials.

In Alabama the State Health Officer is appointed for a period of five years by the State Board of Health which as we have seen is a purely medical organization. The subordinates in the State Health Department are all subject to dismissal by the State Health Officer and there is, nominally at any rate, no appeal from this decision. They are, in effect, simply employees of the State Health Officer. The County Health Officer is appointed by the County Board of Health, subject to the approval of the State Board of Health. He may be removed, however, by the State Health Officer, but in this case there is an appeal to the State Board. The county staff may be dismissed by the County Health Officer.

In Pennsylvania the Health Officer is appointed and dismissed at will by the local Board of Health. County Health Directors, who are all with one exception part-time officers, are under the jurisdiction of the State Secretary of Health. The Secretary himself may be dismissed with a change of Governor. The "spoils" system, so essential to "boss" control, still obtains to a large extent in most departments of local government. But the Civil Service Commission idea is growing, particularly in the larger cities and States, and there is little doubt that in a few years there will be great progress to record in this respect. All that is best in American public opinion is anxiously seeking for any measure that will make for greater efficiency in the management of public affairs.

As regards remuneration of public health officials, this is at present hopelessly inadequate to attract or retain the best type of official. It is unreasonable to expect a professional man to devote his life to a service which offers as its highest prize a salary of 6,000 to 8,000 dollars per annum. For whole-time service in a county a salary of 2,500 dollars per annum is common. Allentown, a city of 80,000 inhabitants, pays its medical officer of health—a most earnest and capable officer—the miserable pittance of 2,400 dollars. These salaries have to be considered in relation to the general standard of living in the U.S.A., which is undoubtedly higher than in this country.

There are no statutory requirements in regard to the training of health officers, but lately a great deal has been done to secure improvement in this direction by the International Health Board and the General Education Board of the Rockefeller Foundation. Five years ago a School of Hygiene was established at Baltimore in connexion with the Johns Hopkins University. Baltimore was selected, mainly by reason of its proximity to Washington and the possibility of maintaining close relations with the United States Public Health Service. It is a School with its own Faculty, capable of granting degrees, and thus differs from a mere training institute. Its aim is to lay a broad, scientific foundation for the science of hygiene and to build on this as the exigencies of the time demand. It is only when such an attempt is made that one realizes on what a slender foundation much of the health advice, often so glibly given, rests. One of the first and most difficult duties of anyone attempting to systematize our knowledge on this subject is to clear away the accumulated débris of ill-founded tradition and *obiter dicta*.

The main divisions of work in the School are statistics, bacteriology and immunology, chemistry and the problems of nutrition, medical zoology and entomology,

public health administration and epidemiology. Generous recognition is given by the School to the services of Sir Arthur Newsholme, the first occupant of the Chair of Public Health Administration.

Contact is maintained with the City Health Department and it is hoped ultimately to arrange for investigation areas in the city and the State, in which the school will defray part of the cost of administration. The degrees given by the School are: (1) Doctor of Public Health (after two years' study); (2) Doctor of Science in Hygiene, for non-medicals; (3) Certificate in Public Health, after a year's study, (comparable to the D.P.H.).

Other courses of shorter duration are given for the benefit of health officers on vacation.

It is to be noted, however, that at present, owing mainly to the lack of attraction of a public health career, the number of students is small and appears to be restricted chiefly to those with Rockefeller Scholarships.

Conclusion.

I have endeavoured to describe as briefly as possible what I have seen of official health organization in the United States. Passing reference has been made to the work of some of the great national voluntary associations, such as the National Tuberculosis Association, and the privately endowed foundations, such as the Rockefeller Foundation and the Millbank Memorial Fund. Working in close co-operation with the Public Health Service and State departments, these various agencies are gradually, by a process of "peaceful penetration," stirring up local authorities to a sense of their duty and responsibilities in health matters, overcoming by education the apathy and indifference of the public, and showing by practical demonstration that money spent on preventive work is, even from the purely material point of view, a sound investment. Already much has been accomplished but much still remains to be done, particularly in the rural and smaller urban communities, to bring local health services up to a reasonable standard of efficiency.

The field of public health in the United States is wide, and, in endeavouring to obtain a comprehensive view, it is not always easy to be sure whether one is standing on the heights or in the hollows. There is much to criticize, but it has seemed to me more profitable to concentrate on positive achievement. One is perhaps too prone to apply the yardstick of one's own experience and to judge American achievement by our own, but it is important to remember that public health effort is, in the main, a reaction of the community to its environment, and if in some respects progress in this country has been greater and along different lines to that in the States, it is simply because our conditions are different. It cannot be denied that at any rate, so far as the native-born white American population is concerned, the conditions of life in the States are, in the main, superior to those in this country. It is otherwise, however, with the negro and poorer alien elements of the population, many of whom are living under conditions that are as bad as the worst in England, and it is important in drawing comparisons to take account of this "residuum" of the population, whose place in this country is taken by our own kith and kin. The cynic may perhaps regard it as a somewhat unsatisfactory foundation on which to rear such fair edifices of idealism and material comfort as one encounters in the United States, but there are compensations. The increase of wealth and the growth of a leisured class in the United States has not been an unmixed evil. It has liberated the energies of many thoughtful and earnest men and women, whose activities, although sometimes embarrassing to official agencies, are in the main wholesome and beneficent.

One comes away from America with an impression of a people abounding in energy, generous in giving, eager to learn, and adventurous in application, realizing that the continued improvement of individual and communal health is worth striving after, and achieving it in its own way.

DISCUSSION.

Dr. J. C. McVAIL (President) said that America, like the rest of the world, owed much of its public health inspiration to Britain, where sanitation had been the offspring of dire necessity, arising from the overcrowding of its urban industrial population under the factory system. Even yet the density of population in England was about twenty times that in America, where the cities, though great, were far distant from each other, and with a very meagrely inhabited country between. Though America owed much to Britain, it had also, with characteristic initiative, struck out on its own account. As indicated by Dr. Carnwath, its attitude towards the rights and liberty of the individual in relation to the rights and liberty of the State was of amusing interest to an onlooker from this side of the Atlantic An immigrant was likely to discover that whilst in theory the individual was absolutely free, yet the State also was absolutely free to do whatever it thought necessary for its own protection, and when these two freedoms in any way clashed, the State, being much stronger than the individual, took its own way while professing the profoundest respect for the liberty of the subject.

Sir GEORGE BUCHANAN said he considered the paper a mine of useful information, and that he regretted that the time at their disposal had made it necessary for Dr. Carnwath to omit notes on some sections of American work which would certainly have been interesting to the Section, particularly as Dr. Carnwath had succeeded so well in the very difficult task of presenting in short compass a review of the impressions made on a well-qualified observer during his American tour. Dr. Carnwath had given evidence of the great value of the system of interchange of public health officers arranged by the League of Nations.

Dr. J. PEARSE remarked that Dr. Carnwath's paper was so comprehensive, that its adequate discussion would require much longer time than was available. He had been specially interested in the observations regarding the relationship between central and peripheral authorities, a subject which was in itself sufficient for prolonged consideration.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

Overcrowding and Epidemic Disease.

By F. E. WYNNE, M.B.

Medical Officer of Health, City of Sheffield.

WHEN I was first honoured by your invitation to address this Section, I hoped that the inquiries which I had begun to undertake as to the relation between the incidence of infectious disease and overcrowding in Sheffield, might enable me to place some fairly definite conclusions before this meeting.

In this hope I have been to some extent disappointed and I must apologize in advance for the fact that I can only offer you a few figures and some very tentative deductions, hoping that these may suggest a line of research to colleagues who may be able to prosecute inquiries on similar lines, but on a larger scale and with greater skill.

Members of the Public Health Service are, I think, entitled to look with some pride on the remarkable improvement which has been effected in the national health during the last two or three decades, and I believe that "Preventive Medicine"—using that term in the sense of the sum of all those activities now largely directed by the Public Health Service—has been a very important, if not the principal factor, in bringing about this fortunate state of affairs.

But everyone in direct touch with the life and environment of our population knows that there is as yet no ground for complacency. None of us are satisfied with our results, while the conditions of so many of our fellow-citizens, especially in large urban communities, are still in so many respects deplorable.

Perhaps the most hopeful sign for public health in the future is the fact that with a general death-rate reduced to one half of what it was, and an infantile mortality-rate almost equally reduced, I do not think there is a single medical officer of health who suffers from "cephalic hypertrophy" on that account. On the contrary, my experience is that we never meet together without asking each other: "Why are the people so well?" "How do they survive?" "What have we done to be saved?"

In the complexity of our modern social organism I am sure there are factors making for improved health and increased resistance to disease, some of which we cannot at present define. They exist perhaps on the plane of social subconsciousness, and it may be that until we can lift them to the level of consciousness, name and catalogue them, we shall not be able fully to bend them to our uses. That there are some such factors at work enabling people to withstand our present relapse into conditions of overcrowding without a recrudescence of something like the old mortality-rates and sickness-rates I think can hardly be questioned.

If the sanitarian of twenty years ago could have been told what our social conditions were to be to-day, had been warned that a social dislocation would occur involving some ten years during which building would be reduced below the level required to compensate the natural increase of population, during much of which even necessary repairs and essential sanitary improvements would be at a standstill, during which unemployment would reach unheard of figures, and over-

crowding would revert to something approximating the conditions prevailing in the middle of the last century, I am sure that the sanitarian would have confidently predicted a reversion to the old death-rates, and, especially, an enormous increase in the incidence and mortality of infectious diseases. Such a prediction would have appeared then to be justified.

Twenty years ago the nation was prosperous, and at peace. The South African War had shaken its self-esteem, but not its self-confidence or its trade. The Public Health Act had been in operation for a generation, and its effect, and that of much consequent legislation, was beginning to be harvested. The building trade was prosperous and the Housing of the Working Classes Act had been in operation for fifteen years with tangible results. The Education Act had been in force for more than a generation, and people were beginning to look for the harvest, though up to then, the seed-time had been comparatively inexpensive, from the point of view of modern standards.

We were on the eve of all the recent work which has dealt so much with the individual as opposed to the environment of the masses.

That generation had provided for an adequate and pure water supply, at all events for urban populations, and much had been done in connexion with drainage, sewerage and disposal of sewage. Communities were no longer dying from the accumulation of their own waste products as they did up to eighty years ago or less. There was a definite prospect of a gradual but steady improvement in housing accommodation, and of the abolition of slums. The engineer and the architect were certainly in the ascendant in Public Health matters, but the medical officer was there as a somewhat obscure and always ill-paid source of inspiration to local authorities and their officials.

This period was followed by the passing of the Housing and Town Planning Act of 1909, and under that Act a great deal of valuable work in the way of slum clearance became possible, and in most districts it was being carried out with considerable success right up to the outbreak of the war.

Unfortunately the passage of that Act was almost immediately followed by a decline in building activity, mainly due in the first instance to other legislation which it is not necessary to specify here, and this was followed by the complete paralysis caused by war conditions. But the seriousness of the partial paralysis of pre-war years is not, I think, generally recognized. In his introduction to Vol. VI of the Census Reports for 1911 the Registrar-General says:

"Speaking generally the number of uninhabited houses was less in proportion to the number of inhabited houses at the census of 1911 than it had been at any of the four preceding censuses; and the proportion of houses being built was very much less than it had been at any previous census" [1].

From the table which follows I extract these very significant figures:—

	Inhabited houses	Uninhabited houses	Houses being built
1901	6,260,852	448,932	61,909
1911	7,141,781	408,652	38,178

Unfortunately the corresponding figures for the 1921 census are not yet available.

That overcrowding now exists—and to an extent probably unequalled since the "forties" of the last century, when the social results of the industrial revolution were at their worst, and hunger, under the Corn Laws, was the real scythe of Death—is, I think, admitted. And it is of great interest to us to find in the speeches of Lord Shaftesbury and of Cobden, references to the frightful influence of overcrowding on the population of those days. We all know that this terrible evil exists, and I have some reason for believing that it is perhaps even more acute in Sheffield than in other large urban agglomerations of population.

But it is curiously difficult at present to obtain anything like an accurate measure of this overcrowding. No large figures tell us anything at all about the cases painfully familiar to most of us, where eight or nine or more persons of both sexes and all ages live in one or at most two rooms, *to-day;* where a baby may be born in the presence of a whole family, and interrupt the cooking of the mid-day meal; where youths and maidens have no separation, no privacy, where nothing is hidden, where nothing is sacred, where there is no solitude, no decency, where everything even death itself, must happen under the eyes of everybody else, as though that human family and their lodgers were a nest of stoats or weasels, or rabbits. Such are the facts with which we are all familiar, but which may be represented statistically by the mere shifting of a decimal point. We ought of course to be able to estimate the facts of overcrowding with something approximating to the accuracy of a chemical titration, but for the purposes of this paper I have found this impossible.

In the prefatory remarks of the Registrar-General to the census of 1911, already quoted, he makes the following statement: "The tables in the present volume do not afford any information as to the relative amount of accommodation in the inhabited buildings returned at the two censuses" [2], and in the Census Reports of 1921 the difficulty is increased by certain alterations in arrangement, for instance in the change of the figures for persons per house to rooms per family; this involves a transposition of figures which cannot always be carried out with complete accuracy when one attempts to compare the conditions of one period with those of another.

The following table, however, gives us some comparison of the conditions both in England and Wales and in Sheffield on the two last census dates. But the inclusion in one group of families occupying from one to nine rooms, of course enormously dilutes the real facts as to overcrowding among the most poverty-stricken and huddled sections of the community.

TABLE I.—PERSONS PER FAMILY, AND DENSITY OF OCCUPATION AT CENSUSES 1911 AND 1921.

	England and Wales		Sheffield	
	1921	1911	1921	1911
Average size of private family (persons)	4·14	4·36	4·37	4·49
Average number of rooms occupied per family:—				
(*a*) In all units of occupation ...	4·55	*	4·43	*
(*b*) In units of occupation of one to nine rooms only ...	4·35	4·52	4·31	4·61
Average number of rooms occupied per person:—				
(*a*) In all units of occupation ...	1·10	*	1·01	*
(*b*) In units of occupation of one to nine rooms only ...	1·06	1·05	0·99	1·03

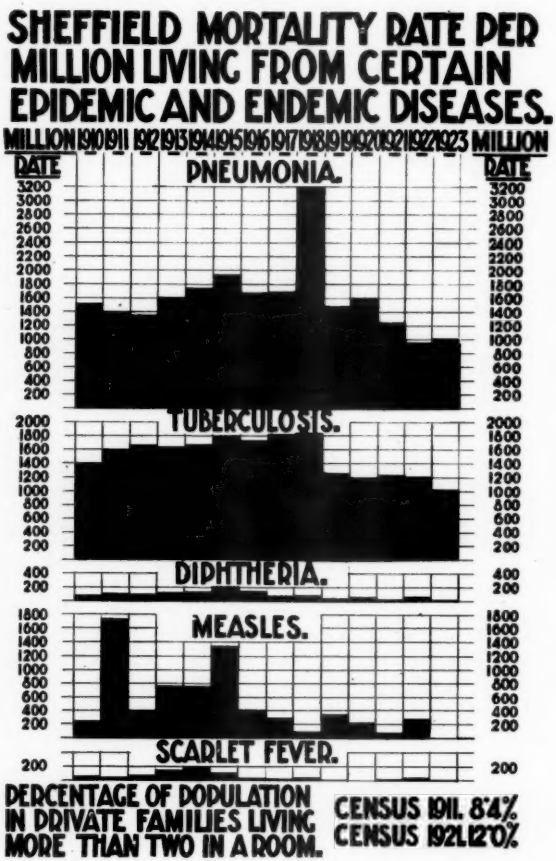
* Figures not available.

It will be noticed that both in Sheffield, and in the country generally, the size of families has decreased with the decline in the birth-rate for the two areas, the birth-rate for England and Wales having declined from 24·4 to 22·4, and that for Sheffield from 27·7 to 23·77. But in England and Wales, in spite of the universal and admitted shortage of houses, there is an actual increase of 0·01 in the number of rooms per person, whereas in Sheffield there is a decrease of 0·04. Even these small fractions are significant when we remember that the groups of population under consideration include the large numbers whose standard of living and housing has improved since pre-war days.

Many of the worst cases of overcrowding occur among the dishoused, who are now occupying one or more rooms in sublet houses or "houses let-in-lodgings."

Of the number of these people I have not yet obtained any satisfactory index, but in Sheffield I find that the percentage of the population in private families living under conditions of more than two per room has increased from 8·4 in 1911 to 12 in 1921. For 1923, therefore, I think we are safe in assuming that the ratio of overcrowding was at least as 3 to 2, as compared with 1911, and in considering the incidence of infectious disease I am assuming this figure as a minimum. This being so, if over-

DIAGRAM I.


SHEFFIELD MORTALITY RATE PER MILLION LIVING FROM CERTAIN EPIDEMIC AND ENDEMIC DISEASES.
MILLION 1910 11 12 13 14 15 16 17 18 19 1920 21 22 1923 MILLION
RATE
PNEUMONIA.
TUBERCULOSIS.
DIPHTHERIA.
MEASLES.
SCARLET FEVER.
PERCENTAGE OF POPULATION IN PRIVATE FAMILIES LIVING MORE THAN TWO IN A ROOM.
CENSUS 1911. 8·4%
CENSUS 1921. 12·0%

crowding were still a main factor in promoting the spread of diseases of microbic origin, we should be entitled to expect a material increase in the incidence and mortality from these causes in Sheffield during this intercensal period. The above diagram, however, shows that in Sheffield, as throughout the country, there has been a steady decline in the mortality from these causes. The curves for enteric fever and infantile diarrhœa would have been even more impressive, the

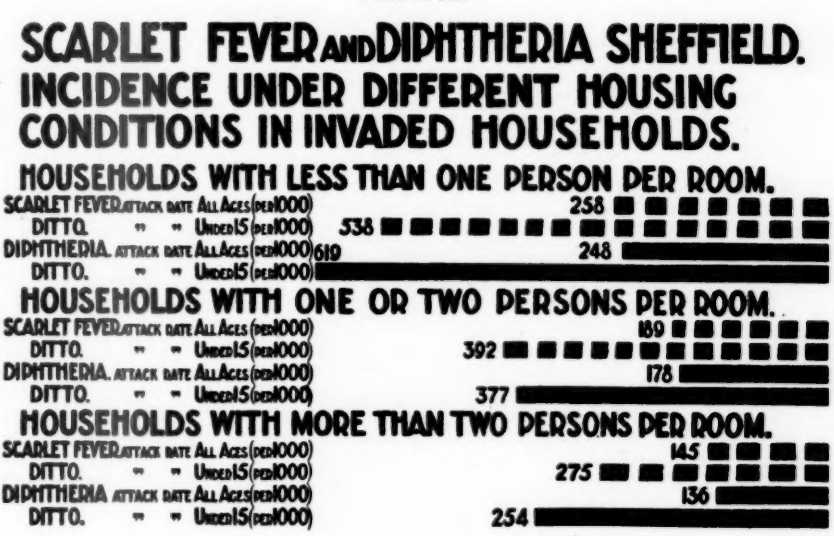
former having virtually disappeared, and the latter declined to one-fourth of the figure for ten years ago.

This is a remarkable contrast to the figures given by Dr. J. B. Russell in 1888, and quoted by Sir Arthur Newsholme in his "Vital Statistics" [3]. At

DIAGRAM II.

SCARLET FEVER AND DIPHTHERIA SHEFFIELD.
INCIDENCE UNDER DIFFERENT HOUSING CONDITIONS IN TOTAL HOUSEHOLDS.
HOUSEHOLDS WITH LESS THAN ONE PERSON PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 2·35.
DIPHTHERIA ATTACK RATE ALL AGES (PER 1000) 0·62.
HOUSEHOLDS WITH ONE OR TWO PERSONS PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 2·54.
DIPHTHERIA ATTACK RATE ALL AGES (PER 1000) 0·84.
HOUSEHOLDS WITH MORE THAN TWO PERSONS PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 3·22.
DIPHTHERIA ATTACK RATE ALL AGES (PER 1000) 1·16.

DIAGRAM III.


SCARLET FEVER AND DIPHTHERIA SHEFFIELD.
INCIDENCE UNDER DIFFERENT HOUSING CONDITIONS IN INVADED HOUSEHOLDS.
HOUSEHOLDS WITH LESS THAN ONE PERSON PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 258
DITTO. " " UNDER 15 (PER 1000) 538
DIPHTHERIA. ATTACK RATE ALL AGES (PER 1000) 610 248
DITTO. " " UNDER 15 (PER 1000)
HOUSEHOLDS WITH ONE OR TWO PERSONS PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 189
DITTO. " " UNDER 15 (PER 1000) 392
DIPHTHERIA. ATTACK RATE ALL AGES (PER 1000) 178
DITTO. " " UNDER 15 (PER 1000) 377
HOUSEHOLDS WITH MORE THAN TWO PERSONS PER ROOM.
SCARLET FEVER ATTACK RATE ALL AGES (PER 1000) 145
DITTO. " " UNDER 15 (PER 1000) 275
DIPHTHERIA ATTACK RATE ALL AGES (PER 1000) 136
DITTO. " " UNDER 15 (PER 1000) 254

that time, nearly forty years ago, Dr. Russell was able to demonstrate a very high degree of correlation between the incidence of zymotic disease and overcrowding, the rate per 100,000 of the population being 478 in one- and two-roomed houses, 246 in three- and four-roomed houses, and 114 in houses of five rooms and upwards.

In attempting to trace this relationship in Sheffield in the case of certain specific diseases during 1923, I find the influence still exists, though to a much less extent.

The diagrams on the preceding page show the incidence of scarlet fever and diphtheria in households coming under the following categories, viz., (1) less than one person per room, (2) one or two persons per room, and (3) more than two persons per room.

These figures are not strictly comparable with those of Dr. Russell, because the standard of housing taken is different, and his figures relate to the whole group of zymotic diseases, whereas these relate only to scarlet fever and diphtheria. They do, however, give us measures of closely related phenomena, and it seems to me that the discrepancies are sufficiently marked to justify us in suspecting that overcrowding *per se* has become a less important factor in the spread of infectious disease than it was even forty years ago.

TABLE II.

SHEFFIELD – SCARLET FEVER AND DIPHTHERIA INCIDENCE 1923, UNDER DIFFERENT HOUSING CONDITIONS IN INVADED HOUSEHOLDS.

	PRIMARY CASES AMONG CHILDREN UNDER 15 YEARS PER 1000 AT SAME AGE.		SUBSEQUENT CASES AMONG CHILDREN UNDER 15 YEARS PER 1000 AT SAME AGE.	
	SCARLET FEVER.	DIPHTHERIA	SCARLET FEVER	DIPHTHERIA
INVADED HOUSEHOLDS WITH LESS THAN ONE PERSON PER ROOM.	511.	605.	27. 5·3%	14. 2·3%
INVADED HOUSEHOLDS WITH ONE OR TWO PERSONS PER ROOM.	357.	356.	35. 9·8%	21. 5·9%
INVADED HOUSEHOLDS WITH MORE THAN TWO PERSONS PER ROOM.	245.	235.	30. 12·2%	19. 8·1%

This illustrates the fallacy in Diagram III by showing the increase in secondary cases as overcrowding increases.

In Dr. Russell's figures the increase in incidence of zymotic diseases in the worst houses as compared with the best houses is 419 per cent. In our Sheffield figures the increase is 37 per cent. in the case of scarlet fever and 87 per cent. in the case of diphtheria. Very similar figures have been obtained by Dr. J. Macmillan, Medical Officer of Health, Woolwich, in that borough [4]. There, over a three-year period, the attack-rate was 0·4 per 1,000 in families of less than one person per room, it rose to 1·53 per 1,000 where there were one to two persons per room, and to 3·31 where there were more than two persons per room.

I regret that so far I have not been able to discover similar figures for other towns, but hope to do so during a more extended inquiry.

Diagram III also shows the incidence on the population in houses attacked only,

and here we get apparently an actual advantage among the less well housed population. This, however, is illusory, because the better housed comprise the well-to-do with smaller families, and this introduces an obvious fallacy in the calculation of a rate. For instance, a family consisting of parent and one child with one servant, when the child is attacked, will give a rate of 25 per cent., whereas one case occurring in a family consisting of parents and eight children will give a rate of 10 per cent.

The figures are only of value as confirming in some degree the evidence that infections are not spreading among the overcrowded to the extent which, as I have said, I believe would have been predicted even twenty years ago. It may properly be objected against these figures that the infection of scarlet fever and diphtheria is presented to a population of whom an unknown proportion is already immune; and that of course is true, though the objection has less weight when we consider the figures for the population over fifteen years.

But if we take tuberculosis we find almost the same relation. The incidence in the first class of household is 2·18, in the second 4·03, in the third 4·45, that is, the incidence in the last class is only a little more than double that in the first class. Dr. Russell in his paper, already quoted, groups "acute diseases of the lungs and consumption," so that again neither his group of diseases nor standard of housing is quite comparable, but his figure for the best class is 328 per 100,000, and for the worst class 985, almost exactly three times as much. In this case also the Sheffield figures show the same apparent advantage among the worse-housed population where the dwellers in invaded households only are considered, the rates being then 289, 170, and 117 in the three categories. Of course the same criticisms apply to these figures as those I have already suggested in connexion with scarlet fever and diphtheria.

Apart from these diseases there are two which are perhaps more than any others associated with overcrowding, namely typhus and cerebro-spinal fever. The association between typhus fever and overcrowding is so marked that it was perhaps the first disease to attract attention to the ill-effects of this condition. As Murchison says:—

"All the historians of the great epidemics of typhus fever testify to the intimate connexion between its prevalence and overcrowding" [5].

It is noteworthy, however, that he insists that—

"Destitution and deficient alimentation are the most powerful predisposing causes of typhus" [6].

With respect to cerebro-spinal fever the author of the chapter on this subject in the "Official History of the War, Medical Services" states that:—

"From the study of recent epidemics one causative factor stands out with startling distinctness, namely overcrowding. In 1915 it was at once recognized that wherever cubic space, either in huts or billets, fell below standard, cases began to occur. Captain Glover's work at a depot in 1917 gives an admirable picture of the mode in which epidemics arise from overcrowding. Glover found that the carrier rate with the peace standard of one yard between beds rarely exceeded 5 per cent. Mobilization standard of 1 ft. 4 in. between beds yielded a carrier rate of 10 per cent. At 1 ft. it rose to 20 per cent., and at less than 9 in. 28 to 30 per cent. When the carrier rate rose to 20 per cent. or over, cases began to occur. At this depot the carrier rate rose as high as 70 per cent. Spacing out produced a fall in the carrier rate in a slower manner than the preliminary rise" [7].

Notwithstanding this very close association between these diseases and overcrowding, typhus has continued to be unknown in this country, with the exception of one recent outbreak at St. Helens, which was limited to a few individuals and was at once stamped out; while, since demobilization, there has been no marked increase

in cerebro-spinal meningitis, although that disease appears now to have become endemic in this country and may still constitute a menace of which we must not be unmindful.

If we are justified by the foregoing considerations and other evidence which I hope will soon be available, in assuming that overcrowding *per se* is a less potent influence in promoting the spread of infectious disease than it was in the days of our forefathers, it is important for us to identify as far as possible the factors which have caused this recession in its importance.

In every case of infection there are two elements to be considered, namely the virulence of the infecting agent, and the degree of immunity possessed by the individual attacked. Since the incidence and severity of specific diseases vary as they do over considerable periods, it is often most difficult to determine which of these two elements is the dominant one. But that there are cyclical variations in the virulence of some infecting organisms I am convinced.

Most of us have witnessed the remarkable decline in the severity and fatality of scarlet fever, which has been for years, in the great majority of cases, an almost trivial complaint. This change set in before there was any marked improvement in the general standard of living to justify an appeal to that much-abused phrase "increased resistance of the patient." If any such "increased resistance" to scarlet fever were present, it was specific and not general, for the virulence of measles showed no diminution, and I am satisfied that the virulence of chicken-pox has materially increased. When I was in practice, the last-mentioned was a disease that produced practically no constitutional disturbances whatever. In fact the rash is usually stated to appear on the first day of the disease, simply because the period of onset was generally unrecognizable. But of late years I have been called in to examine many cases of chicken-pox which were very properly reported as doubtful cases of small-pox by practitioners unfamiliar with that disease, simply on account of the severity of the constitutional symptoms and the profuseness of the eruption. The antecedent improbability of a population such as that of England and Wales acquiring an increased resistance to scarlet fever, none to measles, and a reduced resistance to chicken-pox is, to my mind, so great that I am driven to conclude that it is at least more probable that the infecting organisms have been subject to influences, at present unknown, which have modified their virulence. Something has also happened to small-pox, but that is ground at present so controversial that I dare not tread on it!

The recent rapid elimination of enteric fever is another problem for which we cannot at present satisfactorily account on any hypothesis except a decline in the virulence of the *Bacillus typhosus*, while the organisms responsible for pneumonia remain as active and vicious as ever.

I believe therefore that there are variations in the infectivity of many pathogenic organisms which, on balance, at present make for a lessened incidence of disease, quite apart from environmental conditions.

Apart from such admittedly questionable theories, we have the very definite effects of increased knowledge of ætiology and rational therapeutics. Small-pox might have been eliminated a generation ago were it not for the magnificent defence put up by its allies. Diphtheria has been rendered almost innocuous and largely preventable when our modern weapons are used with sufficient promptitude and dispatch. Typhus fever would very probably have got a footing among us during and since the war had we still regarded the infection as due to "inhalation of emanations from the patient"—the doctrine even in Murchison's day. As it is, the conquest of typhus in Serbia during the war, and the protection of our own troops, is one of the romances of modern preventive medicine. Military experience has shown that enteric fever can be completely controlled among enormous bodies of

men especially exposed to it, and there is now at least strong presumptive evidence that the child population may be protected against scarlet fever by the toxi-antitoxin method, so long as the administrative machinery is provided, and knowledge is not again compelled to give way "when the heathen rage furiously together and the people imagine a vain thing."

Another very important factor is the increased accommodation in isolation hospitals and improvement in notification and administration.

There remains the question of "resistance to disease." While bacteriology and serology have taught us something about the mechanism of acquired immunity, we still know very little about the conditions which confer natural immunity. It seems common sense to believe, as our ancestors did, that this belongs primarily to the individual who possesses what is commonly known as good health—the happy individual with a good heredity and good habits, free from organic disease, with tissues well aërated, well nourished, and well drained. Such a condition can only persist in a healthy environment, and the older physicians were doubtless right in dividing their discussions of ætiology into "predisposing" and "exciting" causes. And here I would again remind you of Murchison's dictum that "destitution and deficient alimentation are the most powerful predisposing causes of typhus fever." This is no doubt equally true of all infections, and it must be remembered that while we have lamentably relapsed in the matter of housing, yet during all the post-war period of dislocation of trade and unemployment, we have not allowed destitution to exist. Everybody has been fed, and among the wage-earners and the unemployed, fewer people have been overfed. Enforced economy has probably a most valuable effect in the selection of the dietary in the humbler households, and there is some evidence that the work of Gowland Hopkins, Mellanby, and other workers on nutrition, is already percolating through various health agencies so as to have a practical effect on the habits of the poorest.

Another cause which is certainly exercising a notable effect on the health, and presumably the resistance, of individuals is the great decrease in the consumption of alcohol as the result of poverty, increased prices, and restricted hours of opening of licensed premises. This effect is both direct and indirect, for I am assured by experienced health visitors that many women are now found looking after their children and homes in the morning, who a few years ago would have been located in the nearest public-house. Other social factors are public health education—which is advancing almost everywhere except in the schools—and the effect of National Health Insurance.

Again we must remember that whilst we have reverted to overcrowded conditions in private houses, and houses let-in-lodgings, public opinion no longer tolerates the fearful conditions that prevailed in the mid-nineteenth century in common lodging-houses, gaols, workhouses and other institutions where the unfortunate were compelled to sleep. In a speech made in 1851 Lord Shaftesbury gave the following account of a room in a lodging-house in London:—

> "It is filled to excess every night, but on particular occasions commonly fifty, sometimes ninety to one hundred men are crowded into a room 33 ft. 9 in. long, 20 ft. wide, and 7 ft. high in the centre. The whole of this dormitory does not allow more air space than is appropriated in the wards of the fever hospital for three patients" [8].

He adds that more than one-fifth of the admissions to the fever hospital in one year were received from that room alone. In Mr. C. J. Stewart's report to the London County Council on the housing question from 1855 to 1900 he quotes another case where fifty-eight people and two or three dogs slept in a room 18 ft. by 10 ft. [9]. Bad as our conditions now are, I doubt if such cases as these could be paralleled anywhere in this country.

While I do not wish in any way to minimize the very serious consequences,

moral, social, and physical, of our present overcrowding, I think it is of great importance that we should seek out and identify the factors that are tending to minimize at all events the spread of infection, in order that we may at least mitigate these evils during the period that must elapse before we see a population adequately and decently housed.

REFERENCES.

[1] "Report of Registrar-General on Census of 1911," pp. x, xi. [2] *Ibid.*, p. vii. [3] NEWSHOLME, Sir A., "Elements of Vital Statistics," 3rd edition, pp. 162-3. [4] MACMILLAN, "Annual Report on the Health of Woolwich, 1923," and *Medical Officer*, December 20, 1924. [5] MURCHISON, C., "Continued Fevers of Great Britain," 3rd edition, p. 72. [6] *Ibid.*, p. 73. [7] "Official History of the War, Medical Services," i, pp. 149-150. [8] STEWART, "The Housing Question in London," p. 68. [9] *Ibid.*, p. 69.

Discussion.—Dr. MAJOR GREENWOOD said that Dr. Wynne's paper was most instructive. It was a contribution to both science and literature. It was not very creditable to the Public Health services of the south of England that this valuable address should have been delivered to so small an audience. He (Dr. Greenwood) thought that Dr. Wynne's comment upon the apparent paradox of Diagram III was just; it might be remarked that if a character were distributed wholly at random through a population grouped in houses, or families, of different sizes, and if one selected *only the houses wherein at least one inmate of each possessed the character*, the proportional frequency of the character among the inhabitants of houses of different sizes would diminish with the size of the house. When the character was not distributed at random, e.g., when it was an infectious disease, the problem was more complex but he thought the same principle was at work. He fully agreed with Dr. Wynne that the better nutrition of the people had been a prime cause of the mitigation of the evils of overcrowding to which Dr. Wynne had called attention. Perhaps one might say that the most serious aspect of the present housing position was not so much its direct relation to the spread of physical disease as its influence upon an absolutely large, if relatively small, number of persons now living under conditions wholly incompatible with a reasonable standard of civic feeling. He (Dr. Greenwood) recalled the words of Simon, writing of overcrowding almost eighty years ago:—

> "I have to deal with the matter only as it relates to bodily health. Whatever is morally hideous and savage in the scene—whatever contrast it offers to the superficial magnificence of the metropolis—whatever profligacy it implies and continues—whatever recklessness and obscene brutality arises from it—whatever deep injury it inflicts on the community—whatever debasement or abolition of God's image in men's hearts is tokened by it—these matters belong not to my office, nor would it become me to dwell on them. Only because of the physical sufferings am I entitled to speak. . . . But while I lift the curtain to show you this—a curtain which propriety might gladly leave unraised, you cannot but see that side by side with pestilence there stalks a deadlier presence; blighting the moral existence of a rising population; rendering their hearts hopeless, their acts ruffianly and incestuous; and scattering, while society averts her eyes, the retributive seeds of increase for crime, turbulence and pauperism."

He would like to think, but he could not think, that these words had no application to the present time.

Dr. G. CLARK TROTTER (Medical Officer of Health, Islington), stated that the statistics given in Professor Wynne's paper had involved a great deal of laborious work. A year ago, Dr. Harold Kerr, of Newcastle-upon-Tyne, had read a paper before the Section [1] likewise involving the tabulation of figures and the expenditure of much time. In both instances—Professor Kerr's and the present—the authors had, in spite of the labour involved, not reached conclusive findings as a result of their work. This was not to be taken, however, as a deterrent; rather must it be remembered that until such work was done, it was impossible to estimate the result, and it was necessary that it should be performed in order to say what the

[1] "Refuse Disposal in relation to the Enteric Group of Diseases," *Proceedings*, 1924, xvii (Sect. Epid.), p. 33.

result would be: therefore the papers had a very definite value. Would Professor Wynne state whether there was any risk of fallacy in respect, for instance, of houses of three rooms and over? Whether every room in the house was actually in constant use? It was a custom in certain parts to reserve a "best" room, which was only used on special occasions. Such a room, although it existed, if it were not usually used, could have no real statistical value. He was able to confirm from his own experience Professor Wynne's statement with regard to chicken-pox being frequently of a more severe type now than was the case some years ago.

Dr. PERCY STOCKS, in expressing appreciation of Professor Wynne's paper, remarked further upon the curious statistical fallacy involved in comparing percentage rates of incidence of an infectious disease in the populations of invaded houses when these were grouped according to the number of persons per room. He had reason to know that fallacies due to the same kind of unconscious selection arose in the statistical analysis of pedigrees of families affected by hereditary diseases.

Dr. JOHN C. MCVAIL (President) said that the Section was greatly indebted to Professor Wynne for placing before it the results of his investigation, so far as yet carried out, into the present-day influence of overcrowding on the prevalence of infectious disease. Since 1909, and especially since the War, overcrowding had immensely increased, yet several infectious diseases had diminished in prevalence or fatality, or in both respects. Notwithstanding this contrast Dr. Wynne had shown that room density did still definitely influence transmission of these diseases from one inmate to another, though not to anything like the same extent as Dr. Russell had recorded for Glasgow forty years ago. Of course the maximum purity which the air of an inhabited room could attain would be that of the atmosphere outside, and improvement in the latter would affect the former. In Farr's time the whole subject was dealt with not in terms of room density but of square mile or acreage density. As regards room density, in comparing England with Scotland, it had to be understood that while the number of rooms in a dwelling in Scotland was on the average decidedly less than in England, yet the average size of the apartment was greater, so that in Glasgow now three persons per room was taken as the standard compared with two persons in England. Dr. Wynne had rightly emphasized the importance of better feeding, both in amount and character, in its influence on resistance. In the Lancashire cotton famine in the last century there was the apparent paradox that infant health improved, but that was owing to the women mill-workers being able, when unemployed, to feed their babies at the breast.

Dr. GRAHAM FORBES said that he could not agree that the schools were largely responsible for the spread of infectious disease. In London schools a close watch was kept, particularly for the occurrence of measles, scarlet fever and diphtheria, and on the first appearance of infection, steps were taken to limit its spread by examining all children in affected class-rooms and excluding any who came under suspicion. In the case of diphtheria the systematic swabbing of throat and nose had also proved of great help in the detection of carriers and cases in the initial stage. In time past attempts to check outbreaks by school closure had not proved effective and had therefore been replaced by the present more reliable system of control by personal inspection. The results of these school investigations seldom provided evidence that the spread of infection was attributable to school influence. He was of opinion that out-of-school conditions, less capable of control, through contact in the overcrowded home, the cinema and elsewhere, offered far more facility for the dissemination of infection than the schools, where medical supervision was constantly being exercised. The discrepancies shown in the final columns of Professor Wynne's statistical tables, where the heavier incidence of scarlet fever and diphtheria was apparent in the less crowded homes of the more well to do, could perhaps be explained by the fact that the higher in the social scale, the less was the chance of exposure

to frequent small doses of infection, hence the less opportunity for the development of immunity and the greater amount of individual susceptibility. This had been well demonstrated by the results of the very numerous Schick tests carried out in America. For the percentage of Schick positives, or persons susceptible to diphtheria, was far higher in the better class, and more protected rural schools, than amongst the poor, who, through closer contact with the infection in overcrowded homes and in the more populous quarters of towns, had presumably acquired a greater degree of immunity, both individually and collectively.

Dr. WYNNE (in replying to the discussion) said that in the industrial centres of the North the sanctity of the "front room" had largely disappeared under the pressure of circumstances, and such rooms were now generally occupied or sub-let. He did not think the fallacy suggested by Dr. Clark Trotter was now material.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

The Administrative Control of Diphtheria.

By WM. ROBERTSON, M.D., D.P.H., F.R.C.P.Edin.

Medical Officer of Health for the City of Edinburgh.

UP till recent times our methods of controlling the spread of diphtheria were guided by the principle of locking the stable door after the horse had been stolen. Notification of the disease, removal to hospital, with subsequent disinfection of the house, clothing and bedding was the routine. When throat swabbing became more general, we felt that we had made a further advance in eliminating possible spreaders of the disease. Still further progress was expected by the administration of preventive doses of antitoxin among immediate contacts, though the practice never became very general in this country. Actual experience has always proved the elusiveness of the "carrier" in diphtheria. In fact it may be said, at the outset of this contribution, that "carriers" in a particular school proved so persistent that they compelled me to embark on a much more drastic method of control than that usually practised in this country. "Carrier" after "carrier" was isolated with irritating regularity where a severe type of diphtheria had, literally speaking, dug itself in. Fortunately the holiday season was not far distant at the period in question, so the infected school was disbanded. This arrested the spread of the outbreak and allayed the unrest that had seized the parents of the scholars. Taking advantage of the opportunity presented, a meeting of parents was held at the school that had been invaded. A surprisingly large audience gathered together, and the whole situation affecting the modern methods of controlling diphtheria was fully explained by Dr. Claude Ker, the physician-superintendent of the City Fever Hospital. The writer also took part in advocating new preventive methods by means of the Schick test. The subsequent protection of reactors was also fully explained. The experiences of Dr. Ker and Dr. McGarrity were sufficiently convincing to enable us to assure the audience of the safety of the procedure involved. It is needless to labour the details of our work, which called for a great deal of careful organization. Fortunately I had in Dr. Benson, my assistant, a most competent, skilled and careful helper, in whose hands the working out of details was confidently left. Dr. Benson has already published the results of his preliminary work among the children attending twenty-six of the Edinburgh and Leith schools. A reference to his contribution (see *Lancet*, November 8, 1924, p. 949) will convince the reader how well his task was performed. He was greatly assisted by the teaching staffs, whose co-operation is of the utmost importance if any such scheme is to be undertaken with any hope of success. It is of the greatest moment to be able to assure an education authority that interference with school routine will be reduced to the minimum. This Dr. Benson was able to do, because the marshalling and control of the scholars was so well carried out by the teachers. Before the campaign was begun we were satisfied that the work of Dr. Ker and Dr. McGarrity (see *Lancet*, 1924, i, 1101) at the City Hospital, among nurses, resident doctors, domestic servants, medical students and patients, left no doubt as to the reliability and innocuousness of the Schick test. Added to that we had the experiences of Dr. A. Zingher and Dr. W. H. Park among New York school-children to fortify us in our assurance that it was perfectly wise, safe and justifiable to proceed along the lines that had been followed in America.

As was to be expected, the determination to proceed was the signal for the anti-vaccinators and their followers to launch a press campaign against the principle of

inoculating "poisonous matters" into the arms of the children. Passionately-worded appeals were also made to the members of the local and education authorities. As far as the writer can ascertain the protests were of little avail, since the numbers of those who were presented for testing and immunization proved eminently satisfactory. Having gained the consent of the education authority to proceed with the use of the schools, a circular explanatory of the system it was proposed to follow was circulated among the general practitioners of the city. Subsequent to that, the leaflet, together with a consent slip, were given to each scholar under the age of 10 years. The consent slip craved the signature of the parent for the application of the Schick test.

The consent slip read as follows:—

I hereby give the Medical Officer of Health or his representatives permission to carry out all the following procedures should he consider them necessary for the protection of my children against diphtheria, namely: I. The Schick test. II. A course of three protective injections. III. A final Schick test and protective injection after an interval of six months.

The consents when returned were tabulated by the school staff, who added the age, name, address and previous diphtheria history of each child. This list when completed was transferred to a special book kept at the Public Health Office.

Suitable dates and hours having been arranged, the tests were carried out, the teachers again offering valuable assistance; one cleansing arms with spirit while another kept a close record of the children as they came forward in their long but orderly queues. A third teacher, when available, directed the tested children to their respective class-rooms. The following time-table, drawn out by Dr. Benson, will indicate how the work was carried on according to a well-defined time-table:—

Day, 1st. Schick test.
,, 8th. First reading and first toxin-antitoxin injection.
,, 15th. Second reading where necessary, and second toxin-antitoxin injection.
,, 22nd. Third toxin-antitoxin injection.
,, 29th. Final visit where necessary to complete the absentees.

Experience proved that between 100 to 120 children could be tested within an hour. The toxin-antitoxin injection can be performed with equal rapidity. Two skilled operators, working at each school for a period of two hours, were able to cover the ground quite comfortably. Day-books were filled in at the time of testing and injection. The particulars noted therein were then transferred to a card index system kept in the Public Health Office. Care is taken to compare the indexed cards every day with the cases of diphtheria notified to the department. Full particulars are also sent to the hospital when patients are sent there. The advantage of tracing "tested" children is obvious.

An interesting commentary on the percentages of consents secured is offered by an analysis of the schools. Among the better-class children the consents returned by parents through their children varied from 86 per cent. on the one hand to 40 per cent. from those drawn from the poorer-grade schools and populations. This feature is significant and suggests the need for an intensive campaign of education and propagandism in matters pertaining to preventive medicine. In some cases where only 50 per cent. of consents were returned, a further canvas, following on the successful and proved harmlessness of the Schick test, resulted in raising the percentage to 70. Fears of reactions and complications having been allayed, parental confidence became restored. The one disappointing feature of our campaign was that children of pre-school age were not submitted for treatment in the numbers expected, though a special invitation had been issued to bring children under 5 years of age for protective inoculation. Doubtless the injection of children of such tender years was dreaded. We must therefore make it known to parents that there is least danger of reaction and no need for applying the Schick test among those who are between the ages of 2 and 5 years.

The result of applying the Schick test at various age-periods differed slightly when undertaken at the Edinburgh Fever Hospital by Dr. Ker and Dr. McGarrity, and by Dr. Benson among Edinburgh school-children. The social factor will always be found to account for discrepancies that may be revealed by different observers, not only in this country but also abroad. The susceptibility is invariably highest among the best grades of the social plane, and lowest among the worst, as measured by environment. The slum child is subjected to repeated doses of diphtheria virus, and becomes more or less actively immunized, whereas the well-housed child is not exposed to the protection of infection. Among the Edinburgh school-children we had revealed to us 100 per cent. of susceptibles in one school between the ages of 1 and 5 ; 82·6 per cent. between 5 and 10 ; and 67 per cent. between 10 and 12. At none of the other schools under review did the percentage of susceptibles between 1 and 5 fall below 73 per cent., whereas between the periods of 5 and 10 the susceptibility varied from 88 per cent. to 34 per cent.

The part played by the factor of social status is illustrated by the instance of the staff of a children's hospital, where among the forty-three nurses tested between the ages of 18 and 25 the susceptibles numbered 58·1 per cent., whilst among eleven maids of the same age-periods the susceptibility was only 45 per cent. The entire medical and nursing staff, fifty-seven in number, showed a susceptibility of 49·1 per cent. Dr. O'Brien found 50·5 per cent. of susceptibles in the nursing staff of a London hospital.

The incidence of susceptibility in families had already been noticed and commented on by Dr. Zingher. Experience in Edinburgh verified the facts quoted from New York. Among 600 families tested every member in 143 instances gave negative Schick results. In 280 families every member was positive. In 126 cases the younger members gave a positive Schick result and the elders a negative one. In fifty-one instances the position was reversed between older and younger members. Thus we had 91·5 per cent. coming under the first three categories, and only 8·5 under the last.

That an attack of diphtheria does not confer a lasting immunity was borne out in America and verified in Edinburgh, when Dr. Benson found that 48 per cent. of those who gave a history of a previous diphtheria attack yielded positive Schick reactions. That immunization with toxin-antitoxin can be confidently undertaken was evidenced by the fact that 92·6 per cent. of the school-children who were given one injection came forward to receive the other two required. A percentage of 3·4 parents refused to permit the completion of immunization.

The re-testing of school children is not called for until six months have expired since the last of the three injections of toxin-antitoxin has been administered. In order to warn parents to be prepared for the re-test the following notice was issued :—

Re-test Notice.

By the application of the Schick test your child was found to be susceptible to diphtheria and duly received three protective injections. A sufficient interval of time has now elapsed to allow for the development of protection. Before we can state generally that the child is protected against diphtheria a final Schick test must be applied. This final test is now to be carried out.

In one group of children ninety-two between the ages of 5 and 15, or 91·3 per cent., developed immunity to diphtheria between five and six months of immunization. Among the other eight children a fourth immunization dose was given to seven because the re-test Schick had proved faintly positive, or moderately so. A re-test two months later yielded a negative Schick response in all seven. In another instance, out of a group of 155 children between 5 and 11 years a re-test after six months showed that only 42·6 per cent. had developed immunity. Of the eighty-nine who showed evidences of susceptibility 34·2 per cent. gave a faint Schick reaction. These were classified as borderland cases. Among thirty-six children, or 23·2 per cent.,

definite susceptibility still remained. At first sight the latter figures are disquieting, but Dr. Zingher has shown that immunity may be delayed in manifesting itself for twelve months, and that without administering additional protective injections.

With the foregoing experiences to guide us we now insert the proviso in the consent slips that a fourth protective dose of toxin-antitoxin may be found necessary.

Certificates have been issued on the lines suggested by Dr. Zingher and his fellow-workers. One deals with the result of the Schick test, a second certifies that protective injection has been administered and that protection against diphtheria has been proved by the Schick test. The third sets forth that three protective doses of toxin-antitoxin have been given, and that the final diphtheria protection certificate will be issued after an interval of six months, provided the child undergoes a Schick test and shows a negative Schick reaction.

The experiences of Dr. Zingher and Dr. Park, verified by our own modest effort in Edinburgh, enable one to offer some facts that may encourage an extended effort in favour of systematized immunization against diphtheria. We are now in a position to state that among children between the ages of 2 and 5 years the injection of toxin-antitoxin is a safe and harmless procedure, and can be carried out without prior application of the Schick test. Also, if we hope to secure the best results in the creation of immunity, we must concentrate on the children of pre-school ages. When the child reaches the school age it can be treated by means of the Schick intradermal injection. Among the school-children we confined our attentions to those under 10 years of age.

Much of the preliminary technique has been removed by the experiences gained. The fact that the Schick testing of children under five can be dispensed with rids us of a vast amount of clinical and clerical work.

When the Schick test is found necessary, those who are responsible for making correct readings must be possessed of previous experience, because it is of the utmost importance to place the reactions in their correct categories. Faulty readings will inevitably repercuss against us when those who have been labelled Schick-negative develop attacks of diphtheria. Such seldom happens, but cases have occurred, and when one has to admit that faulty sera or bad technique were to blame, the position of the administrative officer is made awkward. When the general practitioner can be awakened from his lethargy and can be tempted to take a keener interest in preventive medicine, he need have no dread of Schick test readings. If he does his duty by the State, and desires to secure a bulwark against diphtheria among his patients, he will make it a point to administer toxin-antitoxin to children under 5 years as a routine procedure. It is just as necessary a step as vaccination has become. As has been said, subsequent Schick testing can be left until the child reaches the school age, when large numbers may be overtaken by specially-trained medical men and women.

For a full study of the subject of protection against diphtheria I cannot do better than refer you to the valuable reports that have been issued from time to time by Dr. Abraham Zingher, of the New York Department of Health. The reliability of the Schick test is now accepted. Over 3,000 children who had given negative Schick results were re-tested by the same observer at different periods extending over three years, and in every case a negative reaction persisted. That and other experiences led Dr. Zingher to reach the conclusion that the development of natural immunity in children between the ages of $1\frac{1}{2}$ to 2 years, as indicated by the Schick test, pointed to a permanent immunity. Between the ages of 6 and 9 months natural immunity is derived from the maternal placenta and is therefore only temporary. The like applies to breast-fed infants, whose supply of antitoxin antibodies is derived from "breast" milk. Infants whose mothers possess no immunity of their own are susceptible to diphtheria at birth, while those whose mothers are immune become susceptible from six to nine months after birth. An occasional child

may retain its maternal immunity for twelve months. Doubtless this feature of maternal immunity explains why breast-fed infants are seldom attacked by scarlet fever. The character of the immunity produced by the injection of toxin-antitoxin is now believed to be a combination of the immunity produced by other forms of vaccination with the earlier development of a natural immunity that would have occurred in the natural course of events in the later years of life. An active immunity is developed, and, to quote Dr. Zingher:—

"The tissue-cells of the individual are so sensitized that under subsequent conditions of exposure to the ubiquitous diphtheria bacillus they readily respond by producing a fairly large supply of antitoxin. This remains more or less constant in some, is increased in others, and is diminished slightly in others."

Dr. Zingher's second report, issued in January, 1921, deals with the "Practical Application and Uses of the Schick Test." Here Dr. Zingher discusses, among other points, the factors influencing the reliability of the Schick test. These are three:—

(1) The toxin used for the test must be of standard strength; (2) the technique in making the intradermal injections must be correct; (3) the interpretation of the reaction must be accurate.

Schick laid it down that the amount of toxin used for his test should be $\frac{1}{50}$ of a minimum lethal dose for the guinea-pig, in 0·1 c.c. of normal saline. Zingher prefers $\frac{1}{50}$ in 0·2 c.c. of saline, because the amount is more easily handled and will, after injection, show a small wheal-like swelling in the skin. The weaker dilution gives as good results as the more concentrated mixture.

The technique is of great importance. Being a local one the reaction must be seen by the naked eye. Therefore the test-fluid must be injected intradermally and not subcutaneously. To make the test, 0·2 c.c. of unheated and properly diluted toxin must be injected on the flexor surface of the forearm, 2·5 in. below the bend of the elbow, and a similar amount of heated toxin on the left forearm. To maintain a systematic plan the right arm should always be used for the test and the left for the control. In susceptible persons a definite, well-circumscribed area of redness will appear at the site of the injection within twenty-four to thirty-six hours. This redness, which at first is about the size of a sixpence, becomes more distinct within the ensuing two or three days, the reaction being at its height on the fourth or fifth day. The redness persists for a week; then it begins to fade away, leaving a brownish area of pigmentation behind it. There may be signs of definite scaling of the skin over the site of injection. The pigmentation may persist in some individuals for a period of three to four months. In older children, and especially among adults, pseudo-negative reactions may occur. These confuse the readings and they must not be confounded with positive reactors. According to Zingher, pseudo-negative reactions indicate immunity. This form of reaction is believed to be produced by the autolysed protein of the diphtheria bacillus, which is also present in the test fluid. It is of the nature of an anaphylactic reaction. The pseudo-negative appears sooner than the positive reaction and is distinct at the end of eighteen hours.

It was in 1913 that von Behring suggested a new method for actively immunizing with diphtheria toxin-antitoxin. The full details of this mixture were not fully divulged at the time. Dr. Zingher and Dr. Park, after full investigation, came to the conclusion that two standards for these mixtures must be established: (1) a standard of safety, and (2) a standard of efficiency. The standard of safety was complied with when 5 c.c. of a mixture of toxin-antitoxin injected into a guinea-pig produced local induration and late paralysis in the animal, but not acute death from diphtheria.

It was found that the standard of efficiency was more difficult to establish.

In this country we are fortunate in having a worker highly skilled in the preparation of toxin and antitoxin mixture, namely, Dr. O'Brien, of the Wellcome Research Laboratories; all our sera for carrying out the Schick test and the protection of school-children in Edinburgh have been secured from that institution.

As is now generally known, toxin-antitoxin in 1 c.c. doses is injected thrice at weekly intervals. Dr. Zingher and Dr. Park tried to reduce the injections to two in number, in the hope that the work would be simplified. Experience proved that two doses were not so effective as three. Dr. Zingher suggests: "There may also be a better antitoxin response when the injections are given two weeks apart."

Before the development of active immunity can be judged, a period of six months must elapse before the Schick re-test is made. If the re-test is positive, a second series of two or even three injections of toxin-antitoxin may be given. There are instances where, even after a second series of toxin-antitoxin injections had been administered, failure to develop immunity was demonstrated.

There is no danger of anaphylaxis, either in repeating injections or in giving toxin-antitoxin.

As a result of their experiences in New York among 150,000 children in public and parochial schools, Dr. Abraham Zingher and Dr. Park felt justified in stating that active immunization against diphtheria should become as much a routine for all children between 6 months and 6 years as vaccination is against small-pox to-day. I might further hazard the opinion that when medical officers of health and local authorities in this country appreciate the safety of the procedure of protection they will not hesitate to advocate its adoption. Still further, when the huge costs incurred in isolating and treating diphtheria cases in hospital are totalled, the adoption of a system of paying medical men to administer toxin-antitoxin will have to be seriously considered. Each case of diphtheria treated in hospital costs a local authority between £15 and £20. If the injections and fee cost even 10s. for each child protected, the saving would soon become evident. The saving to the rates effected by reducing the stay of scarlet fever patients in hospital from seven to five weeks becomes appreciable when the yearly budget of expenditure is examined. One can well imagine what a large reduction could be brought about even if the numbers of diphtheria admissions could be reduced by 50 per cent. The clinician does not favour the discussion of infectious diseases from the standpoint of pounds, shillings and pence. But the administrative officer must face facts, and the facts in this case are indisputable.

It is a weak argument to offer, that the people of this country are not prepared to accept the principle of immunization against diphtheria. The man in the street is not devoid of intelligence. If he is once persuaded that the case is a good one, he will support it. That, at least, has always been my personal experience. To affirm that apathy exists on the part of the parents is begging the question. One must look for apathy in other directions. Where there is a will there is a way.

Discussion.—Sir WILLIAM HAMER said he desired to call attention to the New York official figures as to cases and deaths (diphtheria), which were now available for the first eleven months of 1924, so that comparison could be made between them and the corresponding figures for 1923. The cases showed increase from 7,232 to 8,741, and the deaths increase from 506 to 636. Diphtheria had been declining in New York since 1918 until this rise occurred last year. This phenomenon following upon four years of the new inoculation procedure was disquieting.

Dr. R. A. O'BRIEN expressed his admiration of the work being carried out in Edinburgh. He said that the efforts of those preparing diphtheria prophylactic were directed to increasing the efficiency of the immunization. They constantly hoped to reduce the three injections

to two and finally to one, but the obstacle which had not yet been overcome was, that as the antigenic efficiency was increased so the liability of the material to cause non-specific reactions, i.e., local redness, temperature and vomiting for twenty-four or forty-eight hours, increased correspondingly. He said he was optimistic enough to hope that some day we should reach the stage at which one injection of combined diphtheria prophylactic and Dick scarlet fever toxin would be sufficient to produce satisfactory immunity against both diphtheria and scarlet fever.

Dr. S. MONCKTON COPEMAN, F.R.S., said that during a recent official visit to Edinburgh he had had an opportunity of investigating the work which formed the subject matter of Dr. Robertson's paper; and he was glad therefore now to be able to congratulate him, and, through him, Dr. Benson, on the admirable manner in which this pioneer work was being carried out in co-operation with the various school authorities. Under the auspices of the Ministry of Health, during the past few years some thousands of children in Poor-Law institutions in London and the provinces had been Schick-tested and when necessary immunized, but, up to the present, similar work had not been carried out in public elementary schools, or in child welfare centres, except to a limited extent in the Borough of Holborn. A few higher grade preparatory and other schools had, however, been visited for the purpose of Schick-testing and immunizing the scholars, and it was in these schools that the highest percentage of positive reactors to the Schick test had usually been met with. This condition however appeared to be not entirely, as usually stated, a matter of social status of the children examined, but to be dependent on the extent to which there had been opportunity of more or less continuous exposure to risk of contact with the disease, since under these conditions in a secondary school recently visited in Norfolk, for instance, the proportion of negative reactors had been found to be somewhat exceptionally high. Dr. Copeman added that when in New York a few months ago, he found that practically all the children of pre-school age brought to various child-welfare centres were receiving immunizing inoculations against both diphtheria and scarlet fever as a matter of routine.

Sir GEORGE BUCHANAN emphasized the value of independent local initiation in epidemiological work, such as had been illustrated by Dr. Robertson. Practical local testing of methods of prevention, other than those sanctioned by tradition, was of the greatest importance to all, particularly when it was carried out with the thoroughness and courageousness which had evidently characterized the undertaking in Edinburgh. In this instance the experience already gained in the United States, and the good fortune of our being able to obtain reliable toxin and toxin-antitoxin in the way other speakers had indicated, fully justified the action taken and allowed good hopes to be entertained of its effects when in due course they could be satisfactorily appraised. Reference had been made to the recent rise in the diphtheria curve in New York; if, as he believed, the New York system involved an accurate register of the reactions and active immunizations of the children, we might soon expect something like a demonstration from this circumstance, as the recent cases could be analysed into "protected" and "unprotected" in proportion to the numbers of the two classes in the New York population. He would be glad to know the Edinburgh experience of febrile reactions and transient constitutional disturbances after active immunization, and whether it was thought desirable to give parents any warning that they might occur.

Dr. MAJOR GREENWOOD said he thought that Dr. Robertson had been wise in refraining from bringing forward statistics of the results of his campaign at the present time. Such statistics could only be incomplete, and much harm was done by the use, for propaganda, of imperfect statistics. Such an analysis of New York experience, as Sir George Buchanan had urged, might indeed be valuable, but, until these data were made available, the method should be advocated on the ground of its inherent reasonableness, not on the ground that its practical value had been statistically demonstrated.

Dr. G. CLARK TROTTER said that while he could not state any personal practical experience of the actual Schick testing and immunization, he thought he could throw some light on the attitude of parents and guardians in Scotland in regard to this matter, which differed from what had to be faced in London. The Public Health Authorities in Scotland had this decided advantage: in the Public Health (Scotland) Act infectious disease was not defined, therefore at law the powers were the same regarding an infectious disease whether it was compulsorily notifiable or not. The only reason that in certain cases more stringent action was not taken was that advance could only be feasible in deference to public opinion. The fact of the possession of these powers gave the public health services in Scotland considerable advantage,

and the people were accustomed to be thus in closer contact with the health services. Now, for example, in London the path of the health officer was not so smooth. In his opinion this was due to the multitude of officials who made visits for various purposes—including rate collectors, gas company inspectors, and others. The result was that the sanitary inspector, epidemic inquiry official, or health visitor, had to be somewhat persuasive, and even then there was a difficulty in gaining admission or obtaining information. The attitude of the people was such that they were up against considerable impediment. In Scotland this attitude was rare.

As regarded the Schick testing, he entirely agreed that it was a valuable means of finding out those who were susceptible.

With respect to the immunization, he felt that with the toxin-antitoxin they were using an unstable product, and were entirely in the hands of the firm that produced it so far as concerned the guarantee that it was in good condition and reliable. Cases had been recorded in respect of which it had been stated that the stability of the immunizing material could be adversely affected. On the other hand, it was the only method at present available; but it was a toxin-antitoxin, not a true vaccine against the bacillus. There had been as yet no satisfactory "vaccine" produced.

Some considerable time ago, with a view to making the test available for any parents who might apply, he had gone into the matter with the Medical Officer of Health of Holborn, under whom Schick testing and immunization had been carried out to a limited extent. He understood from Dr. Hutt that it was very sparsely taken advantage of. It was difficult to get any considerable number of parents to bring their children at weekly intervals for at least three or four weeks, and relatively few had taken advantage of the process. In the circumstances all that had been done in Islington was tentatively to obtain permission to a limited extent to have the process carried out should any applications be received.

The progress made in Edinburgh in introducing the preventive control of diphtheria was an outstanding event.

Fleet-Surgeon W. E. HOME, in order to relieve the anxieties of some Members of the Section living in the south, submitted that the Medical Officer of Health of Edinburgh, in his introduction of a new and thoroughly scientific preventive method, was in a much stronger position than a medical officer of health elsewhere. The Medical Faculty was the largest in the University of Edinburgh, the University a proportionately larger element in the life of a larger City than was any other University in the kingdom. Dr. Robertson had therefore had behind him more educated opinion, and before him less merely ignorant opposition than anyone would have found elsewhere. Dr. Robertson's paper was most interesting, but, like other Members who had spoken, he (the speaker) would have been glad to have heard the record of histories showing that immunizations had been followed, in fact, by protection against diphtheria.

Dr. GRAHAM FORBES said that the valuable pioneer preventive work which Dr. Robertson and Dr. Benson were carrying out in the Edinburgh schools was the first instance in Great Britain of the recognition and adoption by the education authorities of these anti-diphtheria measures. He said that during the past year he had had occasion to make inquiries as to the extent to which the use of the Schick test and immunization had developed in Great Britain. Information, very kindly forthcoming from the medical officers of health in London and twenty of the largest towns, showed that this work was mainly confined to fever hospitals and certain residential institutions, such as homes and Poor-Law schools and some private schools. From published records and the information in response to his inquiries, he estimated that in Great Britain some 20,000 individuals had been Schick tested and only about 4,000 immunized—a very small number compared with the half-million of children in New York alone who had been Schick tested and the one-third of the child population which had been immunized. In London, action by any authoritative body was at present confined to the Metropolitan Asylums Board, at whose hospitals Schick testing and immunization of the medical and nursing staff had only recently been instituted. In one only of the London boroughs—Holborn—had any definite provision so far been made to supply the public with facilities for obtaining immunization against diphtheria, namely, at the Maternity and Child Welfare Centre in Theobalds Road, where, as Dr. Hutt had informed him, during two and a half years up to the end of 1924, 628 individuals had been Schick tested and 351 immunized.

In some of the residential institutions, e.g., the Poor-Law Schools of Holborn and Lambeth

and Dr. Barnardo's Homes, a considerable amount of work had been done by the Wellcome Research Laboratories, under the control of Dr. O'Brien and his colleagues, in providing a total of over 4,300 Schick tested and close upon 1,200 immunized. The results appeared to have been successful in view of the absence of further outbreaks of diphtheria. In the provinces, at Birmingham and Manchester, and to a less extent at Newcastle-upon-Tyne, Bristol and Brighton, preventive measures had been adopted in the City Fever Hospitals, and directed particularly to the staffs, with good results in the reduction of cases of diphtheria among nurses exposed to infection. The papers published by Dr. Harries and Dr. Dickinson on the Schick test as applied to patients in their hospitals were well known. In no instance, however, had the education authorities of the twenty largest towns in England and Wales taken any action up to the middle of the year 1924. The replies received from the medical officers of Edinburgh, Glasgow and Aberdeen showed that the preventive control of diphtheria in Scotland had reached a considerably more advanced stage than in England. In Edinburgh, as they had just heard, the work of Dr. Claude Ker and Dr. McGarrity in the City Fever Hospital had led Dr. Robertson and Dr. Benson in December, 1923, to obtain the consent of the Education Committee to the application of the Schick test and immunization in the schools, where the results would be awaited with much interest. In Aberdeen County Dr. J. P. Watt had started a clinic at Fraserburgh in June, 1924, and since then, up to last November, well over 2,000 children of pre-school and school age had been either tested or immunized, and parental consent had been given in over 80 per cent. of cases. In Aberdeen City clinics for preventive measures had been instituted and the work was progressing there as well as at the City Fever Hospital. In Glasgow the reply from Dr. Chalmers showed that the steps so far taken were confined to the Fever Hospital and residential institutions, and that no action had yet been taken by the authorities as regards the schools and general population. It was obvious that London presented a very difficult problem so far as the school population was concerned, and he felt that it would be long before preventive measures were likely to be adopted on any large scale. The Maternity and Child Welfare Centres provided the most hopeful means of introduction in the way of persuading the mothers of the value of a method which promised protection to their children against a disease responsible for over 45,000 cases and over 3,700 deaths in those of pre-school and school age in London alone in the years 1920-1923. The undertaking of educational propaganda of school nurses, teachers and all concerned in child welfare work was also very necessary before any real progress could be made.

Dr. ROBERTSON (in reply): Sir George Buchanan and Dr. Greenwood have very properly referred to the importance of reassuring data that must necessarily be held over in support of the value of immunization. I might have presented some figures to you based on our own experiences, but these obviously would not have been offerings of convincing or scientific proof. Several years must necessarily elapse before we can support with facts the methods we have been discussing. Statistical evidence is always open to criticism, especially when the data are confined to small figures and short periods of time.

Speaking of intelligence and with special reference to Dr. Monckton Copeman's generous praise of our Edinburgh efforts, it occurs to me that you in England have displayed far greater intelligence and foresight in the co-ordination of your public health and school medical services than we have shown in Scotland in this respect.

The school medical officers in Edinburgh simply folded their arms and looked on at our testing and immunizing work, thereby adopting an attitude of what might be termed passive assistance. They were with us in spirit, but it "was not their job." We were compelled to denude our hospitals of their residents for the time being to enable us to carry on our work. And all this because the school and public health medical services in Scotland are things apart. I take leave to say that the work of prevention would have been of much more importance to the child than some of the routine inspections to which it is often subjected.

Reactions were few, and when they did occur were mainly of the local variety, though now and again a temperature did cause parental alarm. It was a very significant feature that among the better grade scholars any reactions that occurred were minimized rather than exaggerated, whereas among the poorer classes temporary sickness or feverishness was made the pretext for abstention from school. We avoided reactions by confining our activities to children under 10 years of age. It is in institutions, among nurses and adults, that reactions are to be expected.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

Immunization against Scarlet Fever.

By S. MONCKTON COPEMAN, F.R.S., M.D., F.R.C.P.

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IT is matter of common knowledge that scarlet fever and diphtheria exhibit certain inter-relations, the precise importance of which, however, is, at present, still somewhat imperfectly understood. On this account it is perhaps not inappropriate therefore that papers dealing with questions of susceptibility to, and with immunization against, diphtheria, which have been read before the Section during the past two or three years, should be supplemented by a brief account of the present state of our knowledge as to the value of recent investigations on like questions in connexion with scarlet fever.

In this country scarlet fever is fortunately nowadays a much less serious disease, *quâ* mortality, than was the case a generation ago, but in America, where the work that I mainly have to discuss originated, this disease, at present, ranks as the fifth most fatal ailment in the age group from 4 to 9 years, 74 per cent. of the deaths from scarlet fever occurring in children before they have reached their tenth birthday. In view, however, of the dangerous sequelæ which not infrequently occur in this country even after comparatively mild attacks of the disease, the possibility of producing artificially, as in the case of diphtheria, a condition of immunity to the disease, and preferably in the pre-school period, will be generally recognized as a matter of the utmost importance.

During a recent official visit to New York I was afforded opportunity of investigating work in progress on the bacteriology of scarlet fever, on methods of immunization, and on the specific treatment of individuals who have already contracted the disease. Work in these directions is now being carried out at the Willard Parker Hospital on a considerable scale by Dr. Abraham Zingher, Assistant Director of the Public Health Department, and his colleagues.

This work is largely based on what is known as the "Dick test," so called from its introducers, Drs. G. F. Dick and Gladys H. Dick, of the McCormick Institute for Infectious Diseases in Chicago—a test which exhibits many analogies to the "Schick test" for diphtheria. The original work of these observers has, however, now been considerably extended as the result of the carefully designed researches of other investigators in this country and abroad.

BACTERIOLOGY.

In a series of papers the Dicks have set out the results of their study of the bacteriology of scarlet fever, in the course of which they apparently confirm the earlier results of Klein, Tunnicliff, Bliss, and Mervyn Gordon, that a hæmolytic streptococcus, long recognized as a micro-organism constantly present in the naso-pharyngeal cavities of patients with scarlet fever, constitutes the probable ætiological agent of the disease.

But considerable difficulty has been experienced by all who have worked at the subject, owing to the fact that it is practically impossible, by the inoculation of guinea-pigs, rabbits, mice or other laboratory animals, to produce a disease bearing any definite resemblance to scarlet fever; consequently experimental work on the subject has necessarily had to be carried out on human volunteers.

Moreover, examination of blood-cultures failed to reveal any organism present constantly enough to indicate a causal relation, and the results of subcutaneous inoculation of volunteers with fresh blood-serum and fresh whole blood from early cases of scarlet fever have been completely negative. This was also the case in attempts by the Dicks at naso-pharyngeal or subcutaneous inoculation of volunteers, with filtered throat mucus obtained from early cases of scarlet fever. As, however, these experiments might possibly have been invalidated, owing to the individuals inoculated having previously suffered from an attack of scarlet fever, a further series of volunteers was obtained who had lived practically all their lives in the country, and concerning whom no history of attack by scarlet fever could be obtained. In this second series of human inoculations a typical case of scarlet fever is said to have been produced by swabbing on the tonsils and pharynx a pure culture of a hæmolytic streptococcus isolated from a lesion on the finger of a nurse who acquired the disease while in attendance on scarlet-fever patients.

As, however, recent opinion has tended to the suggestion that a filterable virus might be concerned in the production of the disease, another group of volunteers was inoculated with the same culture after it had been passed through a Berkefeld V filter. These all remained well. After an interval of two weeks they were once more inoculated, this time with the unfiltered culture; forty-eight hours later one of them developed scarlet fever. As bearing on this point it may be mentioned that I learnt from Dr. Krumwiede, bacteriologist to the Public Health Bureau, that one of his assistants at the Research Laboratory recently contracted scarlet fever following on the accidental swallowing of a culture of a special strain of a hæmolytic streptococcus with which he was working at the time. From cultures of this streptococcus, which is apparently identical with that of Dochez in its special properties, the Dicks obtained their "toxin," in the first instance, from the condensation fluid of cultures of the organism on blood-agar slants, the fluid being subsequently passed through a Berkefeld filter in order to obtain it free from organisms. This, however, was not a practical method for producing the toxin on a large scale. But Dr. Anna Williams, Dr. Huntoon and other observers subsequently showed that the toxin could be readily produced by cultivation of the streptococcus in blood-broth. A large dose of this particular toxin is said to have produced some of the main features of the natural disease, including the rash.

The Dick Test.

This toxin, suitably diluted to the extent of about 1 in 5,000, is now used for the purpose of determination as to the susceptibility of individuals to scarlet fever or the reverse; the test fluid is inoculated intradermally in precisely the same manner as the solution of diphtheria toxin in performance of the Schick test. The results following on the use of this test are also very similar to those observed in the case of the Schick test, except that the reactions appear more rapidly, are at their maximum intensity after an interval of about twenty-four hours, and subsequently fade more quickly. I think it will be generally admitted that accurate determination of the readings is apt to be somewhat more difficult of accomplishment than are those resulting from the Schick test. Dr. Zingher, who had kindly inoculated a number of children on the day previous to my visit to the Willard Parker Hospital, in order that I might see the results obtained, laid great stress on the fact that pseudo-reactions are even more frequent than in the case of the Schick test, and that consequently a control inoculation on the opposite arm is of very special importance. The control test is carried out with a solution of the toxin which has been heated in a water bath to 100° C. for an hour, the toxin being less easily destroyed than in the case of the Schick material. A similar dose (0.2 c.c.) of the toxin is employed as in the Schick test, and the results following on this use will be either (1) positive, (2) positive and pseudo-, (3) negative, (4) negative and

pseudo-, the reddened area, representing the positive result, being of about the same extent as is observed in a positive Schick test.

As bearing further on the specific effect of the toxin, it is found that when a patient who has been inoculated intradermally subsequently contracts scarlet fever, the area occupied by the preceding positive reaction is marked as a pale area surrounded by the scarlet fever rash, which is specially intense in a narrow area immediately surrounding that previously occupied by the positive reaction. This effect of local pallor is apparently due to a certain amount of cellular immunity having developed at the site of the original positive Dick test; while the ring of intensified rash, making up the border which surrounds the pale area, is probably due to the interaction between cells which were sensitized, rather than protected, by the minute antibody content within their substance and the specific scarlet-fever toxin.

The effect of the toxin can be completely neutralized by mixing with it blood-serum obtained from a convalescent scarlet fever patient, or from the serum of a horse which has been immunized with the toxin; furthermore, if serum from either of these sources be injected intradermally into the skin of a patient suffering from an early scarlet fever rash, a blanching of the rash over the area of the injection is produced subsequently in the course of six to eight hours. This phenomenon is known as the Schultz-Charlton reaction.

Information afforded by the Dick Test.—All patients coming into the Willard Parker Hospital, whether supposed to be suffering from scarlet fever or not, are now as a regular matter of routine inoculated intradermally with streptococcus toxin. Of the cases which definitely turn out to be scarlet fever, it is found that in the very early stage a positive Dick test may be obtained, whereas, if the test be repeated about ten days later, the result is usually negative. In a certain number of cases admitted on account of mistaken diagnosis a still more marked positive result may be obtained, but again if, as occasionally happens, the patient contracts scarlet fever after a stay of some days in hospital, a negative test will be obtained after a corresponding interval of time.

As regards the reaction likely to be obtained following on the Dick test among children not suffering at the time from scarlet fever, results very similar to those well known in connexion with the Schick test have been forthcoming. Thus, in the earliest stage of life, a child apparently acquires fleeting placental immunity from the mother, so that up to the age of three months at any rate the Dick test is likely to afford a negative result. At subsequent ages the relative proportion of positive and negative results, as in the case of the Schick test, will vary according to age, to social status, and as to whether the child is town- or country-bred. In the case of many hundreds of children from congested slum areas in New York, Dr. Zingher finds that the proportion of positive Dick tests is about 32 per cent., whereas in children from better-class areas, or from country districts, the proportion of positive reactions may be as high as 88 per cent.

Treatment of Scarlet Fever.

Like the Dicks and certain other subsequent observers, Dr. Zingher finds that the course of the disease in those who have already contracted scarlet fever can be modified and curtailed by the inoculation of blood-serum from a case convalescent from the disease, or by the simpler procedure of using for this purpose citrated whole blood to the extent of double the amount necessary in the case of convalescent serum. Arrangements have recently been made, however, for the immunization of horses with streptococcus toxin, for the antitoxic serum obtained from streptococcus toxin has been found to act as well as, or better than, convalescent serum, and will have the advantage of being much more readily obtainable.

IMMUNIZATION.

Some thousands of children in hospitals or at infant welfare centres in New York, who have been found to give a positive reaction to the Dick test, have now been inoculated intramuscularly with scarlatinal hæmolytic streptococcus toxin. Again, as in the case of the Schick test, these inoculations have been three in number at weekly intervals. These, in the majority of instances, have not given rise to any serious reaction locally or constitutionally. The amount employed for the purpose of these immunizing inoculations is based on the quantity of toxin used for the Dick test, children under 12 being injected with 100, 250 and 250 skin-test doses, while for persons over 12 years, 100, 250 and 500 skin-test doses have been employed; the toxin is so diluted that each cubic centimetre represents 500 skin-test doses, the initial dose therefore being 0·2 c.c.

In a small proportion of the inoculated persons constitutional symptoms, including a scarlatiniform rash, slight sore throat and elevation of temperature, have been observed, these symptoms disappearing, however, in from thirty-six to forty-eight hours. In order to avoid, as far as possible, the occurrence of such reactions, Dr. Zingher and his colleagues are at present investigating the effects of using a toxin purified by treatment with formaldehyde and subsequent incubation, according to the method worked out by Ramon in France and by Glenny and Hopkins in this country, for the production of a "toxoid" from diphtheria toxin.

Again, as in the case of the Schick work, it is important that the results of these inoculations, *quâ* immunization, should be determined by a further Dick test after an interval of three or four months from the last inoculation. Of 274 children recently re-tested in three institutions in New York, at an interval of from *four to five weeks only* after the last toxin injection, 167, or 61 per cent., were proved in this manner to have become immune. Dr. Zingher further informed me that no child giving a negative Dick test on admission to hospital (the Willard Parker) has, thus far, contracted scarlet fever, even though bedded in the scarlet fever wards.

The results obtained in America thus briefly summarized have naturally attracted attention in this and other countries. But, up to the present, the amount of work carried out with the object of obtaining confirmation of the investigations of the Dicks, of Dochez, and of other American observers, though energetically pursued, has hardly been extensive enough for the various experimental procedures to be established on a firm basis of scientific fact.

To the initiative of Dr. O'Brien and his colleagues, in collaboration with Dr. Caiger and his staff at the South-Western Hospital, we owe much of the bacteriological and clinical work that, as yet, has been accomplished in this country—the earlier results of which have been set out by Dr. Okell and Dr. Baxter in a short paper recently published in the Proceedings of the Pathological Society of Great Britain and Ireland.[1] These observers record the fact that they found hæmolytic streptococci almost always present in the throat in cases of scarlet fever. Of streptococci isolated by them, seven strains were examined in detail, all of which afforded sugar-reactions identical with those obtained from a couple of different strains of American origin. From these latter toxins were prepared, while a still more potent toxin was obtained from a strain which they themselves isolated from a case of scarlet fever. The organisms, actively hæmolytic in each instance, were found to grow profusely and rapidly in trypsin broth made from horseflesh.

Dr. Okell and Dr. Baxter set out in tabular form the results of their experience with the Dick test carried out with an appropriate dilution of this toxin on sixty individuals, the majority either suffering from, or convalescing from, scarlet fever. Of these, three medical men and one nursing sister, each with long experience of

[1] *Journal of Pathology and Bacteriology*, 1924, xxvii, pp. 342-345.

work in scarlet-fever wards, all gave negative reactions. They add that the general results of their work, in agreement with American experience, suggest that patients in the initial stages of scarlet fever are positive reactors, and that towards the end of the second week the majority become negative reactors, indicating immunity to the toxin.

A few months ago the Medical Officers Committee on scarlet fever appointed by the Ministry of Health came to the conclusion that it was desirable that further intensive investigation into the ætiological relationship of certain hæmolytic streptococci to this disease, and the diagnostic value of the Dick test, should be undertaken under the co-ordinated direction of a team of expert pathologists and clinicians. It is naturally hoped that the resources of the Metropolitan Asylums Board will also be available for this purpose as soon as the new arrangements in the Pathological Department of the Board are completed.

Pending this investigation, however, and with a view to some immediate progress being made, the Committee decided to adopt a scheme, the outlines of which were submitted by Dr. Eastwood and Dr. Griffith of the Ministry's Pathological Laboratory. They also agreed to devote the remainder of a small financial grant, which happened to be available, to the necessary expenses of some preliminary amount of research work in other centres, to be co-ordinated with that undertaken under the direct auspices of the Ministry.

To this end meetings were arranged at Liverpool and Manchester in conference with, in each instance, the Medical Officer of Health, the Professor of Pathology in the University, and the Medical Superintendent of the Infectious Diseases Hospital, all of whom expressed their willingness to co-operate with the Committee in furtherance of the scheme. It was also arranged that the preliminary work contemplated should not extend, in the first instance, beyond the end of March, 1925, when an interim report was to be submitted.

A further meeting of all the research workers concerned was held shortly afterwards at the Ministry's Pathological Laboratory in order to discuss necessary arrangements for co-ordination of the work. At this meeting Dr. O'Brien and Dr. Okell were also present by invitation, and afforded valuable assistance in discussion of the various details of the proposed investigation.

With the object of ensuring as far as possible that the work carried out in the different laboratories should be on strictly comparable lines it was decided that similar methods and, as far as possible, identical materials should be employed by all the different workers concerned; Dr. O'Brien for instance most kindly undertook to supply to each laboratory such amount of horse-blood as might be required.

The scheme ultimately adopted has comprised, on the clinical side, performance of Dick and Schick tests as a routine, on all patients notified as suffering from scarlet fever admitted to the Fazakerley Hospital at Liverpool, and the Monsall Hospital at Manchester, together, if possible, with a further Dick test preceding their discharge.

On the pathological side, throat discharges of scarlet-fever patients admitted to these hospitals have been examined for the presence of hæmolytic streptococci; while, for purposes of comparison and control, strains of these organisms of non-scarlatinal origin have also been isolated from various sources, such as non-scarlatinal throats, cases of general septicæmia, of puerperal fever and of septic wounds.

At the suggestion of the Committee I also wrote to Dr. Zingher requesting that he and Dr. Anna Williams would kindly send us small quantities of their antigenic serum, and of specimen strains of hæmolytic streptococci of scarlet-fever origin, for purposes of comparative study. These they generously promised to supply as soon as possible, and about a month ago I received, from the Bureau of Laboratories of

the Public Health Department of New York, a package containing cultures of Dr. Williams' type-strain of scarlet-fever hæmolytic streptococcus No. 55 (4), and of another strain from the same group, No. 45 (2). In addition there were enclosed six ampoules of undiluted toxic filtrate, the skin doses of which, just before dispatch, had been determined to be 0·1 c.c. of a 1 in 2,000 dilution. These were all immediately handed over to Dr. Griffiths for comparison with material available in the Ministry's Pathological Laboratory, derived, in part, from other laboratories associated with the Ministry's investigation previously referred to.

In an accompanying letter, Dr. Williams expressed regret at her inability to include any agglutinating serum, for the reason, as she states, that so many tests are being made in their own laboratories that it is being used up as fast as it can be produced. From this statement it would appear that she and her fellow workers have met with difficulties in the production of agglutinating serum comparable with those which have been generally experienced by investigators in this country.

As illustrative of interesting and unexpected observations in connexion with the standardization of their toxic filtrates, a subject which Dr. Williams informs me is being studied minutely by Dr. Park, Dr. Schroder and herself, I may quote from her letter as follows:—

"If we standardize a new preparation of a toxic filtrate by injecting a case or two with it, and at the same time with a Dick toxin the skin dose of which has been *determined*, we might get a very wrong idea of its strength; for if we inject a large series of cases with the estimated skin dose of the new preparation and, in the same case, with a skin dose of one-fifth the strength of the estimated dose we find that the weaker dose also gives a strong reaction in quite a large percentage of the cases.

"Another observation is, that after heating the toxic filtrate in boiling water for 1½ hours, it may still give in quite a number of cases a skin reaction, but that this reaction ('pseudo-reaction?') in the majority of cases is fully neutralized by convalescent scarlet-fever serum."

Progress reports have now been received from all who have been working in connexion with the Ministry's scheme. At a later period an account of the work that has been accomplished will doubtless undergo official publication, but I am permitted on the present occasion to make reference to one or two points of interest that have emerged. On the bacteriological side, in view of the brief period of time that has been available, the investigation has necessarily been concerned largely in a clearing of the ground, and in the evolution of practical working methods, so that, in any case, at present, it would be premature to express any opinion as to the ultimate outcome of the work.

In accord with experience in New York, considerable difficulty has been encountered in the production of agglutinating serum, partly for the reason that at first it was found to be impossible to produce a satisfactory emulsion from cultures of any of the numerous strains of hæmolytic streptococci isolated from cases of scarlet fever. Methods have, however, now been devised by means of which it would appear that this difficulty may eventually be overcome. In this respect it would almost seem that the Dicks and Dochez must have experienced, at an early stage in their investigations, exceptionally good fortune, in that they were successful in isolating strains which, as regards production of a satisfactory emulsion have afforded results unsurpassed, so far as I am aware, by those of any subsequent investigator.

On the clinical side, the Dick test has been studied on a fairly large scale by Dr. Rundle and Dr. Hodgson, at the Fazakerley Hospital, Liverpool, and by Dr. Sutherland, at the Monsall Hospital, Manchester, test solutions, supplied in each instance by Dr. O'Brien, being used. Owing to the fact that the greater part of this work was carried out on patients admitted to these institutions for scarlet fever, in practically all of whom doubtless the characteristic eruption had already developed, exceptional difficulty in certain instances, at any rate, is likely to have been

encountered in accurate reading of the test reactions. It is to this difficulty that Dr. Sutherland, who has supplied an analysis of 420 Dick reactions in 127 cases of scarlet fever, attributes the low percentage of positive results obtained by him on the second day of the disease. An occurrence of special interest reported by Dr. Sutherland followed on a case of septic scarlet fever, in the post-exanthematous stage, which had been inadvertently admitted to a diphtheria ward of fifty beds, where it remained for forty-eight hours before removal. Six cases of scarlet fever resulted from this source of infection. The Dick test was carried out in all cases in the ward on recognition of the special case as one of scarlet fever. Of the fifty patients thirteen gave positive reactions. All of the six scarlet fever cases occurred among those who had given a positive reaction to the Dick test.

Dr. Hodgson, reporting on the work carried out at the Fazakerley Hospital, refers to the anomalous case of a boy, aged 8 years, in whose instance the Dick test carried out on the twenty-eighth and again on the fifty-third day of the disease afforded a positive result on each occasion, notwithstanding "very regular and positive evidence from the bacteriological aspect." This was a severe "septic" case when admitted. The boy was extremely ill for over two weeks and subsequently developed otorrhœa, followed by persistent rhinorrhœa. A further case (boy, aged 12 years) admitted with the remains of a rash, which was followed by typical desquamation, gave a positive reaction on the sixth day of the disease, and a (?) positive reaction on the thirty-third day. With these apparent exceptions both Dr. Sutherland and Dr. Hodgson are in agreement as to the probable value of the test for the differentiation of individuals susceptible or insusceptible to scarlet fever respectively.

It is essential to bear in mind that the problems connected with the diagnosis of scarlet fever and with immunization against this disease are, in certain respects at any rate, much more complex than in the case of similar work in connexion with diphtheria. Of special importance, as antecedent to all else, is the necessity for accurate differentiation of the specific micro-organism responsible for the production of the disease in susceptible individuals. In the case of diphtheria, this difficulty has not arisen, for the reason that there has been for many years past consensus of opinion on this point. But in the case of scarlet fever the matter is quite otherwise. As the outcome of the large amount of research which has been carried out within the past two or three years, especially in America, a considerable body of evidence has accumulated in favour of the belief that hæmolytic streptococci are to be regarded as the responsible agents. But the variety of strains that has been isolated from the throats of patients suffering from this disease has rendered it difficult, as yet, to determine precisely whether one particular strain is concerned, or whether any of several varying strains are specially in question.

Again, the divergent results not infrequently met with in connexion with the classification of hæmolytic streptococci by agglutination and absorption tests introduce further difficulties. As previously stated, the Dicks apparently claim that strains of hæmolytic streptococci, one of which exhibits the property, in culture, of fermenting mannite while the other does not, are both capable of inducing an attack of scarlet fever when applied to the mucous membrane of the throat. If this be so, the question arises as to the extent to which the evidences of the disease are possibly the outcome of multiple infection with various strains of hæmolytic streptococci, since, as already stated, instances are on record of two or more strains, varying in their agglutination and other reactions, having been isolated simultaneously from the same patient. On this point obviously more knowledge is desirable.

As Dr. Goodall recently pointed out in a discussion on the differential diagnosis of scarlet fever, a mistake frequently made, especially by students, is to think that the diagnosis of this disease can be based upon any one symptom rather than, as is essential, on a combination of symptoms. Is it, then, possible that the variation in individual symptoms referred to by him may be correlated with infection by

particular strains, or by any particular combination of strains, of hæmolytic streptococci? And further, may some such condition have relation to the special type of disease, known as the "fourth disease" or para-scarlet fever, which, incidentally, Dr. Goodall states he has never met with in epidemic form?

CONDITION AS TO SUSCEPTIBILITY OF POPULATION TO BE IMMUNIZED.

In reference to the ultimate objective aimed at in the investigations under consideration, it may be well to direct attention to an interesting point to which sufficient attention has hardly, perhaps, hitherto been paid. This concerns the nature of the population which it is sought to immunize, *quâ* degree of susceptibility as estimated by means of the intradermal test. Experience has clearly indicated, in the case of our work on diphtheria, that where a certain general basis of immunity already exists, it is comparatively easy to increase the extent of this immunity to a sufficiently high level. On the other hand, in certain institutions which for years past have been completely free from this disease, probably owing to the fact that most of the children are recruited from rural areas, we found that immunity was practically non-existent. And, unfortunately, we also found that, in consequence, the building up of a sufficient degree of immunity was a matter of no little difficulty, requiring indeed, as eventually became evident, a time interval more lengthy than observed in any of our previous investigations. It is true that "toxoid" instead of toxin-antitoxin solutions were employed in the earlier stages of the work of immunization in the particular instance under consideration; but this fact, if one may argue from the experimental animal to man, should presumably, have had the effect of lessening rather than of lengthening the period of time required for the acquisition of complete immunity. It remains to be seen whether the response to "immunization" work in scarlet fever will be found to run on similar lines.

In conclusion, I ought perhaps to confess that I was decidedly impressed with what I learnt in New York of the results there of the practical working of the Dick test; of its potentialities in the determination of susceptibility or immunity to scarlet fever; and of its usefulness in aid of diagnosis. In spite of certain experiences which require further elucidation, my opinion as to its probable value has not been altered to any appreciable extent by subsequent experience of the use of the test in this country. There can be also, I think, but little doubt as to the specificity of the Schultz-Charlton reaction, or as to its diagnostic importance.

On the other hand the question of immunization against scarlet fever is, at present, in a somewhat different position, and it is, I think, as yet hardly possible to estimate what is likely to be its precise value as compared, for instance, with immunization against diphtheria. As a matter of fact, so far as is at present known, attainment of anything like complete immunity by means of inoculation methods appears to be decidedly more difficult of accomplishment in scarlet fever than is the case in diphtheria. Moreover there is, unfortunately, some reason for believing that any immunity thus produced is by no means permanent. But, in spite of—indeed in consequence of—apparent difficulties, it is obviously essential that further work, both on the clinical and laboratory side, should be initiated and carried through with all possible dispatch. From what has been so far accomplished there would appear to be definite grounds for belief that ultimately a really satisfactory method for controlling the incidence of scarlet fever may emerge from the present experimental work.

Discussion.—Sir WILLIAM WILLCOX said that the paper on this subject by Dr. Copeman was a very valuable contribution. It was most important that the medical profession in this country should be kept in touch with the researches proceeding in America. From what had been said by the lecturer it was clear that the Dick test provided a valuable means of determining whether immunity was present or not in an apparently healthy person. The test was

also of some value in diagnosis. With regard to the production of immunity it was important that the immunizing injections should be free from horse-serum. At the present time sera prepared from immunized horse-serum were in use for diphtheria, tetanus and streptococcal infections. It was well known that one or two injections of horse-serum might render a person sensitive to this serum. In other words, an anaphylaxis might be caused by the use of an immunized horse-serum and serious collapse might ensue from its use. If an immunized horse-serum were used in the protection against scarlet fever this might contra-indicate the use of more important serum injections such as those of diphtheria, tetanus, streptococcal infections, should the need arise. It was quite realized that the inoculations with scarlet fever toxins in the performance of the Dick test did not involve the use of horse-serum, and therefore these inoculations were not contra-indicated on account of anaphylaxis.

Dr. J. A. NIXON said that at Bristol the Schick test had yielded such satisfactory results that all concerned in its application were desirous of learning about the possibility of detecting susceptible and non-susceptible cases of scarlet fever also, and of immunizing susceptible subjects. He was not in the least deterred by the danger of anaphylaxis pointed out by Sir William Willcox. During the war repeated doses of anti-tetanic serum were given to wounded men without the occurrence of any untoward results.

Dr. E. W. GOODALL said that, as one who was not a bacteriologist, he was surprised that, though it had been believed for about forty years that a streptococcus was the cause of scarlet fever, it had been left to the Dicks to bring forward what, up to the present time, was the most convincing evidence in favour of a particular micro-organism. The late Professor Klein was, he believed, the first to associate streptococci with scarlet fever, but absolute proof that a special organism was the cause of the disease had never been produced, though from time to time very confident assertions had been made in support of particular organisms. He (the speaker) in common with the majority of clinical observers, had come to the conclusion that the true cause of scarlet fever had not yet been found and that the streptococci were only secondary invaders. No bacteriologist before the Dicks had, he understood, succeeded in obtaining a toxin from the streptococci concerned. This seemed to him to be a remarkable fact. It might be stated that the Dicks were the first to make use of the human skin to demonstrate the presence of the toxin in the medium in which the cocci had been grown, but they had gone further than that and had succeeded in immunizing horses by its means. Certainly the result of the Dicks' work was more convincing than any that had yet been obtained.

He had had some experience both of the Dick and of the Schultz-Charlton tests. In respect of the Dick test he had got results which had not agreed with those obtained by other observers. By the kindness of Dr. O'Brien he had been supplied with some of the toxin in two strengths, one of $\frac{1}{50}$, the other of $\frac{1}{500}$. He had been assured that the $\frac{1}{50}$ had yielded good positive results in other hands, but in the cases in which he had tried it he had found that while the $\frac{1}{50}$ gave good positive results the $\frac{1}{500}$ gave negative or extremely feeble positive results, even when tried on the same person. Published accounts of the test had also contained conflicting statements as to results. It appeared, therefore, that a satisfactory method of standardizing the toxin had yet to be devised.

As regards the Schultz-Charlton reaction, it had been stated that the serum of most horses did not produce the blanching effect in the rash of scarlet fever; but he had tried one horse-serum which had certainly effected a very marked blanching. He (Dr. Goodall) hoped that some help to clinical physicians would be forthcoming as a result of all these observations. It would be of the greatest assistance if it could be shown, for instance, that a negative Dick test in a person who had, or had just had, a rash of doubtful character was evidence that that rash was not the rash of scarlet fever.

Sir William Willcox was under a misapprehension when he stated that immunizing persons with the scarlet fever toxin would sensitize them to horse-serum and that, therefore, they might get an anaphylactic attack if subsequently it was necessary to inject them with any therapeutic horse-serum. The scarlet-fever toxin did not contain any protein that would sensitize to horse-serum, so that there was no fear of such an anaphylaxis. Dr. Nixon had remarked on the rarity of anaphylactic phenomena in the Army during and immediately after the war, when so many soldiers were re-injected with antitetanic serum. But he (the speaker) had reasons for believing that many of the cases had been overlooked. When he was in charge of a military hospital he himself had seen five severe cases, nearly all of which would probably have been misinterpreted had he not seen them himself, as he had caused it to be

known that he was interested in the effects of serum and that he desired to be informed of any out-of-the-way occurrences which might follow its use. However, these serious phenomena were certainly not so common as they had been for some time after serum treatment was first introduced, and he believed that that was largely due to the use of Besredka's method of de-sensitizing the patient by injecting about $\frac{1}{2}$ c.c. of the serum at least four hours before the therapeutic dose was given. The interval should be at least four hours. In one case—the last case of severe anaphylaxis that he had seen some three years ago—the de-sensitizing dose had been given only two hours before the therapeutic dose.

Lastly, he (Dr. Goodall) said he was glad to know that the Ministry of Health was organizing an investigation of these tests in several fever hospitals in different parts of the country, as the results of that action should enable a definite conclusion as to the value of these tests soon to be reached.

Dr. FREDERIC H. THOMSON said that work at his hospital had been mainly directed towards testing the treatment value of anti-scarlatinal serum and the local blanching it produced when injected intradermially. He found it very difficult to form a sound judgment as to the value of the serum for treatment, because scarlet fever was at present so very mild; but so far he was inclined to be favourably impressed. It was probably easier to form a judgment in New York, for the death-rate from scarlet fever in the Willard Parker Hospital during the five years 1919-1923 was about six or seven times as great as at his hospital.

Dr. W. MAIR said that to speak of the Dick test as a test for susceptibility to scarlet fever, and of the injection of the Dick toxin as an immunization against scarlet fever, was begging the question. These assumptions took for granted that the hæmolytic streptococcus was the essential causal organism of the disease, and this was not yet by any means proved. There was no statistical evidence of value in support of either assumption. Since it had been shown by Dochez and others that an anti-streptococcal serum could be prepared which had the property of blanching the rash in scarlet fever, it was now clear that the rash was the result of a streptococcal toxin, and that the streptococcus played a more important part in scarlet fever than had generally been supposed. This was the one substantial fact which had emerged from the American work. It might be that purely streptococcal infections were sometimes diagnosed as scarlet fever; the diagnosis of mild cases of the disease was notoriously difficult. In the disease as it occurred in epidemic form it was probable that other essential factors were concerned.

Sir WILLIAM HAMER said that Dr. Klein, rather more than thirty-five years ago, believed he had discovered the causal streptococcus of scarlet fever. Now the like conclusion regarding a similar organism had been reached by American workers. It thus appeared that the periodicity of the wave of prevalence of the belief that this streptococcus was the cause of scarlet fever had now been established. Consideration of the evidence recently adduced, with respect to infection of volunteers, suggested the following criticism. When the hog cholera bacillus had been dethroned and an ultravisible virus reigned instead, the fact was noted that, whilst the bacillus produced a disease like hog cholera, it differed from hog cholera in not being transmissible from pig to pig. The bacillus was thus an associated organism and not the real cause of the disease. In the light of this fact was it not now incumbent upon those, who claimed that they had discovered the causal organism of scarlet fever, to produce evidence to the effect that this organism was capable of producing a disease which was infectious from case to case?

Dr. COPEMAN briefly replied to the various points raised in the discussion.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

An Outbreak of Food Poisoning Caused by the Bacillus Aertrycke.

By W. L. BURGESS, M.D., D.P.H.

(Medical Officer of Health, Dundee.)

THE outbreak of food poisoning to be described was very thoroughly investigated. This was necessary in order to satisfy the minds of the members of the affected community, who were considerably disturbed by the occurrence, and who rightly demanded an explanation and an assurance that repetition was not likely to take place. The infected food and the causative organism were discovered without any very great difficulty, and the further investigation was carried out in the hope that the route of infection of the foodstuff might be discovered, and that in this way a small contribution might be made to our knowledge, admittedly somewhat meagre, of this aspect of food poisoning. If some of my hearers consider the description of the inquiry a trifle laboured, I hope they will accept that as my excuse. We cannot yet afford to permit any outbreak believed to be due to an infected food, however small or however unimportant it may appear to be, to pass without very minute inquiry into the probable causes. Effective preventive measures depend on a thorough knowledge of the routes of infection, and while we are now very familiar with the micro-organisms usually responsible, we have yet a great deal to learn regarding the more remote causes.

As is usual in the course of a detailed inquiry, other issues, not directly concerned with the main object in view, presented themselves. These are not dealt with in detail in this paper, but one or two of them are merely referred to as being of importance in connexion with the general questions of food control and food poisoning.

The conclusions arrived at are in some cases based on evidence which is of necessity incomplete, and quite different interpretations may be possible. Every effort was made, however, to put to the best use the information collected.

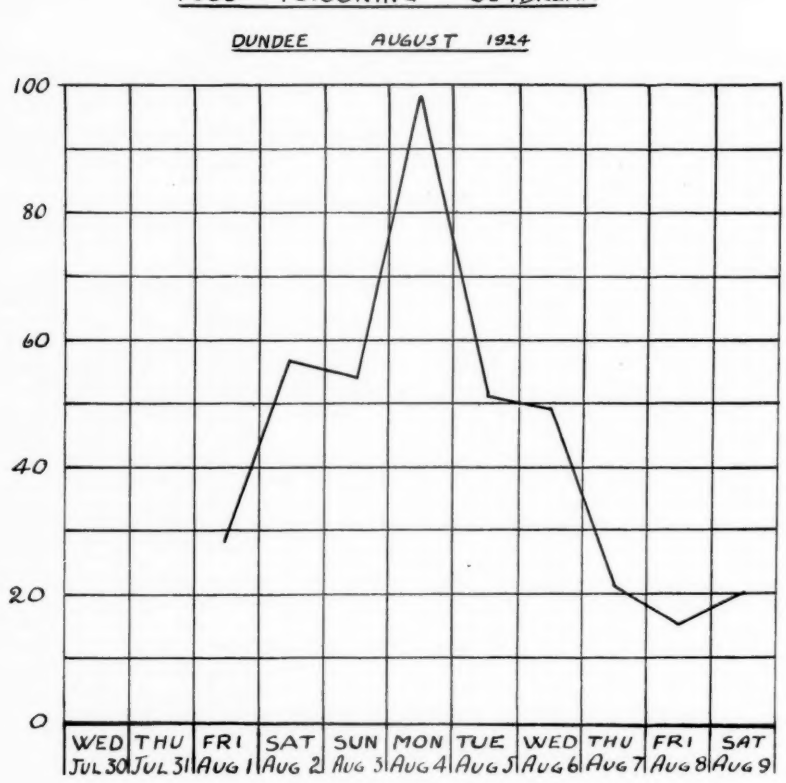
On Monday afternoon, August 4, 1924, my Department received information from a medical practitioner that there were an unusually large number of cases of gastro-enteritis in the city. In his opinion, the condition was due to food poisoning, and I found from several other practitioners, who were immediately communicated with, that they concurred in that view.

Subsequent inquiry showed that an outbreak had commenced on Friday, August 1, that a large number of cases had occurred on the 2nd and 3rd, and that new cases appeared daily until Wednesday, August 6. Probably the largest number of persons sickened on the Saturday or Sunday, judging from the fact that the daily number of new messages received by medical practitioners reached its maximum on the Monday. It was not until the Monday that it was realized that there was an extensive outbreak and that it might be due to food poisoning. Particulars were obtained from medical practitioners regarding a number of the affected households. These were immediately investigated, and the information collected in the course of the first hour or two pointed strongly to cream cakes as being the responsible foodstuff. More complete investigation proved conclusively that the outbreak was due to the

Bacillus Aertrycke, conveyed by a cream mixture put into cakes made at a certain bakery in the city. The infected foodstuff was on sale to the public for some six days, but the evidence suggests that only one large batch of cakes was contaminated.

GENERAL CHARACTER OF THE OUTBREAK.

The outbreak comprised 703 known cases. Information regarding 641 was supplied by medical practitioners in the district, while the remaining 62 were discovered in the course of the investigation by members of the staff of the



Department. The number of known cases is probably far short of the actual number, for there is good reason to believe that there were many victims who did not receive medical attention. Probably the actual number of patients was nearer 1,000 than 700. The diagnosis in the majority of the known cases was based on clinical and bacteriological evidence. Laboratory tests could only be applied to a limited number, but in all of them the findings were definite and uniform, proving beyond any doubt that the infecting agent was common to them all.

The outbreak began very suddenly and ended in a similar fashion. In nearly all the thoroughly investigated cases, the illness commenced on Saturday, August 2, or Sunday, August 3, and no case sickened after Wednesday, August 6. In 393 of the

641 medically attended cases, I was able to ascertain the dates on which the patients were first seen by their medical advisers. The accompanying chart has been prepared from this information. The highest point in the curve is probably at least one day later than it would be in a curve representing the course of the outbreak, based on the dates of onset. Many of the victims seen by doctors for the first time during the last three days of the period included in the curve had been ill for several days.

The week from July 31 to August 6 inclusive is considered to be the period during which the public were exposed to infection. I have no record of any cream cakes eaten before or after these dates having given rise to illness. In more than 50 per cent. of cases the food was consumed on Friday, August 1.

In 433 cases, the age and sex distribution was ascertained to be as follows :—

Males—125 or 29 per cent. Females—308 or 71 per cent.
Under 5 years—29 or 7 per cent; between 5 to 15 years—44 or 10 per cent,
Over 15 years—360 or 83 per cent.

The figures indicate a high incidence in females and a very low incidence among infants and children.

The 433 cases belonged to 269 households.

The signs and symptoms were those usually met with in an outbreak of this nature. Numerous instances were provided by medical practitioners showing that the severity of the symptoms appeared to depend very often on the amount of the suspected food consumed. The incubation period varied from two and a half to twenty-four hours. In one case, with an incubation period of two and a half hours, it was stated that the patient had consumed a very large amount of the infected cream. In the majority of patients the onset was extremely sudden, pain, vomiting and diarrhœa being the initial symptoms, these being accompanied by pyrexia, headache, chills, sweats, &c. As a rule the diarrhœa was very severe, amounting very often to seven or eight motions in one hour, continuing thus for varying periods and ending sometimes in collapse of the patient. Hæmorrhage from the bowel occurred in a limited number of patients. Delirium, erythematous skin rash and jaundice also presented themselves, while in two patients it is reported that appendicitis supervened. Recurrence of the diarrhœa is recorded in a few cases only.

The acute stage lasted from two to ten days, the average being four to five days. Convalescence was usually rapid. Although in many cases the illness was extremely severe, only one death, an infant, aged 6½ months, is believed to have occurred. The diagnosis in this case was made on clinical and epidemiological evidence only.

The Vehicle of Infection.

It is unusual in extensive food-poisoning outbreaks to have much difficulty in detecting the responsible foodstuff. On this occasion, however, the peccant food was only found after very careful inquiry. From the first it was evident that the infection was very widespread, and that it was not confined to any particular quarter of the town; consequently that the food was one in common use. Popular opinion at the time pointed to the water supply as being responsible. Milk was ruled out early, as also were meat foods. It was only after a process of very careful elimination that cream cakes were suspected, and this suspicion was rather strengthened from the fact, noted very early, that the majority of the patients were adult women. The continued investigation rapidly confirmed our suspicions beyond any possibility of doubt, the overwhelming epidemiological evidence being supported by the bacteriological findings.

The widespread character of the outbreak is explained by the fact that the cream cakes were made in a very large bakery and distributed through eighteen retail shops and two restaurants situated in various parts of the town. I am informed that there

are sent out from this particular bakery about 5,000 cream cakes daily and that there are about eighteen different varieties of these cakes.

The kinds of cakes which gave rise to illness included cream sandwiches, cream tarts, cream horns, cream cookies and cream meringues, &c., and this variety in kind pointed to the probability that the cream and not the cake substance was the vehicle of infection. In the end, this proved to be true. Immediately the confection was suspected, a search was made by the investigating officers for portions of cakes alleged to have given rise to poisoning. Two pieces of sandwich were secured from different houses, and from the cream in both of these, Professor Tulloch, Dundee City Bacteriologist, recovered an organism which he has proved to be the *Bacillus Aertrycke*, a member of the Salmonella group. He succeeded in isolating exactly the same organism from the stools of eight out of ten persons who suffered from the infection. The identity of the organisms obtained from these two sources was established by means of comprehensive agglutination tests, and their pathogenicity was proved by animal inoculation.

Portions of the infected cakes were examined by the City Analyst for the presence of preservatives and of metallic and organic poisons. Except for a small percentage of boric acid (·06) the results were negative.

On the first day of the inquiry, when it was considered that there was sufficient evidence to justify action, the bakery was visited and the managing director interviewed. It was explained that the cream cakes purveyed by his firm were suspected of causing serious illness among the consumers. As all the affected households investigated up to this point had purchased the cakes on Friday, August 1, he was informed that immediate steps must be taken to withdraw from sale any of Friday's goods remaining undisposed of. The manager stated emphatically that it was quite impossible for any of the cream goods on sale on the preceding Friday to be still available to the public on the day of the interview (Tuesday, August 5). He explained that the cream arrived at the bakery from a large dairy in the city each morning at 6.30 a.m., and was used immediately for the preparation of the various confections which were issued the same morning to the various shops and restaurants, and were all sold or returned to the bakery before the end of the day. It was made quite clear to me that it was not possible for one day's preparation of cream goods, always made with fresh cream, to be sold to the public the following day. That seemed very satisfactory, and I felt sure that there was no immediate danger to the public health unless the cream should again become contaminated. The manager was, however, asked to provide a written guarantee regarding Friday's goods in terms of his statement. I received the required letter dated August 7 on Friday, August 8. It said, *inter alia*, "you further asked me to state that no goods of Friday's cream are now in existence as far as we can ascertain. We have no hesitation in giving this assurance. No sale can now take place of any of Friday's goods or cream." I understand that a member of the staff of the firm visited the various shops in order to be quite sure of the position.

I also interviewed the foreman confectioner of the bakery at his residence in order to get some first-hand information regarding the general routine in his department. His statement was substantially the same as that of the managing director. He stated that the cream arrived from a local firm of milk dealers about 10 a.m. every morning, and that it was stored in the churn, unsealed as it arrived, surrounded by ice, in the cellar of the bakery, until 6.30 the following morning, when the preparation of cream goods commenced. On all other points his story agreed with that of his manager. For the preparation of the cream mixture, the churn is taken to the confectionery room, emptied into a large metal receptacle and mixed with beaten-up whites of eggs (fresh or preserved—liquid eggs are never used), 1 gallon to 7 gallons of cream and sugar (6 to 7 lb. to 7 gallons of cream). A few drops of

vanilla essence are then added. The mixing is usually done with the hand and arm, and there seems to be no doubt that great care is always taken to cleanse the hands thoroughly before commencing to handle the material. Thorough cleanliness of the hands is apparently absolutely necessary for the success of the mixture. After washing, and immediately before commencing to handle the cream, the hands are dried on empty flour bags. Immediately the mixing is completed, the cream is introduced into the various kinds of cakes by means of what are known in the trade as savoy bags.

The week ending Saturday, August 2, was the Dundee holiday week, and the bakery was closed for three days—Monday, Tuesday and Wednesday of that week, so that no cream goods were made from Saturday, July 26, until Thursday, July 31. The outbreak commenced immediately after the bakery was re-opened. In the course of the inquiry I found that 1 gallon of cream, not required on Saturday, July 26, had been kept in the churn in the cellar and was mixed with 14 gallons of cream received on Wednesday, July 30, and used on the morning of Thursday, July 31. I was informed that this was not a common practice, but that each day the consignment of cream arriving the preceding morning was completely used up. The foreman stated that in the summer months particularly there was no difficulty in using the cream, as there was a greater demand for cream cakes then than at other seasons of the year.

The cream used is supplied by a local dairy, which handles daily some 3,000 gallons of milk and from 30 to 70 gallons of cream. It receives its supplies from farms situated in various parts of the country, but chiefly in the south of Scotland, which district provides the whole cream supply, a consignment arriving at one of the Dundee railway stations about 4 a.m. each day. Soon after that hour the cream is transferred to the company's premises for distribution. It does not come direct from the farms, but is prepared in a large creamery in a border town, from milk supplied by farms in the district. As a rule, all the cream arriving in Dundee is disposed of on the day of arrival, only very occasionally is there any kept (in a cold store) until the following day. I was informed by the manager of the dairy that the cream for the bakery involved in the outbreak was always sent out about 10 a.m. and that it invariably formed part of the consignment arriving in the city the same morning.

Having obtained this general information regarding the source of the cream and the method of its handling in the course of the manufacture of cream goods, an effort was made to discover if only one day's preparation of cream cakes had been contaminated or if the infection had been repeated day after day; and, further, to discover the particular batch or batches responsible for the trouble. An attempt was also made to find out as nearly as possible the particular point at which the contamination of the cream took place in its long journey from the farms in the south of Scotland to the homes of the consumers in Dundee.

At first sight, it would appear that at least five and possibly six separate days' supplies of cream cakes had been infected, for, while the majority of the cases of poisoning followed the consumption of cakes bought on Friday, August 1, cases were undoubtedly caused by cream goods purchased on the afternoon of Thursday, July 31, on Saturday, August 2, and on Monday and Tuesday, August 4 and 5. There is also evidence that a purchase made on Wednesday, August 6, was responsible for illness. These facts, coupled with the information already obtained, that cream goods are prepared every morning from fresh cream, and sold the same day or withheld from sale, certainly suggest that several batches of cakes were infected. Nevertheless, I am inclined to believe that only one batch was responsible for the whole outbreak. The curve of the outbreak (p. 62) is that of a one-day infection. Repeated infection of the cream would have produced a curve with a flat summit

and not a peak. Further, the evidence suggests the possibility that the cream goods prepared in the morning may not always be sold out or returned to the bakery the same day. Several trifling incidents in the course of the investigation pointed to this possibility, and an examination of the accompanying table would appear to support this view. Column 3 of the table shows that no cream was supplied to the bakery on any of the three days, Friday, Saturday, and Sunday, August 1, 2 and 3 (from books of dairy). Presumably, therefore, no cream goods were prepared on Saturday, Sunday or Monday (column 4 of table). If Friday's baking was sold out that day, or withdrawn from the retail shops, no cream cakes of any kind could be for sale after Friday evening until Tuesday morning. Nevertheless they were undoubtedly sold on both Saturday and Monday, as I have definite evidence of purchases made on both these days (column 5).

FOOD-POISONING OUTBREAK.

Dundee, August, 1924.

1 Date	2 Cream consigned to dairy	3 Cream supplied to bakery	4 Cakes prepared on early morning of days marked +	5 Poisoning caused by cakes purchased on days marked +
Monday, July 28	0	0	0	0
Tuesday ,, 29	0	0	0	0
Wednesday ,, 30	34 gallons	14 gallons	0	0
Thursday ,, 31	34 ,,	14 ,,	+	+
Friday, August 1	68 ,,	0	+	+ + +
Saturday ,, 2	0	0	0	+
Sunday ,, 3	0	0	0	0
Monday ,, 4	34 gallons	14 gallons	0	+
Tuesday ,, 5	34 ,,	14 ,,	+	+
Wednesday ,, 6	51 ,,	14 ,,	+	?

It seems, probable, then that only one batch of cakes was infected. By far the largest number of cases of illness was due to cakes bought on Friday, but nine persons were infected by purchases made on Thursday afternoon. The whole outbreak would seem to have resulted from the cream goods prepared on the morning of Thursday, July 31, a certain amount being sold the same afternoon, the main bulk on Friday and smaller quantities on Saturday, Monday and Tuesday, and possibly even Wednesday. This batch was prepared from 14 gallons of cream arriving at the bakery on the preceding morning, to which was added the gallon of cream left over from Saturday, July 26; the latter formed part of a consignment of 35 gallons delivered to the bakery on Friday, July 25. This circumstance sounds suspicious, but there is no definite proof that it had any direct bearing on the outbreak. The mere keeping of the cream for several days might possibly increase the opportunities for contamination, and once infected there would be ample time for the growth of the specific organism. The consignment to which this gallon of cream originally belonged was presumably free from infection, for no cases of illness could be traced to the cream goods prepared from it.

The cream, as already stated, had a long journey from the place of production to its final destination in the house of the consumer, and no doubt it had many opportunities of becoming contaminated, but I think the evidence is in favour of the specific infection having occurred after its arrival in Dundee. The milk from part of which the cream was separated was produced at some sixty farms in the southern counties of Scotland, and pasteurized in a neighbouring creamery. Cream out of the same bulk was distributed in the neighbourhood of the creamery and transmitted by train to Edinburgh and Dundee. I made inquiries from the medical officers of health concerned and found that Dundee was the only district receiving cream from this source in which an outbreak of gastro-enteritis was known to have occurred.

The Dundee consignment arriving on the morning of Wednesday, July 30, part of which is believed to have been responsible for the outbreak, consisted of 34 gallons in two 17-gallon churns. These were not locked, and they travelled to Dundee in a luggage van with a large number of other churns containing milk. On the arrival of the train in Dundee, the cream was taken to the milk depot, and one of the churns, after the removal of 3 gallons of cream, was delivered to the bakery at about 10 a.m. The contents of the other churn and the 3 gallons of cream removed from the first were distributed in varying quantities to bakers and retail milk shops in Dundee and district. We have details of these deliveries, and while it was impossible to obtain information regarding all the purchasers, we were able to follow up a certain number. No case of illness traceable to this cream was found. Particular attention was paid to the 3 gallons removed from the churn sent to the bakery. These 3 gallons were mixed with another 3 from the second churn and sent to a retail dairy from which it was sold in small quantities to regular and casual customers. No illness occurred among the few regular customers who could be followed up.

The evidence therefore suggests that the cream was free from infection on its arrival at the bakery. There can be little doubt, however, that when it left as cream mixture in cakes it contained the *Bacillus Aertrycke*. This batch of cakes was distributed through eighteen retail shops and two restaurants, and when purchased therefrom and consumed gave rise to symptoms of poisoning.

The Source of Infection.

It was thought advisable to exclude as the source of infection the only employee of the dairy who handled the churn containing the cream. Specimens of his blood and fæces were examined by Professor Tulloch. The results were negative.

Apparently either one of the ingredients of the cream mixture had been infected before its preparation commenced, or the mixture itself had been contaminated in course of preparation, but before being introduced into the cakes.

Although the investigation had been considerably narrowed down in regard to the time and place of infection, no evidence was obtained to indicate the actual point of infection. Apart from the gallon of cream which was used after being retained in the bakehouse for some days, the most careful inquiry failed to elicit information regarding any factor in operation in the bakehouse about the period of the commencement of the outbreak which was not in operation at any other time. Accordingly, the investigation had to be continued along generally accepted lines. An endeavour was made to discover, in or near the bakehouse, a focus of the infecting organism, and to find the means by which that micro-organism had been transferred to the cream mixture or to one of its ingredients. This was done by attempting to exclude centres which are known to be liable to be sources of infection of food poisoning.

The employees in the bakery engaged in the handling of the cream mixture and its constituents were examined. These numbered thirty-one (seventeen women and fourteen men). No information was obtained suggesting that any of them had suffered either recently or at a more remote period from any illness likely to be due to a food-poisoning organism. None of them had, to their knowledge, been at any time in contact with persons suffering from food poisoning. The blood and fæces of each one of the thirty-one employees were submitted to bacteriological examination. In all except six the results were negative. The employees in the six positive cases all admitted having suffered from gastro-enteritis during the period of the outbreak, and they had to be accepted as victims of the infection and not as carriers of the organism. The presence of a carrier in the bakery engaged in the preparation of the cream mixture might account for the outbreak, as the transference of the infecting organism to the foodstuff would be an easy matter, especially if the carrier

happened to be the employee engaged in stirring the cream mixture with his hand and arm.

Attention was also directed to the possibility of animals in or near the premises harbouring the micro-organism. A piggery is run in connexion with the bakehouse, but situated at least two miles from it. The pigs, sixty in number, were examined by the city's veterinary surgeon, who reported that they were all in a healthy condition and that there was no suggestive history of illness among them. Nor could he discover any history of contact with pigs suffering from illness which might be associated in any way with the *Bacillus Aertrycke*. It was nevertheless decided to examine the fæces of all the sixty animals, but by the time twenty-seven had been examined and found negative, the remaining thirty-three had all been disposed of for purposes of slaughter. The fact that the piggery is situated some considerable distance from the bakery might in itself be considered sufficient to exclude it as a source of infection, but there was a channel by which infection could have been conveyed to the bakery. Circumstances made it quite possible for an employee to use for drying his hands or for wiping-out baking tins old flour bags which had been left at the piggery for a day or two after having been used to convey food refuse from the bakery. The use of such a bag by the confectioner for drying his hands immediately before commencing to stir the cream might conceivably result in contamination of the mixture. No proof that that occurred in this particular case is available.

The possibility of the cream mixture or one of its constituents having been infected by mice or rats harbouring the specific organism had to be considered. We were informed that there were mice but no rats in the premises. From the nature of the materials or from the manner of their storage it was possible to exclude the likelihood of contamination by rodents of the cream, the whites of eggs and the vanilla essence. This was not quite so certain in the case of the sugar, but I feel confident that no such contamination of this ingredient took place in the bakery. The sugar is kept in stout canvas bags in the centre of the store-room floor, the whole contents of a bag being emptied as required into a metal bin with sliding lid in the confectionery room. It is improbable that mice could reach the sugar in the bin, and even in the store I do not think they would trouble themselves to eat their way into a bag with any amount of easily accessible foodstuff near by. Five mice were captured and submitted to complete bacteriological examination. The findings were negative.

A poison for mice had not been used in the bakehouse for some eight months. None of the material actually used was available, but a sample of the same brand gave negative results.

An outbreak of poisoning caused by a food of animal origin suggests the possibility of the animal supplying the food being the source of infection. Arguments have already been submitted which suggest that the cream was free from the specific infection on reaching Dundee. If these are sound, infection in the cow can of course be excluded. The manager of the creamery in the border town made inquiries at the supplying farms, but no significant information was obtained. Nothing further could be done in this direction.

The cream mixture may have been contaminated by flies, and it was very easy for insects to gain access to the foodstuff while in the bakery. It was not possible to prove the case against this route of infection.

While, therefore, bacteriological proof was not forthcoming pointing to any particular route of infection, the epidemiological evidence was, in my opinion, at least suggestive. Assuming that the cream was contaminated in the bakery, two circumstances appear to me of outstanding importance.

(1) The stirring of the cream by the confectioner by means of the hand and arm, and (2) the fact that a quantity of cream left over on Saturday, July 26, was retained and mixed with

the fresh cream on the morning of Thursday, July 31, and that the outbreak is believed to have been caused by the cream goods made from this lot of cream.

In the case of the first of these, if the confectioner's hands were contaminated, self-infected or infected from flour bags used for drying the hands or from any other source, the cream would obviously be liable to be, and probably would be, infected. It will be admitted that only a comparatively small number of micro-organisms would cling to the hands, which have always to be exceptionally clean, at least from a culinary point of view, if a successful mixture is to result. The cream mixture (cream, plus whites of eggs, &c.) amounted to some 17 gallons, so that any slight infection on the hands of the operator would be enormously diluted. Nevertheless, the small quantity of cream present in a cream cake caused very severe illness when consumed only a few hours after the mixture was made. It would appear to be doubtful whether the micro-organisms would multiply so rapidly as to make such a large volume of cream so toxic in a short time.

I am inclined to attach more importance to the second circumstance which has been emphasized. It was reported that the gallon of cream used after being kept for some days was stored in the churn in the cellar. If that was the case, it was not likely to be contaminated from animal sources, but was of course quite accessible to human beings. I am not convinced, however, that the cream was stored as stated. On one occasion during the course of the investigation a small quantity—about a gallon—of cream mixture in a wide shallow basin was seen on the concrete floor of the cellar. Having been left over on the particular morning it was being stored there until the following day. Cream kept in this fashion would easily be contaminated by rodents. Mice for example could help themselves to it, and could even walk through it if they so desired. At any rate, a mouse falling into the cream while feeding on it could very easily escape. It is known that mice are susceptible to infection by the *Bacillus Aertrycke*, and, although unsupported by bacteriological evidence, I am of opinion that in the present outbreak the cream became infected in this way. It appears to me to be the only conclusion possible from the available evidence. The *Bacillus Aertrycke* gaining access in this way to the gallon of cream would have ample time to multiply, so that on the morning of July 31 the cream would form a very strong culture of the organism. This culture would fortunately be diluted by being mixed with 14 gallons of fresh cream and with the other ingredients. In this way a very much larger number of people became infected, but in all probability more serious results affecting a smaller number of persons were prevented.

The immediate preventive measures consisted in securing greater care in the storing of the cream, and in its handling during preparation. Definite instructions on this matter were given to the manager, those being confirmed in writing. As a certain number of the employees were proved to be harbouring the specific organism, having been victims of the outbreak, these were not permitted to return to work until repeated examinations proved them to be free from infection. Medical practitioners were asked to observe the same precaution with all patients who were engaged in the handling of foods, in their own homes or at work. It was suggested in a circular letter that such convalescents should be told to take special care in regard to personal cleanliness, and that they should, if possible, cease to handle food for a few weeks, or until they had been proved by laboratory methods to be free from infection. No cases of secondary infection are known to have occurred, and indeed, in those examined, the infection usually disappeared from the stools soon after the disappearance of the acute signs and symptoms.

Laboratory Investigation.

In the course of the investigation over 150 specimens of various materials were submitted for examination in the laboratory. These materials included various foods, fæces (human and porcine), bloods, mice, mouse virus, &c. Except for small quantities of boric acid in the samples of cream and of cream mixture, the chemical examinations by the City Analyst were negative. The City Bacteriologist, Professor Tulloch, submitted a very full report on his findings. I am not qualified to discuss the bacteriology of the infection, and I merely record that Professor Tulloch was able to satisfy himself of the nature of the organism and to establish unequivocally the relationship between the micro-organisms isolated from the peccant food and the cases of illness it was alleged to have produced.

General Comments and Conclusions.

The results of the whole inquiry may be summarized as follows:—

(1) An outbreak of gastro-enteritis occurred in Dundee early in August, 1924.

(2) The outbreak involved at least 703 persons, mainly adult women.

(3) The cause of the illness was the *Bacillus Aertrycke*, a member of the Salmonella group.

(4) The vehicle of the infection was the cream mixture contained in various forms of cream prepared in a certain bakery.

(5) The available evidence suggests that only one large batch of cream cakes contained the organism.

(6) The available evidence points to the probability that the cream or the cream mixture was contaminated in the bakery.

(7) The source of infection of the cream was not proved beyond dispute, but channels were found to exist by which the organism could too easily reach the foodstuff, given a focus of infection in or near the bakehouse. The evidence was at least suggestive that the cream, while in storage in the cellar of the bakehouse during a holiday period, was contaminated by rodents, probably mice.

The demand for cream cakes has apparently increased during recent years, with the result that cream is now being handled commercially to a large extent by persons other than those engaged in the production and distribution of milk. It may not be permissible to assume that the conditions above described are to be found in all similar premises. They certainly, however, point to a lack of appreciation of the care necessary in handling such a susceptible medium. The necessity for more supervision of the processes of preparation and handling of certain foodstuffs by the health officials is emphasized, when one learns that in such a large bakery a susceptible foodstuff like cream should be mixed by a man's hand and arm instead of by some device similar, but on a larger scale, to the beater used by the housewife in beating up the whites of eggs for use in home-made confections.

It would appear also that undue reliance was placed on the keeping qualities of the cream. In the case of a small quantity of the consignment believed to have been responsible for this outbreak, a period of almost a fortnight must have elapsed between the production of the milk from which it was separated and its consumption in cream cakes. No doubt the knowledge that the cream contained a certain amount of preservative was relied on. Unfortunately, while the boric acid may have prolonged the life of the cream, it failed to destroy or apparently even to prevent the growth of the food-poisoning bacteria which had gained access to it. It would seem therefore that the addition of the preservative did more harm than good.

Powers have been granted enabling local authorities to control to some extent the production and distribution of milk. No such powers have been given in regard

to the preparation and distribution of foods in bakeries. If, however, cream is to become an important ingredient in the goods made in these premises, it would appear necessary to consider whether the provisions in the various statutes dealing with the registration and supervision of purveyors of milk should not be made applicable to bakeries from which cream is issued to the public in small amounts contained in cakes. Those bakeries become literally purveyors of cream and, as such, require as much supervision as, or even more than, the recognized milk purveyor.

Discussion.—Dr. A. W. J. MACFADDEN said that Dr. Burgess had given a very lucid and able account of this extensive outbreak. He (Dr. MacFadden) said that there was no branch of epidemiological investigation which called for greater patience, attention to detail and the adoption of a wider outlook as to the scope of the inquiry than food-poisoning outbreaks. He had had an opportunity, at an earlier stage, of reading the official report of the occurrence just described, and he congratulated Dr. Burgess and Professor Tulloch on an excellent piece of work. Investigations of cases of food poisoning such as this, and the work which had been done recently by Dr. Savage and Mr. Bruce White on the identification and proper classification of the organisms concerned in such outbreaks, supplied material for a useful step forward in our knowledge. The next and perhaps most important advance was to be sought in the origins of these organisms which from time to time left their natural habitat to manifest themselves in human illness. The most hopeful field for exploration in this respect was, he thought, that of animal pathology, and he looked to such institutions as the Department of Animal Pathology at Cambridge for co-operation in this line of research. The scheme of investigation which the Ministry of Health had set on foot in conjunction with the Medical Research Council, reports on which had recently been published by the Medical Research Council, would be continued by the Ministry, and bacteriological examinations of specimens from local outbreaks in England and Wales were now being undertaken in the Ministry's Pathological Department.

Sir WILLIAM WILLCOX said that the very interesting epidemic of food poisoning investigated by Dr. Burgess had been due to one of the commonest causes at the present day in this country, namely, contaminated milk or cream, such as was used in the preparation of ice-creams, cream cakes, &c.

Dr. Burgess had made most thorough investigations, and it was impossible to criticize his methods or to suggest further lines of investigation, since nothing appeared to have been left undone. In his (the speaker's) experience epidemics of food poisoning which occurred during the war were, in almost every instance, traceable to a carrier, and it seemed probable in this case that the infection of the contaminated milk must have been due either directly or indirectly to infection by a carrier. The utmost care was required in the storage of milk and cream; frequent inspection of premises, in which food was stored, by representatives of the Public Health Department would be bound to lead to greater care and would be a protection to the public. At the present time those responsible for the storage of food usually lacked the requisite knowledge which would ensure an avoidance of contamination.

Dr. G. CLARK TROTTER (Islington) drew attention to the large amount of tedious and costly bacteriological work which was always necessary in these investigations. Dr. Burgess had been lucky; very often the results were not as conclusive, and not infrequently negative, but Dr. Burgess had obtained sufficient evidence at least to be strongly presumptive. Dr. Clark Trotter gave some instances in his experience of cream, and particularly ice-cream, associated with a small outbreak of enteric fever, the cream having been exposed to contamination by the vendor under particularly disgusting conditions. The association was, that several of the cases had partaken of the particular vendor's wares, and the remaining few children possibly also had partaken of them although forbidden to do so by their parents. Elaborate bacteriological examinations of scrapings from dirty vessels, and of the stools of the vendor's infant, who suffered from diarrhœa, had proved to be quite negative. The inquiry certainly stopped the conditions giving rise to the infection and, if the surmise of cause were correct, the occurrence of fresh cases.

With regard to cream cakes, the result of a dispute between a lady and a waitress overheard in a London restaurant was worth noting. At first he (the speaker) had been at a loss to understand the purport of the wrangle. The reason of it became apparent upon the lady

threatening to call the manageress, which resulted in her being served with the particular make of "cream" cake she had insisted upon receiving. She then remarked that the cake she had just been supplied with was made with fresh cream, whereas what she had been given first was compounded with preserved cream. Probably Dr. Burgess would confirm the fact that the victims of "cream" cakes were women, not children, owing to price and taste of the individual, and that the cream was preserved and kept for a considerable time, being used for more than one batch of cakes.

Knowing that it was possible to get fresh cream, that boric acid masked "smell and taste" warnings, and delayed putrefaction, while the other contamination was not checked, the wise person would avoid "cream cakes" until the law (as had been done in the case of milk) forbade preservatives in cream.

Section of Epidemiology and State Medicine.

President—Dr. JOHN C. MCVAIL.

The Health of London in the Eighteenth Century.

By JOHN BROWNLEE, M.D., D.Sc.

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PART I.—THE DEATH-RATE AS A WHOLE.

THOUGH the health of London in the eighteenth century has been the subject of a certain amount of attention in literature, yet the statistics in existence have never been thoroughly examined. These statistics for London are contained in the weekly and yearly bills of mortality. The area to which the bills refer includes ninety-seven parishes within the walls, sixteen parishes without the walls, twelve out-parishes in Middlesex and Surrey, and six parishes in the liberties of Westminster. For convenience this area to which the bills of mortality refer will be simply termed London.

The bills originated in connexion with the epidemics of plague, but they are not continuous till 1657, after which the series is complete. They contain the numbers of christenings and deaths occurring in each week of the year, the deaths being ascribed to a large variety of causes. From 1728 the ages at death are given for persons of both sexes combined in certain age-groups. It must be admitted that this is the most formidable body of statistics which exists. It cannot be dismissed summarily, but its trustworthiness can only be revealed by systematic statistical examination. I think that the value of the bills has been generally underrated, and, when not underrated, misunderstood. The bills at first are not homogeneous, but as the eighteenth century progressed I think they became considerably more homogeneous and capable of statistical use. The causes of death bear no relation to modern diagnosis.

Health is generally measured by death-rates, but before death-rates can be interpreted it is necessary to know how the population has varied by immigration and emigration. The estimated number of inhabitants of London at four different dates is shown in Table I. For 1665, Creighton estimates the population at 460,000; Sir William Petty for 1682 makes the figures 670,000; in 1738 Maitland

TABLE I.—POPULATION IN THE AREAS OF THE BILLS OF MORTALITY IN LONDON.

Date	Population	Authority
1665	460,000	Creighton
1682	670,000	Petty
1738	725,903	Maitland
1801	744,803	Census

makes it 725,903, and in 1801 the census enumeration gives the population for the same area as 744,803. The eighteenth century calculations are based on the ratio of eight persons to a house, and as this ratio was found at the 1801 census, the earlier value would seem justified. The increase was not continuous. There was an immense migration into London between the date of the plague and 1682. The actual increase between 1665 and 1682 is, according to the estimate, 210,000; if the deaths from plague be allowed for, a further addition is necessary. From 1682 to 1738 the increase was 60,000, or less than 10 per cent., and for the rest of the century was quite insignificant. Thus, for most of the eighteenth century, the population of London may be taken as approximately constant. In Table II a certain number

TABLE II.—SHOWING THE NUMBERS OF DEATHS IN LONDON CLASSED AS AGED AND OVER 60 YEARS. FOR COMPARISON WITH THESE, THE NUMBERS OF DEATHS FROM SMALL-POX AND VIOLENCE ARE GIVEN, TO WHICH IS ADDED THE NUMBER OF DEATHS IN CHILD-BED AND THE NUMBER OF ABORTIONS AND STILL-BIRTHS, WITH THE DEATH-RATES PER THOUSAND BIRTHS FOR MOTHERS CONFINED, AND CHILDREN BORN IN THE LYING-IN-HOSPITAL.

Periods	Aged Over 60 years (Numbers in hundreds)	Small-pox	Killed	Child-bed (Numbers in tens)	Abortions and still-births	Death-rates per 1,000 Births in lying-in hospital: Mothers	Children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1701-10	172	—	125	55	239	588	—	—
1711-20	205	—	195	52	245	624	—	—
1721-30	241	—	230	54	259	681	—	—
1731-40	188	329	206	49	246	621	—	—
1741-50	209	363	185	51	202	549	—	—
1751-60	156	287	206	61	190	592	26·7	68·2
1761-70	153	336	242	70	231	704	19·8	38·1
1771-80	135	296	209	64	194	601	14·3	21·4
1781-90	124	293	179	57	172	644	15·7	22·9
1791-1800	128	290	185	62	171	684	2·4	12·0

of causes of death in the bills have been selected in order to discover how far the constant population given by the estimates is supported by the bills. When those dying over 60 years are taken as a criterion (column 2), there is little variation after 1750. The high values for 1720 to 1750 are expected from the increase of the population at the end of the seventeenth century. Of these, more than 200,000 immigrants, probably a large majority were between the ages of 15 and 30 years, and therefore would be from 60 years of age and upwards during the years in question. With small-pox, the number of deaths shows no more than an ordinary epidemic variation from decade to decade in spite of the alleged increase of mortality due to infection from inoculation. The number of killed does not vary beyond random error, while the number of abortions and still-births shows like constancy. In all of these cases after the year 1750, there is a curious and, to me at present, unexplained large mortality in one decade, 1761-1770.

After these explanatory remarks, the births and deaths have now to be considered. In Table III the numbers of baptisms, estimated births and the number of

TABLE III.—CONSPECTUS OF THE HEALTH IN LONDON IN THE 18TH CENTURY, SHOWING FOR EACH DECADE THE NUMBER OF BAPTISMS, THE CORRECTED NUMBER OF BAPTISMS, THE NUMBER OF BURIALS, THE NUMBER AND PROPORTION OF DEATHS ASCRIBED TO CONVULSIONS AND DEATHS UNDER TWO YEARS OF AGE, THE CRUDE DEATH-RATES FOR LONDON AND ENGLAND AND THE DEATH-RATES FOR LONDON FOR CHILDREN DYING UNDER TWO YEARS IN RELATION TO THE NUMBER OF BAPTISMS AND THE CORRECTED NUMBER OF BAPTISMS.

Periods	Baptisms (Numbers in thousands)	Baptisms × 1·386	Burials	Deaths from convulsions (Numbers in hundreds)	Deaths under 2 yrs.	Ratio of deaths from convulsions to deaths under 2 years	Death-rates per 1,000: London	England	Death-rates under 2 years per 1,000 baptisms	per 1,000 estim'd births
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1701-10	156	216	215	582	—	—	29·4	28·6	—	—
1711-20	171	237	239	678	—	—	32·8	31·1	—	—
1721-30	182	252	275	767	—	—	37·8	34·9	—	—
1731-40	168	233	265	795	1019	0·78	36·2	35·8	607	438
1741-50	145	201	254	643	883	0·73	34·7	33·0	592	426
1751-60	148	215	205	538	751	0·71	28·2	30·3	500	360
1761-70	161	223	233	606	788	0·77	32·0	30·0	494	386
1771-80	173	239	215	561	772	0·72	29·5	31·1	442	319
1781-90	179	248	193	447	636	0·70	26·4	28·6	351	253
1791-1800	188	261	197	421	618	0·68	26·9	26·9	333	240

burials are given in columns 1, 2 and 3. The numbers of baptisms are much in defect of the numbers of births. The numbers relating to baptisms as compared with the numbers of births are too small, for the baptisms in the eighteenth century Dr. Farr adopted a correcting factor of 1·33. There are only three years, 1838-40, for which the numbers of baptisms and the numbers of registered births can be compared, and for these three years the ratio of registered births to baptisms in London is 1·386. I think this figure may be taken as probably near the mark, for although fewer children, proportionately, in 1838-40 would be baptized by the rites of

the Church of England, yet in the years 1838 to 1840, the registration of births was not compulsory and was defective. Dr. Farr used a correcting factor of 1·04 for the years 1840-1850. The product of 1·33 and 1·04 is very nearly 1·386.

When the estimated births and the burials are compared, they are seen to be very nearly the same from 1700 to 1720. For the years 1720-50, the decades at the ages when the immigration into the city towards the end of the seventeenth century would produce its effect, the deaths are notably higher. They are again much the same from 1750-1770, after which the falling death-rate among young persons brings the births in excess of the deaths. Though a high degree of accuracy cannot be claimed for these figures regarding the births and deaths, they would seem to indicate that the amount of immigration into London during the eighteenth century was not large and does not compare with the immigration into London itself and the chief towns of Britain that took place during the nineteenth century.

The crude death-rates for London and England are contrasted in columns 7 and 8. The death-rates for England are those I estimated some years ago. They are in essential agreement with those estimated independently by Dr. Farr, though my values throughout are less than his. It is not a matter of much moment which are taken, but I think it is better to compare two estimated values which are subject to the same personal equation. These estimates suggest that in the eighteenth century there is very little difference between the mortality in London and England as a whole. The crude death-rates for England and Wales as a whole, and for London since registration was introduced, are given in Table IV. Between 1840 and 1870 there was an

TABLE IV.—SHOWING THE CRUDE DEATH-RATE PER THOUSAND FROM ALL CAUSES FOR ENGLAND AND WALES AS A WHOLE, AND LONDON.

Period	England and Wales	London
1841-50	22	25
1851-60	22	24
1861-70	22	24
1871-80	21·3	22·4
1881-90	19·1	20·3
1891-1900	18·2	19·2
1901-1910	15·4	15·8

excess in London, but from that date the difference is very small, so that though the figures for the eighteenth century may look a little surprising, they are in accord with what is known in an era of accurate statistics. The impression obtained when reading eighteenth century writers is that London was a great source of the destruction of the manhood of England, immigrants being attracted like the moth to the candle only to meet extinction. Such evidence as exists shows that it is as likely that England as a whole was unhealthy. It is rather astonishing when the contemporary descriptions of life in parts of London are considered that the difference should have been so small.

From 1750 to the end of the century, the number of persons dying over the age of 60 is sensibly constant. The case with young children is, however, quite different. In Table III, columns 4 and 5, the numbers of deaths from convulsions are given for the whole century and the deaths under 2 years from 1731. A comparison of these columns shows that only a few of the causes of death of children under 2 years have even superficially been distinguished. In the seventeenth century the two great groups are convulsions and teething. From the beginning of the eighteenth century convulsions take the chief place, while teething sinks to insignificance. In column 6 the ratio of the deaths from convulsions to the deaths under 2 years is given from 1740 to the end of the century. This ratio is comparatively constant, and this is important, as it makes it probable that in the fifty years preceding 1730 the weekly deaths from convulsions may be taken to indicate fairly accurately the number of deaths of children under 2 years of age. In columns 9 and 10 the death-rates under 2 years per thousand baptisms and per thousand estimated births are given for comparison.

From 1730 to the end of the century there is a steady decline in the number of the deaths of children under 2 years, the death-rate at the end of the century being only 60 per cent. of what it was in 1730-40, so that a very remarkable fall in infantile mortality took place preceding the introduction of vaccination, the death-rate under 2 years of age per thousand births of children falling from 438 to 240. The infantile mortality, i.e., the mortality under 1 year, if modern conditions can be expected to hold, may be obtained from this by subtracting one quarter. Thus the infantile mortality in the last decade of the eighteenth century in London becomes about 180, not very much higher than that in the decade 1861-70, when it was 162, while in individual years about the same period it passed 170. Though, therefore, at later ages no improvement of any moment is found, a marked improvement in the hygiene of young children was continuously observed. A considerable fall in the mortality from child-bed accident also occurs, as may be seen from Table II, column 5, where the number of women dying in child-bed fell from 2,390 in 1701-10 to 1,710 in 1791-1800. The same fall in mortality is found in the London Lying-in Hospital, where the death-rate per thousand births among mothers fell from 26·7 in 1751-60 to 2·4 in 1791-1800, with a corresponding fall in the mortality among the children born. It must, however, be noted that no record with regard to the births is given of the length of stay in hospital.

The population from 1730 to 1800 increased very little. It is thus possible to construct rough life tables from the numbers of deaths alone. These must not be expected to possess a high degree of accuracy. A uniform statistical method, however, applied to the seven decades, 1730-1800, yields results of singular constancy. In Table V the expectations of life obtained by the calculation are shown for each ten

TABLE V.—SHOWING THE EXPECTATIONS OF LIFE IN LONDON FOR EACH DECADE FROM 1730 TO 1800 AS ROUGHLY CALCULATED, AND FOR COMPARISON CERTAIN DATA REFERRING TO RECENT TIMES.

Age	London							London 1910-1912		Manchester Township 1881-1890		Manchester 1881-1890
	1730-1739	1740-1749	1750-1759	1760-1769	1770-1779	1780-1789	1790-1799	Males	Females	Males	Females	Persons
10	36·9	37·0	37·3	36·9	36·7	37·5	38·2	—	—	37·47	40·94	44·11
20	29·1	28·9	29·2	29·3	29·4	29·9	28·4	42·35	46·71	29·61	33·08	35·99
30	23·7	23·5	23·8	24·1	24·1	24·9	24·4	33·87	37·94	22·82	26·04	28·43
40	19·6	19·2	19·4	19·6	19·5	19·5	19·5	26·03	29·67	17·39	19·95	21·88
50	16·1	15·8	15·7	16·1	15·9	15·7	15·8	19·09	22·17	12·77	14·62	15·95
60	12·2	12·4	12·1	11·9	11·9	12·0	11·9	13·09	15·39	9·24	10·21	10·82
70	9·4	8·6	8·7	8·5	8·3	8·7	8·5	8·17	9·57	5·91	7·18	6·92
80	5·9	5·7	5·7	5·7	5·7	6·3	6·2	4·79	5·39	3·98	5·02	4·40

years of age from 10 years to 80. The lack of variation seen in these figures seems to require that human life in London, from 1730-1800, should have been both homogeneous in itself and subject to homogeneous environment. The expectations of life are for males and females; in other words, in modern life tables, persons. The life table for persons which is most suitable for comparison is the Manchester life table for the years 1881-90. This city, which was then very unhealthy according to modern standards, shows at the age of 10 years six to eight years' greater expectation of life, the expectation becoming about equal when the age of 50 years was reached. The nearest approach to London in the eighteenth century is the table for males in the Manchester Township, which was the old town of Manchester, and much the unhealthiest part, but if the females are added in, the expectation for persons was higher in Manchester Township in 1881-90 than in London in 1730-1800. In the eighteenth century the population of London at the age of 20 years had an expectation of life about fifteen years less than at present, as is seen from the figures for London, 1911-12.

From the same data a table of death-rates has been constructed for the age-periods 10-20, 20-30, &c. (Table VI). Here, again, a singular constancy is noted from decade to decade. Comparing the death-rates with those in London for the years

1911-1912, it is evident that an immense improvement has taken place, the improvement being most marked between 30 and 50 years of age. Old figures for comparison

TABLE VI.—SHOWING THE DEATH-RATES AT AGE GROUPS FOR EACH DECADE IN LONDON FOR THE EIGHTEENTH CENTURY WITH SOME RECENT DATA FOR COMPARISON.

Age	London							London	Glasgow	Manchester Township
	1730-1739	1740-1749	1750-1759	1760-1769	1770-1779	1780-1789	1790-1799	1911-1912	1838-1844	1881-1890
10-20 ...	6·5 ...	5·9 ...	5·9 ...	7·1 ...	8·2 ...	7·1 ...	6·5 ...	— ...	7·5 ...	6·1
20-30 ...	17·5 ...	17·6 ...	17·2 ...	17·9 ...	17·3 ...	16·4 ...	15·1 ...	3·7 ...	10·0 ...	11·3
30-40 ...	27·1 ...	26·3 ...	26·1 ...	25·2 ...	25·0 ...	23·9 ...	23·7 ...	6·2 ...	15·3 ...	19·9
40-50 ...	36·6 ...	38·6 ...	35·9 ...	36·3 ...	35·5 ...	35·4 ...	34·3 ...	11·7 ...	23·6 ...	31·1
50-60 ...	45·5 ...	47·9 ...	45·7 ...	42·1 ...	45·0 ...	44·7 ...	44·6 ...	21·1 ...	34·7 ...	42·8
60-70 ...	61·4 ...	63·4 ...	63·7 ...	64·4 ...	62·8 ...	64·7 ...	64·1 ...	40·3 ...	61·5 ...	82·5
70-80 ...	83·7 ...	98·3 ...	96·4 ...	101·8 ...	105·0 ...	101·7 ...	104·4 ...	87·3 ...	136·0 ...	137·2
80-90 ...	143·1 ...	150·4 ...	151·0 ...	153·0 ...	152·7 ...	153·0 ...	155·1 ...	181·9 ...	249·4 ...	—

are scanty. Mr. Watt, the City Chamberlain, gives them for Glasgow for the years 1838-1844. It would seem that there must have been some different type of error in recording ages, more persons being recorded as dying at over 80 years of age in Glasgow than in London in the eighteenth century. The death-rates for Manchester Township are also given, these being smaller at the lower ages and greater at the higher ages. Taking the population as a whole, this conspectus gives some rough idea of the conditions of life in the eighteenth century.

PART II.—SPECIFIED INFECTIOUS DISEASES.

Preliminary.

We pass now to a consideration of the specific infectious diseases. I propose to use the weekly deaths from convulsions as a fairly safe guide to the variation in the mortality of children under 2 years of age during the year from decade to decade. I have had, therefore, taken out, for the twelve years 1728-40, the number of deaths

DIAGRAM I

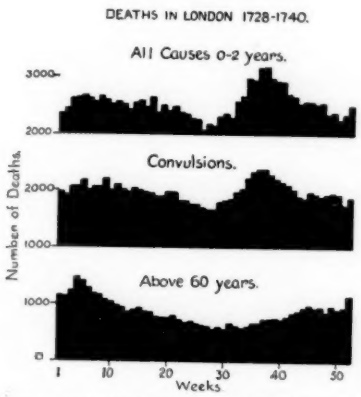


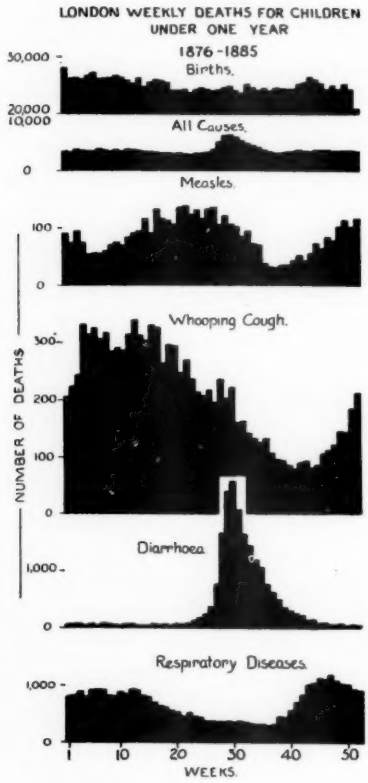
DIAGRAM I.—This diagram illustrates the seasonal prevalence of the diseases in children, from all causes below the age of 2 years and from convulsions. The seasonal distribution of deaths above 60 years is also shown.

in London from convulsions and the deaths from all causes under 2 years; the deaths above 60 years of age are given for comparison. These figures are graphed in Diagram I. It is seen that the course of the weekly variation in the number of deaths from convulsions and from all causes in children under 2 years of age is very much the same. It can be concluded, therefore, that if an increase in the number

of deaths from convulsions at any time is observed, there will be a corresponding increase in the number of deaths from all causes under 2 years. I hoped, on account of the seasonal prevalence of disease, that by considering the variation of the number of deaths throughout the year, to obtain some additional information of the course of the mortality from the infectious diseases of children. A diagram (Diagram II) illustrating the seasonal prevalence of disease in children has been prepared for the ten years 1876 to 1885, the year 1876 being the first year for which the Registrar-General published weekly deaths under 1 year of age.

The seasonal incidence of each disease is very different, but it will be observed

DIAGRAM II



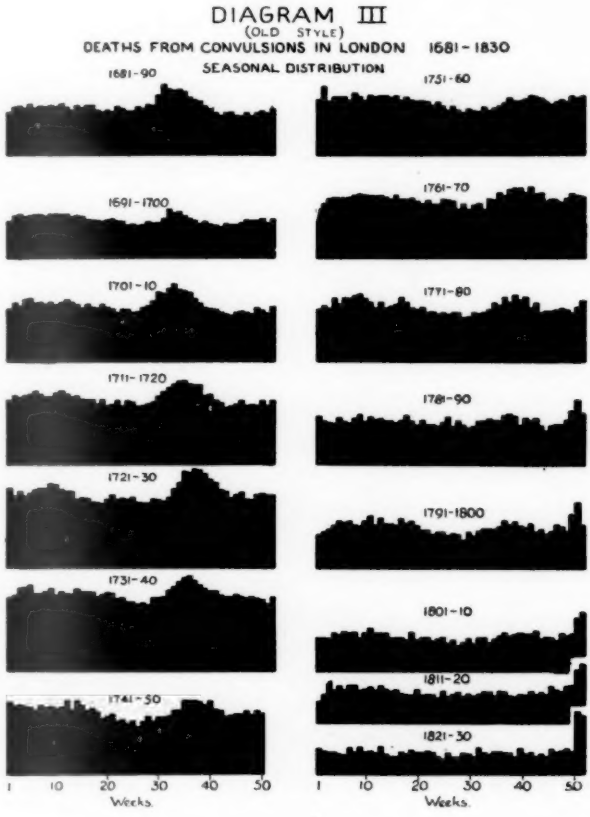
that when all these different causes of death under 1 year are added together, only one disease—summer diarrhœa—makes a permanent effect on the graph. I therefore had to abandon hope in this method and use it only as a clue to the epidemic course of summer diarrhœa. Some very surprising results have been obtained.

Thomas Sydenham, in his "Processus Integri in Morbis," writing *De Cholera Morbo*, begins:—

"Intra Augusti limites se continens, vix in priores Septembris hebdomadas evagatur."

"This disease, containing itself within the limits of August, scarcely wanders into the early weeks of September."

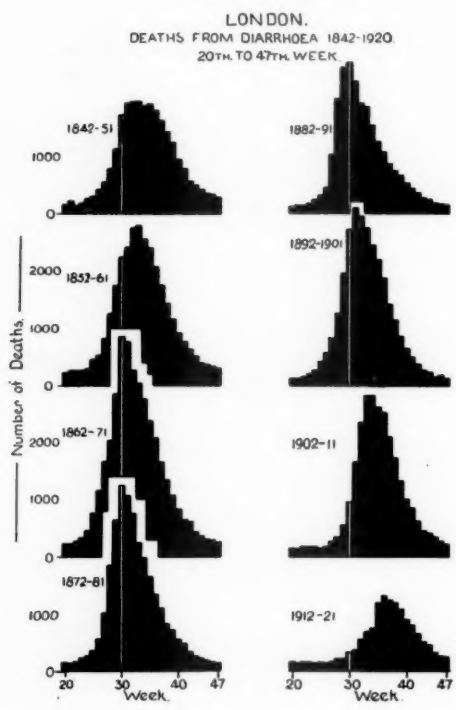
Sydenham died in 1689, and the "Processus Integri in Morbis" was first published in 1692. I therefore began my investigation of the weekly distribution of convulsions with the year 1681. The diagram (Diagram III) conforms to the Old Style from the beginning to the end, so that Sydenham's August is August 11 to September 10 in the New Style. In the ten years 1681-90 the rise of the curve is very marked in the thirtieth week, coming to a maximum in the thirty-first week and remaining at a high level until the thirty-fourth week, corresponding to the last week of July and the next four weeks of August, so that Sydenham's observation is



obviously correct. The figures for the next decade are not complete, the graph referring only to seven years, the records in the Guildhall being defective. The maximum of the epidemic has moved a week later. In the ten years 1701-10 it moved to the thirty-third week. From 1711-20 the maximum occurred in the thirty-fourth week, and in the following ten years reached the thirty-sixth week, and though it moved back between 1731 and 1740 to the thirty-fifth week, the progress into the autumn continued, and with these movements the number of deaths fell manifestly. When we come to the decade 1761-1770 there is an elevation about

the thirty-ninth and fortieth weeks. What this is due to is doubtful—possibly small-pox. There is no indication of any epidemic of summer diarrhœa from this decade until 1820. In the last decade, 1821-30, the number of deaths for each week of the year is practically constant. In the later decades, it will be noticed in the diagram that a great number of deaths apparently occur in the last two weeks of the year. This is due to the fact that certain parishes, growing careless, made a return at the end of the year of all the deaths in the parish in place of a weekly return, so that these deaths, instead of being given at their proper date in the weekly bills, are included *en masse* in the last returns of the year. The facts regarding summer diarrhœa in London since the introduction of registration are

DIAGRAM IV



very interesting in comparison with the phenomena just described. We do not know the amount of diarrhœa in London in the decade 1830-40. For the decades 1842-1921 the deaths have been graphed (Diagram IV). The diagram is not comparable as regards date with the previous diagram, as it is drawn in the New Style. For purposes of comparison as regards the date of the epidemic, a white line has been left in the middle of the graph relating to the thirtieth week. In 1842-51 the height of the epidemic was approximately the thirty-third week. In the next decade it moved back between the thirty-second and the thirty-third weeks. In 1862-71 the height of the epidemic was in the thirtieth and the thirty-first weeks, a phenomenon which persisted in the next decade, but the maximum moved still earlier

in the decade 1882-91. From this date there has been a continual advance of the height of the epidemic into the year, till, for the last decade, 1912-21, the height of the epidemic is in the thirty-sixth week of the year. The size of the epidemics has roughly corresponded with the season in which they occur, the later in the year the less marked the epidemics have become.

In our last paper Dr. Young and myself attributed this phenomenon to the fact that summer diarrhœa consisted in reality of two diseases, an earlier and later, and that the early form has disappeared. I still think that is probable, but there is now another explanation. The presence of the same phenomena in the eighteenth century does not negative it, but affords cause for doubt. We are too apt in epidemiology to theorize without any real basis. Take this case as an excellent example. The reason why summer diarrhœa is disappearing has been correlated with the introduction of motor-cars and the consequent diminution in the number of horses and the amount of stable manure, depriving the fly which is credited with carrying the virus of its favourite breeding ground. Here we have a similar phenomenon in the eighteenth century which cannot be correlated with motor-cars, unless it can be contended that the introduction of the sedan chair played the same part. Under conditions which were, at least to say, bad—conditions which, it might be argued, favour the spread of the disease—summer diarrhœa actually disappears. When the disease was re-introduced on a large scale I do not know. It perhaps was never completely absent, for Willan records the cases of cholera in his own practice as in the accompanying table:—

Year	Quarters			
	First	Second	Third	Fourth
1797	—	—	—	3
1798	—	—	3	3
1799	—	—	3	1
1800	—	1	33	4

The earlier years mean little, but there is a suggestion of something more serious in the year 1800, which was a specially hot summer. An attempt was made to associate hot summers with the number of deaths from convulsions in the third quarter of the year for the early part of the eighteenth century, without obtaining anything like a proved connexion. The weather data are very imperfect and are absent for some of the crucial years. It seemed, however, that the number of deaths was greatly increased by a series of hot summers.

Measles.

The measles statistics of London for the first half of the eighteenth century are in my opinion very inaccurate. After 1750 there seems to be some regularity in the ascription of measles as a cause of death. They present points of considerable interest, but before discussing these, I think their significance will be better understood if I proceed from the known to the unknown.

In the accompanying diagram (Diagram V), the seasonal distribution of measles in London is shown from 1841-1924. The deaths, recorded in each part of the diagram, are given in two-yearly periods, so that the "mountain" in the middle and the divided "mountain" at the end represent the deaths for December and January, the spring or summer epidemics being shown in between. The phenomena exhibited in this diagram are very curious. I have thought over the matter at intervals during the last ten years without arriving at any satisfactory explanation. It will be observed that in the first eighteen years of this period, the chief epidemics of measles were in the winter months. In the eighteen years beginning with 1859, there is, in one-half of the diagram, a very well-marked series of summer epidemics which, however, is almost completely absent in the other half. In the next eighteen years both summers of the two years' period show the presence of epidemics, and the same condition still holds. It will be noticed that with each succeeding period the summer

epidemics have moved back earlier in the year. The rate at which this movement has taken place is very slow, being only about 0·44 of a week in two years. When, however, the graph of the last ten years is observed, while the motion backwards is still evident, the winter epidemics will be seen to have completely disappeared. With this observation of what is taking place at present, the course of measles in London during the latter half of the eighteenth century may perhaps be better understood.

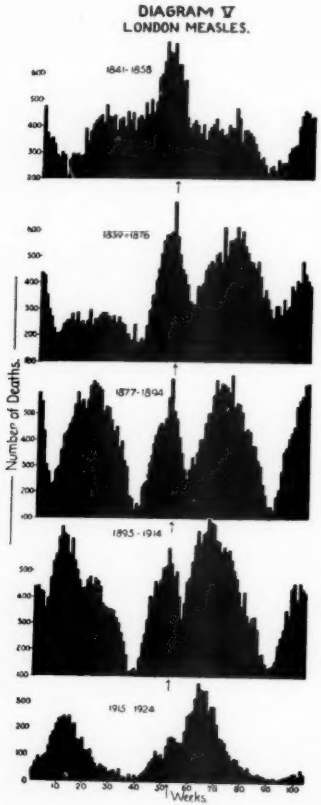


DIAGRAM V.—This diagram illustrates the seasonal distribution of measles in London from 1841-1924. The graphs are for two years, so that January corresponds to the beginning of every graph and December and January are easily marked by the epidemics. The beginning of the second year is marked by an arrow.

In Diagram VI the biennial distribution of deaths from measles is shown for London in three periods of approximately twenty-five years, 1755-1780, 1781-1804, 1805-1828. It will be observed that in the first period there is a summer epidemic in both summers of the biennia. In the second period the epidemic has disappeared in the first summer. When the figures are individually examined, this is apparently a true phenomenon, the figures showing the deaths in this summer disappearing about 1783. When the drift of the epidemic in the second summer is

measured, it is found again that this two-yearly epidemic is rather less than two years and has been moving to a maximum earlier in the year at approximately the same rate of 0·44 of a week in two years, which has already been found in the modern statistics. It is not, however, a continuous phenomenon, because the estimated rate of movement is not sufficient to make a direct passage from the epidemics of the eighteenth century to those of the nineteenth. Further, there is no evidence of the presence of epidemics in winter. These were practically absent in the eighteenth century. The same condition prevailing has been observed in London

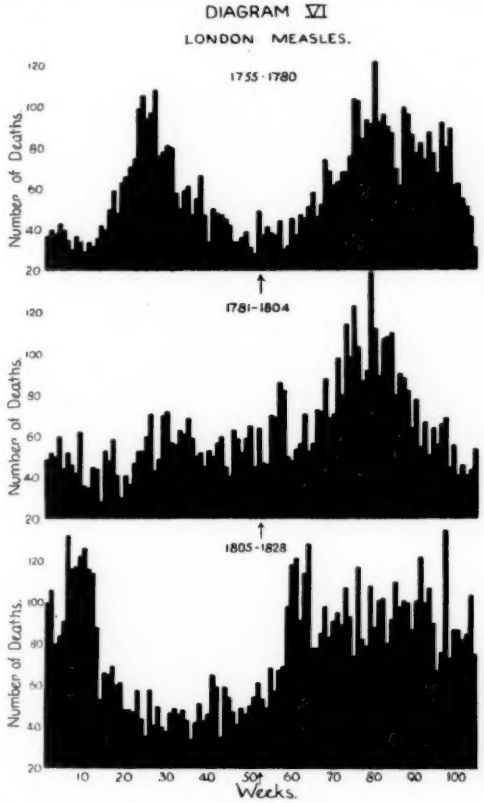


Diagram VI.—In this diagram the seasonal distribution of the measles epidemics is graphed in biennial periods.

from 1915-1924. In the last twenty-five years measles is continuously present during the second year of the biennium and the beginning of the first. It must thus be apparent that seasonal mortality from this disease depends upon factors very distinct from the ordinary variations of climate in the solar year.

Influenza.

During the eighteenth century there were four epidemics of influenza in London sufficiently large to allow of approximations as to the number of deaths due to the epidemics being made. These are shown in Table VII. The year is given in

TABLE VII.—SHOWING THE NUMBER OF DEATHS FROM INFLUENZA AT DIFFERENT AGE PERIODS FOR THE FOUR LARGE EPIDEMICS IN LONDON DURING THE EIGHTEENTH CENTURY, AND FOR COMPARISON, THE THREE EPIDEMICS IN LONDON, 1918-1919.

Year	Weeks of year	10-	20-	30-	40-	50-	60-	70-	80-	90.
1729	41-43	57	114	160	139	113	97	91	40	4
,,	44-46	85	222	284	291	293	236	167	82	20
,,	47-49	65	159	179	198	148	138	70	53	10
	Difference:	24	85	115	123	163	118	86	35	13
1733	2-3	33	80	121	137	90	103	67	73	15
,,	4-5	70	178	259	292	265	290	221	187	60
	Difference:	37	98	138	155	175	187	154	114	45
1743	11-13	48	111	130	112	113	113	80	38	3
,,	14-16	102	243	288	370	350	352	385	189	35
	Difference:	54	132	158	258	237	239	305	151	32
1788	21-23	35	62	113	101	73	66	53	11	2
,,	25-27	83	152	195	168	146	133	97	37	6
	Difference:	48	90	82	67	73	67	44	26	4
1918	June	1,473	2,243	1,932	1,491	1,204	1,007	602	179	—
1918	October	11,370	23,361	19,388	9,710	6,751	5,306	2,949	651	—
1919	Spring ..	2,961	7,944	7,307	4,737	4,476	4,307	2,912	887	—

the first column, the weeks of the year in which the disease was epidemic in the second column, and the number of deaths at each age-period above 10 years in the succeeding columns. The epidemic in 1729 occurred at the end of the year in the forty-fourth to the forty-sixth weeks. As at this season there is progressively increasing mortality due to cold, the numbers of deaths are given both for the three weeks before the epidemic and the three following. The subsequent epidemics of large size were in 1733, 1743 and 1788. In these cases the deaths in the weeks before the epidemics alone are given. Below these figures the numbers of deaths at the same age-periods are given for the three epidemics occurring in London in 1918 and 1919. Comparing these two sets it is at once evident that none of the eighteenth century epidemics shows any resemblance to the October epidemic in 1918. If, in the recent London epidemics, the number of deaths between 60 and 70 years of age is taken as the standard and compared with the number of deaths between 30 and 40 years of age, it is seen that in the epidemics of July, 1918, and the spring of 1919, the latter was barely twice the former, whereas in the October epidemic it approached to four times. Taking the epidemic of 1729, the number of deaths estimated between 60 and 70 years of age is about 140, and of those between 30 and 40 years about 180. In the epidemic of 1733, the figures for the same age periods are 190 and 138; in 1743, 239 and 158; and in 1788, the last large epidemic, 66 and 82. The smallness of the excess of deaths between 30 and 40 years over those between 60 and 70, is probably due to the fact that London had a much older population in the eighteenth century, the population being nearly stationary. It will be noticed that the 1788 epidemic, which occurred at an unusual period, the end of June and the beginning of July, was marked by many fewer deaths than in the winter or spring epidemics.

Phthisis.

The mortality from phthisis in London in the eighteenth century I have already dealt with elsewhere. That the proportionate mortality for that disease increased very markedly at the end of the eighteenth century is I think generally admitted, though the interpretation may be disputed. I think that there was a real increase, first, because the proportionate mortality rose very distinctly, and secondly, because in 1821, the first year for which data exist to enable an estimate of the mortality at different ages to be made, there was a very considerable excess of deaths at the ages at which deaths from phthisis commonly occur in London, namely, the middle ages of life. This is in agreement with what would be expected if there was an increase in the amount of phthisis, though it is not a complete proof.

Discussion.—Sir GEORGE BUCHANAN said he felt sure that, when the statistics and diagrams were reproduced and available for study, they could be very usefully considered in their relation in point of time to the political and social history of London. Reference had been made to the social conditions of London after the Napoleonic Wars, and their probable effect on tuberculosis prevalence and mortality. It would be instructive to consider other diseases from a similar point of view. An increase in diarrhœa mortality, for example, during the last century, might be found to have been associated with a considerable growth of horse traffic and the multiplication of stables, or perhaps with the increased establishment of food markets. Local historical facts should certainly be considered before these variations of causes of death over long periods of years were attributed to some intrinsic characteristic of the epidemiology of the disease, or to essential changes in the character, infectivity or virulence of the causative organism. It would be useful, also, to have some observations on the probability of the causes of death, as recorded by the searchers in past centuries, being reasonably comparable. All deaths put down to whooping-cough, for example, could hardly be supposed actually to be due to this disease, but their records would still be valuable if, over an interval of years, the rough practice of ascribing deaths of a certain class to whooping-cough remained the same. But fashions in nomenclature, which are not without their effect in the present day of more accurate diagnosis, might well have been quite as operative in the past.

Dr. A. K. CHALMERS said he was especially interested in what had been said regarding the emergence of diseases not formerly recognized, and wondered particularly if anything suggesting substitution was observable. Dr. Farr had at one time regarded this as likely, and Dr. Watt had added an appendix to his volume on "Chincough in Glasgow" to show that, while deaths among children from small-pox had undergone a very marked diminution following the introduction of vaccination towards the close of the eighteenth century, the total volume of deaths in childhood had remained fairly constant owing to the increased prevalence of measles, which had replaced small-pox, and for a time proved quite as fatal to child life. The other point present to his mind was the apparent recrudescence of phthisis in London about the same period. Was this in any way associated with the European War years, which had apparently led also to a recrudescence of typhus fever in this country?

Dr. MCVAIL (President) said he regarded Dr. Brownlee's paper as a contribution of great value relating to a period of special epidemiological interest. It was true that, with respect to differentiation between diseases, they had to depend on the "searchers"—women whose knowledge was very limited and who, as had been shown in a recent research into the Great Plague of 1666, were not above accepting a bribe to make a false return as to the cause of death. But allowance being made for this, the available material had to be used for what it was worth, and there was no doubt that it was worth a very great deal. The profession were indebted to Dr. Brownlee for his studies in the periodicity of epidemic disease, and the tables now submitted relating to that particular problem added to their debt. Reference had been made to the treatise on whooping cough, by Dr. Robert Watt, of Glasgow, who wrote early in the nineteenth century. Watt, looking to the remarkable diminution of small-pox as a result of vaccination in Glasgow, was astonished to find that measles and whooping-cough had greatly increased in the same period, and he speculated as to whether small-pox had not been a preventive of measles, and as to whether increase in the latter disease was not due to diminution in the former. In a sense, of course, that was so; the children who were saved from death by small-pox were liable to death by measles. Watt was a genuine searcher after truth, but his intelligence had its limitations, and, referring to his pathetic record of the death from whooping-cough of his own little boy, who would not, owing to a competition for a prize, stay away from school while under an attack of the disease, Dr. Jas. B. Russell remarked how Dr. Watt's knowledge of infection lay inert like a stone in the earth, instead of fructifying in efforts at segregation or isolation. Nevertheless, in the circumstances of the time, isolation as a general measure was a practical impossibility, and immunization was possible only in the case of one disease, namely, variola, while the rapid and utterly uncontrolled growth of Glasgow at the time in question favoured the increased mortality of the population from all sorts of diseases.

SECTION OF THE HISTORY OF MEDICINE.

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Section of the History of Medicine.

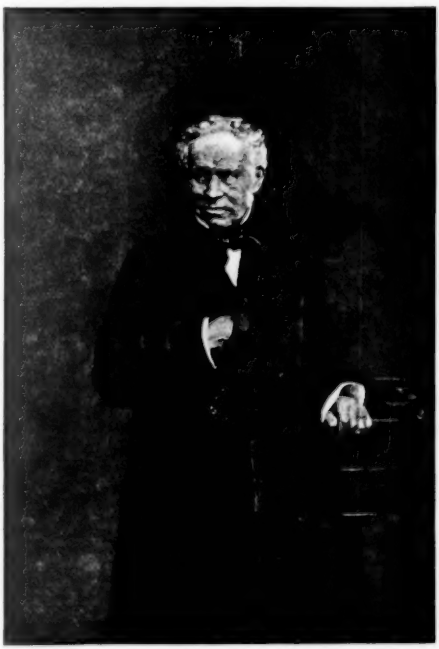
President—Dr. J. D. ROLLESTON.

Bretonneau: His Life and Work.

PRESIDENT'S ADDRESS.

By J. D. ROLLESTON, M.D.

WHEN the Council of this Section did me the honour of nominating me as President, I thought that I could not choose a more suitable subject for my address than the life and work of Bretonneau, for the following reasons: Apart from the fact that he was the first to establish the specificity of the two diseases, in which I have personally always taken the keenest interest, namely diphtheria



BRETONNEAU.
Portrait by Moreau-de-Tours.

and typhoid fever, Bretonneau's work admirably illustrates the practical value of the study of the history of medicine. Moreover, with the exception of the remarkable chapter on the history of diphtheria in the recent monograph issued by the Medical Research Council, little attention has of late been paid

in this country to Bretonneau, whose name is chiefly remembered in connexion with diphtheria without sufficient credit being paid to his other services to medicine.

It is no exaggeration to say that by his clinical perspicacity, in conjunction with elaborate post-mortem examinations, Bretonneau did as much for the study of acute infections as Laennec did for chronic diseases.

At the outset I should like to express my indebtedness to the late Dr. Paul Triaire's "Bretonneau et ses Correspondants," which, as Garrison truly says, forms the most interesting collection of medical letters since Guy Patin, as well as to the introduction by Dr. Louis Dubreuil-Chambardel to his edition of Bretonneau's posthumous work on typhoid fever and specificity.

Pierre Fidèle Bretonneau was born at St. Georges-sur-Cher, a village in the department of Indre-et-Cher, on April 3, 1778. Not only were his father and uncle surgeons, but his genealogical tree, which has been worked out by Dr. Louis Dubreuil-Chambardel, for nine generations contained fifteen doctors of medicine, surgeons or apothecaries, the most celebrated of whom was René Bretonnayau, who flourished in the middle of the sixteenth century at Beaulieu-lès-Loches, and was the author of a medical poem entitled "L'Ésculape François."

Far from being a precocious child, Bretonneau did not learn to read until he was nine years of age, and was not taught Latin, but his father seems to have inspired him in early life with a taste for natural history and medicine. In 1795, at the age of 17, he was sent by the department of Loir-et-Cher to study medicine at the École de Santé in Paris, where his fellow students included Dupuytren, Bayle, Récamier, Esquirol and Guersant. His interest in anatomical studies even at this early stage of his career is shown by a translation which he published in 1798, in collaboration with Saclier, of Mascagni's work on the lymphatics.

The hard work and privations of his life in Paris caused a breakdown in health and rendered his return home necessary before he had finished his three years' course of study. It was then that he was introduced by his father to Mme. Dupin, the châtelaine of Chenonceaux, once one of the most beautiful and wittiest women of the time, but now an old lady of 90. In spite of her advanced age she was in full possession of her faculties and appears to have taught Bretonneau not only Italian and English, but also the art of conversation, in which he subsequently excelled. In later years he frequently used to quote her aphorism, "Ce que nous savons souffre de ce que nous ne savons pas." After her death, in 1799, he returned to complete his medical studies in Paris, where he passed his first two examinations brilliantly, but failed to satisfy the truculent surgeon, Boyer, in botany, in spite of its being his favourite subject. He thereupon decided to abandon his examinations for the doctorate and to content himself with the modest title of *officier de santé*—a qualification now extinct—and returned to Chenonceaux, where he set up in practice, his spare time being devoted to the study of chemistry, natural history, gardening, the keeping of bees, of whose epizootics he acquired considerable knowledge; drawing and painting, in which, like Pasteur, he showed considerable skill. He made his own cataract needles, devised capillary tubes for vaccine lymph and amused himself by making thermometers and barometers for his friends.

His reputation as a practitioner rapidly grew. In the salon of M. de Villeneuve, who had inherited the château of Chenonceaux from his great-aunt, Mme. Dupin, he made the acquaintance of M. de Kergariou, the prefect of Indre-et-Loire, who invited him to come to Tours as principal physician to the hospital. For his post it was necessary to qualify as a doctor of medicine He therefore returned to Paris, passed the necessary examinations in 1814, having

rapidly acquired a knowledge of Latin, in which he remained a proficient scholar throughout his life, and in 1815 read his thesis, of which the subject was the value of compression, and especially of Theden's bandage in idiopathic inflammations of the skin. The same year, at the age of 37, he became "médecin-en-chef" of the hospital at Tours. His visits to the hospital occupied only six months of the year, namely, from November 1 to April 30, but during this period he showed an unprecedented activity. He arrived at the hospital at 6 a.m., made a careful examination of each patient, and dictated the notes that formed the enormous *dossiers* which, by the kindness of Dr. Dubreuil-Chambardel, I had the opportunity of seeing on a visit to Tours a few years ago. When a patient was suffering from a disease in which he was specially interested, he not infrequently passed whole hours at the bedside, trying to detect any changes in the general appearance, in the colour of the skin or in the respiration. But his work at the hospital was not entirely of a clinical nature. Deeply imbued with the importance of the study of morbid anatomy, like his contemporaries, Corvisart and Laennec, he spent much of his time in making autopsies, by which he was able to correlate his clinical observations with the post-mortem findings. Experimental work was also carried on by him at the hospital, especially at night, the patients not infrequently complaining that they were kept awake by the barking of the dogs which served as the subjects of his investigations. Lastly, a considerable part of his time was taken up in clinical teaching, for during the twenty-three years that he was physician at the hospital he was also in charge of a course of clinical medicine and materia medica. His instruction was not delivered in a professorial manner, but was carried on in a conversational and informal way, which did so much to endear him to his pupils, and particularly to the two who were to make known their master's doctrines in Paris long before he published his work on diphtheria. I refer to Velpeau and Trousseau, who became his internes in 1816 and 1824 respectively.

Before his appointment to the hospital at Tours Bretonneau had already had considerable experience of epidemic diseases in the form of two epidemics of typhoid fever at Chenonceaux in 1802 and 1812, occasional outbreaks of scarlet fever, measles and malignant angina in the valley of the Cher, and a severe epidemic of small-pox in 1814. His experience, however, had been purely clinical, as he had had no opportunities for performing autopsies. After his arrival in Tours he was able to study a third epidemic of typhoid fever, which broke out in one of the barracks at Tours in 1819, and caused a large number of victims both in the civilian and military population. Almost at the same time an epidemic of a peculiar form of sore throat was introduced by the soldiers of the Legion of La Vendée, and continued uninterruptedly until 1820. The epidemic was attended with a remarkably high mortality, so that in the course of a few months Bretonneau was able to carry out sixty autopsies.

In his study of the disease he received the help of Velpeau, who not only took an active part in the performance of autopsies, but was also of considerable assistance after he left Tours for Paris, where he made researches in the libraries and sent his master extracts from the old writers showing that the Syriac ulcer, malignant angina and croup were the same disease.

Bretonneau, who was never a *persona grata* with the medical profession at Tours, met with considerable opposition to his ideas, and some practitioners did not hesitate to maintain that the diseases observed by him at the hospital were quite distinct from those seen in private practice. In order to confute these assertions Bretonneau did not hesitate to play the part of a resurrectionist, and at night, accompanied by the faithful Velpeau, scaled the cemetery walls under fire from the neighbours, who mistook them for robbers, hastily dug up the bodies of the private patients who had died of diphtheria

or typhoid fever, and made an examination which was sufficient to prove the identity of the disease as seen in private practice with most of the hospital cases. Subsequently the opposition to Bretonneau in Tours reached such a pitch that this town, as Triaire remarks, was perhaps the only one in France in which the term "diphthérite" was never pronounced at the Medical Society in speaking of membranous sore throat, and it was one of the first to adopt the denomination "typhoid fever," introduced in 1829 by Louis, in place of Bretonneau's "dothienenteritis" which was henceforth expunged from the local nosology.

As the result of repeated entreaties from Velpeau, as well as from Duméril and Guersant, Bretonneau at last decided to communicate his researches to the Académie Royale de Médecine, before which he read two papers in 1821, the first on June 26, and the second on August 6. In the first he showed the relations existing between croup, malignant angina and scorbutic gangrene of the gums, all of which he regarded as the same affection, for which he proposed the term "diphthérite."

The second memoir consisted partly of historical evidence from the earlier writers and partly of a description of various therapeutical measures including alum, calomel, hydrochloric acid, blood-letting and tracheotomy, a detailed description being given of the operation on Elizabeth de Puységur, the first successful tracheotomy to be performed in a case of laryngeal diphtheria.

Bretonneau's communications were listened to with considerable interest by the members of the Académie de Médecine, who, as a token of their approval, elected him as a corresponding member in 1824. Apart, however, from a memoir in 1829, on the mode of propagation of dothienenteritis he made no further communications to the Académie de Médecine.

Although, as I have said, the two memoirs on diphtheria were read in 1821, five more years elapsed before their publication—the *Bulletin de l'Académie de Médecine* did not come into being until 1836—and it required the constant entreaties, reproaches, and threats of his friends, especially Velpeau and Trousseau, who undertook to correct the proofs, before Bretonneau could be induced to publish his researches in book form. The work on its appearance in 1826, in addition to the two memoirs read before the Académie, contained supplements on the symptoms and treatment of croup, a third memoir on an epidemic of diphtheria observed at La Ferrière and a fourth memoir on another epidemic at Chenusson. Extracts from the earlier writers such as Ghisi, Samuel Bard, Carnevale, Rosen von Rosenstein and others were appended, and were followed by a note on the use of alum in diphtheria. The work was illustrated by three plates showing: (1) A vertical section of the face and neck with figures of the tracheotomy tube; (2) the posterior aspect of the pharynx and trachea lined with diphtheritic membrane, and (3) a dissection of the anterior surface of the neck. The terms of publication were that Bretonneau was to receive sixty copies of the work and was to be allowed to choose 600 francs' worth of books from the publisher's shop.

Apart from the communication to the Académie de Médecine on the contagion of dothienenteritis already alluded to, and a memoir which appeared in the *Archives générales de Médecine*, in 1855, in the form of a letter addressed to Blache and Guersant entitled "Sur les moyens de prévenir le développement et le progrès de la diphthérie," Bretonneau did not publish any more works of importance during his lifetime. It was only in 1922 that his two posthumous works on dothienenteritis and specificity appeared under the editorship of Dr. Louis Dubreuil-Chambardel, of Tours, with a preface by Professor Gilbert, of Paris.

In 1841, as the result of the opposition he received from the local

practitioners, he resigned his position on the staff of the hospital and refused to take part in the formation of the new School of Medicine at Tours, in spite of the protests of Velpeau, Trousseau, and others.

He was thus able to devote more time to his large practice, much of which was gratuitous, and to what interested him still more, his garden at Palluau, near Tours, which like its owner possessed a European reputation, and formed a rendezvous for his friends, who consisted not only of medical celebrities such as Guersant, Lasègue, Cruveilhier and Bouillaud, but also distinguished literary and political personages such as Béranger, de Tocqueville, Lamennais, and the charming Mme. Récamier.

Few provincial practitioners have attained such a world-wide recognition in their lifetime, so that Bouillaud, in the address delivered at his funeral, remarked, as was once said of Boerhaave, that a letter addressed to Bretonneau, Europe, would have reached its destination.

In 1856 Bretonneau paid a visit to London, where, as we learn from Triaire, the welcome accorded him by the medical profession had almost the character of a triumphal progress.

He preserved his faculties almost intact until his death in 1862, at the age of 84. At his funeral, which was celebrated with great pomp at Tours, addresses were delivered by Bouillaud, President of the Académie de Médecine, Velpeau, representing the Académie des Sciences, and Trousseau, in the name of Bretonneau's former pupils. "Seldom," says the correspondent of the *Lancet* (1862, i, 531), "has a medical man been conveyed to his last resting place with such affectionate respect and deep regret!"

Bretonneau left no issue, although he was twice married. His first wife, who was the companion of Mme. Dupin, was a lady twenty-five years his senior, whilst his second wife was a young girl, aged 18, the niece of the celebrated alienist Moreau, of Tours, whom, in spite of the protests of his friends, he married at the age of 78.

Coquerelle remarks that everything in Bretonneau destined him for his revolutionary work. The most vivid description of his personality is to be obtained from the address which Velpeau delivered at his funeral.

"The individuality of M. Bretonneau," says Velpeau, "was exceedingly original, and his everyday life in no way resembled that of others. He worked and slept at all hours, was indifferent to heat and cold, and his regimen was altogether irregular. He ate and drank when he liked, at night as well as in the daytime, and quite regardless of domestic arrangements. Sometimes he would, without losing his balance, fall asleep whilst riding on horseback, and in the midst of conversation, or he would drop off while sitting at the bedside of a patient or in the midst of a sentence. As soon as he was awake he resumed the thread of his conversation, and his companions were sometimes not even aware that he had thus been absent. Everything he did was spontaneous, and he never troubled himself or others about the future, but when struck by an idea he took it up, and nothing afterwards could draw him away from it. If called to a patient he would tell his servant to ask if it was a case of sore throat or fever; and if it was neither to say his master was not at home. Again, when returning from the hospital he went into his garden, he immediately forgot all about his patients and thought of nothing but legumes, scions, grafts, saps and stocks. . . . He looked upon every patient as a problem which it was his duty to elucidate; and he came to see his patients at all hours, without being called, and at long or short intervals, according as he found it necessary. He sometimes remained for a long time at the bedside of a single patient, while to others he scarcely gave a minute. Being very absolute in his opinions and disliking useless discussions he submitted with great reluctance to the conventional claims of large consultations. Moreover, he often missed the hours of such meetings. On one occasion he kept three practitioners waiting for him at the house of a patient three-quarters of an hour; as, however, he did not appear, they all went away. Meanwhile M. Bretonneau had been exceedingly busy in the kitchen discussing

with the cook the composition of a certain dish. Bretonneau might have accumulated an immense fortune, but he cared for money just as little as for reputation. It did not matter to him whether his patients paid their fees or not, and he could never be prevailed upon to name a certain sum as a suitable reward for his services. His devotion to those dear to him was boundless, and being of a generous and tender disposition he felt happy or unhappy to excess according as those to whom he bore affection were glad or grieved. He once fainted away when a friend of his was operated on for empyema by M. Gouraud, and the same thing occurred to him at the mere sight of M. Roux's knife when about to operate on one of his relations. At the same time he practised the nicest operations on strangers without hesitation and with the firmness and skill of an accomplished surgeon."

Bretonneau was all the more fortunate in having such devoted friends as Velpeau and Trousseau, for, as Triaire points out, not only was the medical press still in its infancy, but the difficulties of communication offered almost insuperable barriers to the diffusion of new ideas, while on the other hand the modesty, disinterestedness and indolence of Bretonneau prevented his doctrines being made known outside his faithful band of pupils. After leaving Tours, Velpeau did so much to disseminate his master's teaching that within a few weeks after his arrival in Paris most of the principal physicians in the capital were familiar with Bretonneau's work on diphtheria and typhoid fever, although he had not published a single line.

But it was to Trousseau in particular that Bretonneau owed the dissemination of his doctrines. Not only, like Velpeau, did Trousseau communicate them orally to the medical world of Paris, but he constantly referred to his master in his contributions to periodical literature, as well as in his "Treatise on Therapeutics," written in collaboration with Pidoux, and his more celebrated "Clinical Lectures," which he dedicated to Bretonneau.

"The work of Bretonneau," says Professor Gilbert, "is a trilogy composed of diphtheria, dothienenteritis and specificity." In the beginning of the nineteenth century the whole subject of sore throat, including its different varieties and its relation to croup, was in a state of confusion, for which, it must be confessed, three well-known British writers, Home, Huxham and Fothergill, were to a large extent responsible. In his essay entitled "An Inquiry into the Nature, Cause and Cure of the Croup," published in 1765, Francis Home attempted to set up croup as a new and autonomous disease, distinct from malignant angina, whilst the condition described by Huxham and Fothergill, under the name of "malignant" or "ulcerous sore throat," seems to have included at least three distinct diseases, namely, scarlet fever, diphtheria and streptococcic ulcerative sore throat or pseudo-diphtheria. Nor did any of the numerous writers who competed for the prize for the best essay on croup offered by the French Royal Society of Medicine in 1783, and by Napoleon I in 1807, throw any light on this obscure subject.

Bretonneau's great achievement is to have shown the identity of malignant angina and croup, and to have differentiated malignant angina from all other forms of sore throat, particularly by its progressive course and contagious character. He proved moreover that in spite of the superficial appearances and the accompanying fœtor the nature of the process in malignant angina was not really gangrene, and even went so far as to deny the existence of gangrenous angina altogether, an error in which he was subsequently corrected by Trousseau. He also showed that two distinct diseases were described by Home, one of which was merely the laryngeal localization of the diphtheritic process, while the other was a mild affection, to which he gave the name of stridulous laryngitis.

The identification by Bretonneau of malignant angina with croup was established in four ways: (1) By extensive researches on the morbid anatomy of the disease, sixty autopsies being performed in the course of a few months; (2) by clinical observation, whereby he proved that the angina, if left to itself, almost invariably tended to attack the larynx, and thus to give rise to symptoms of croup; (3) by chemical examination, by which he proved the identity in composition of the false membrane in the pharynx with that in the trachea; (4) by the study of the works of the earlier writers. A considerable part of the second memoir on diphtheria is taken up by quotations from Aretaeus and the Spanish and Italian writers of the seventeenth century, as well as by more quotations from Ghisi, Starr and Bard in the eighteenth, showing that the disease commenced in the fauces and finally attacked the trachea.

Bretonneau furnished experimental proof of the specificity of diphtheria by the experiments on dogs to which I previously alluded. The application of extract of cantharides to the buccal, faucial, and conjunctival mucosæ produced a membrane closely resembling that of diphtheria, but was differentiated from it by there being an absence of any tendency to spread.

It was owing to the special character of the inflammation and the lack of any suitable term to describe it that Bretonneau invented the new word "diphthérite," derived from the Greek διφθέρα, meaning a skin or leather coating. The term was at once attacked by Boisseau on the ground that it must mean "inflammation of a skin," and that consequently the expression "diphtheritic inflammation," frequently used by Bretonneau, was tautologous. Bretonneau, however, who was a better Hellenist than his critic, pointed out in the supplement to the treatise on diphtheria—as was done more than sixty years later by Dr. Thomas Buzzard[1] (though even now it is not generally known)—that the termination "itis" did not really indicate inflammation, but was merely a feminine adjective often used as a substantive, the word νόσος being understood, so that "diphtheritis" merely meant the pellicular disease.

As early as 1825 Trousseau[2] suggested the substitution of the term "diphthérie," but it was not until thirty years later that Bretonneau accepted his suggestion, as is seen from his article in the *Archives générales de Médecine*, published in 1855, where the new spelling is adopted. After another thirty years the second "h" was omitted, so that the word is now invariably written "diphtérie." This change seems to have occurred about the year 1889, as in the first[3] of the three classical memoirs by Roux and Yersin the form "diphthérie" is used, but is replaced by "diphtérie" in the second and third memoirs published in the two following years.[4]

Epoch-making as Bretonneau's work on diphtheria undoubtedly is, we must not let ourselves ignore its defects, like the writer in the Medical Research monograph, who describes it as "a medical classic of the first rank, to which but little has been added from a purely clinical standpoint." Apart from its complete absence of unity of composition and frequency of repetitions, which made Trousseau[5] describe it as "the most inconceivable hotch-potch ever imagined by a writer," the work contains little, if any, indication that Bretonneau regarded diphtheria as a general disease affecting the heart, kidneys and nervous system. The only allusion, indeed, to diphtheritic paralysis in Bretonneau's writings is to be found in the letter to Blache and Guersant published in 1855, in which a description is given of the attack of generalized paralysis, from which

[1] *Trans. Path. Soc. Lond.*, 1889, xl, p. 347.
[2] Triaire, i, p. 549, lettre cix.
[3] *Ann. de l'Institut Pasteur*, 1888, ii, 629.
[4] *Ibid.*, 1889, iii, 273, 1890, iv, 385.
[5] Triaire, ii, p. 40, lettre cxxxvi.

Herpin, a well-known surgeon of Tours, suffered. It is the great merit of Trousseau to have supplemented his master's teaching in this respect and to have dwelt at length in his clinical lectures on the constitutional effects of diphtheria, in which he showed that death was not always due to asphyxia from invasion of the larynx as Bretonneau had taught, but was often caused by general intoxication. The symptoms of diphtheritic paralysis and the occurrence of albuminuria, which was first noted by Wade of Birmingham in 1858, are also fully described by Trousseau.

Bretonneau undoubtedly attached too great importance to the efficacy of fuming hydrochloric acid which was applied to the inflamed mucous membrane in the hope of substituting one inflammation for another. The extraordinarily painful character of the procedure and frequent aggravation of the symptoms soon caused this method to fall into a well-deserved oblivion.

Lastly, there appears to be little doubt that the so-called scorbutic gangrene of the gums, which Bretonneau regarded as diphtheritic, along with malignant angina and croup, was, as Bergeron has clearly shown, the ulcero-membranous stomatitis which has so often been the scourge of armies as well as of young children and is totally distinct from diphtheria, being due to the symbiosis of the fusiform bacillus and spirillum first described by Vincent in 1896.

Bretonneau's contribution to our knowledge of typhoid fever ranks only second in historical importance to that of his work on diphtheria. At the beginning of the nineteenth century the nosography of the continued fevers was in much the same chaotic condition as that of the different forms of sore throat. As in the case of diphtheria Bretonneau succeeded in establishing the specific character of the disease, to which he gave the name of "dothiénentérite" or "dothiénentérie"[1] (from *δοθίην* a pustule, and *ἔντερον* the intestine), not only by a clinical study for which he had ample opportunity owing to the epidemics in the garrison and civilian population of Tours in the years 1815, 1819 and 1820, but also by numerous and careful autopsies. His studies of the normal intestine, of which he gives a detailed description, were based on the examination of 300 bodies of men and animals. Perusal of the works of the earlier writers was not of such value in the study of typhoid as it was in the case of diphtheria, but Bretonneau maintains that the epidemic continued fevers of the years 1673-1675 in London observed by Sydenham, as well as the bilious fever described by Tissot at Lausanne in 1755 and by Sarcone at Naples in 1764, and the slow nervous fever reported by Stoll, at Vienna in 1777 and 1779, were examples of dothienenteritis. Bretonneau's work on typhoid fever, which was written between 1821 and 1827, but not actually published until 1922, was intended as a companion volume to the treatise on diphtheria, being entitled "Recherches sur les inflammations du tissu muqueux. Tome ii. Mémoire sur la dothinentérie ou inflammation exanthématique de la membrane muqueuse du canal intestinal." The work, which is preceded by a short introduction, is divided into four sections devoted to the anatomical lesions, the symptoms, the relation of the lesions to the symptoms, and the question of contagion respectively. In the first section the description of the intestinal lesions, which is based on 120 autopsies of persons who had died of the disease, includes a consideration of the features distinguishing the intestinal enanthem of dothienenteritis from tuberculous ulceration of the intestine, aphthous enteritis, dysentery, erythema of the gastro-intestinal mucosa and post-mortem changes which were often mistaken at that time for the effects

[1] The term is often incorrectly spelt "dothinentérie" probably owing to the erroneous derivation in an early paper by Trousseau (*Arch. Gén. de Méd.*, 1826, x, p. 67) who uses the fictitious word *δοθιωη*, though he gives the correct etymology and orthography in his clinical lectures published many years later.

of disease. In the second section Bretonneau describes the symptoms occurring in the first three weeks of dothienenteritis and draws a clinical picture to which little has been added since, even by Murchison. Although, strange to say, he makes no admission of the lenticular rose spots which were first described by Louis in 1829, he mentions the frequent occurrence of sudamina and the subsequent desquamation.

The third section opens with a review of the work of Bretonneau's immediate predecessors: Prost, the author of " La médecine éclairée par l'ouverture du corps " (1804), and Petit and Serres, whose "Traité de la fièvre entéro-mésentérique " was published in 1813. These observers had confined themselves to noting the presence of intestinal ulcers and enlarged glands, and regarded the ulceration of the intestine as the result of purgatives or as the evidence of syphilis or dartrous metastasis, whereas Bretonneau demonstrated that the exact situation of the lesions was in Peyer's patches, and maintained that the eruption on the intestinal mucosa was specific and went through all its stages like small-pox. Bretonneau then attacks Broussais's doctrine of gastro-enteritis, stating he had never been able to find a trace of gastritis in the numerous autopsies which he had performed on cases of dothienenteritis, whereas in true gastritis, such as that caused by ingestion of arsenic, none of the symptoms attributed by Broussais to gastro-enteritis, such as stupor, delirium, and ataxo-adynamia, were present. He then shows that ataxic fever and adynamic fever were not two distinct diseases as maintained by Petit and Serres but were only two forms of dothienenteritis, the former being usually an early condition, while the latter did not develop until after intestinal ulceration had taken place. In addition to a description of intestinal hæmorrhage and perforation the section also contains an account of other complications of typhoid fever, such as ulceration of the pharynx and larynx, otitis, ophthalmia and joint lesions, including dislocation of the femur. Bretonneau was thus about four years prior to Louis in his description of pharyngeal ulceration in typhoid fever, while his account of the articular complications preceded Keen's by over half a century. In the fourth section, which deals with contagion, Bretonneau maintains in opposition to several of his contemporaries, such as Andral, Bouillaud and Chomel, that dothienenteritis is a contagious disease, of which it presents all the essential features.

The doctrine of specificity, in which, as Professor Knud Faber has recently shown, he continued the work of Sydenham, forms Bretonneau's third most important contribution to medicine. Although he discussed the subject with Trousseau and his other pupils as early as 1818, Bretonneau published no work of this kind in his lifetime, and it was only two years ago that his treatise on specificity appeared in the incomplete form in which he had written it in 1828. The essence of this doctrine, however, had been made known to the medical world many years before by Trousseau in his remarkable clinical lecture on specificity published in 1861. Bretonneau's posthumous work, which was published in the same volume as the treatise on dothienenteritis, under the editorship of Dr. Louis Dubreuil-Chambardel in 1922, was originally intended to consist of four chapters devoted to the specificity of diphtheria, dothienenteritis, dysentery, and malaria respectively. Only the first chapter, however, was finished; the second is incomplete, and the last two were never prepared for the press, but exist only in a collection of rough notes and case reports. In his funeral oration Trousseau sums up Bretonneau's work on this subject as follows:—

" With a rare order of conception and a sort of intuitive genius Bretonneau wished to apply to the whole of pathology that which he had discovered concerning diphtheria and fever; he wished to prove the specific nature of all diseases, to show that if only

they were studied with sufficient care they could and should be ranged in distinct classes. His great knowledge of natural history led him to take the same road as that on which the illustrious Pinel had lost himself. Between Pinel and Bretonneau, however, there was this difference, that the former having once determined the nosological class, enclosed all subordinate species in a circle of iron and inflicted upon them a narrow and stereotyped treatment, while Bretonneau, when he had laid down his morbid species, did not think that this should at all regulate therapeutics. Bretonneau believed that each morbid seed caused a special disease, as every seed in natural history gives rise to a determined species, and as every animal or vegetable species has a distinct origin and development, in the same manner every species of disease should receive a specific treatment."

Bretonneau's first investigations on specificity, as Dr. Dubreuil-Chambardel has shown, had been made in the gardens of the Château de Chenonceaux, where he had studied the blistering action of the fluid secreted by mylabris insects, and had observed that each species produced a fluid of which the blistering effect was distinct. His investigations were described in a memoir presented to the Académie des Sciences in 1827, but never published, entitled, "Recherches sur les propriétés épispastiques et vénéneuses de plusieurs insectes de la famille des cantharides."

The doctrine of specificity is now so well established that it is one of the commonplaces of medicine, but it should be remembered that it is only comparatively recently that it has become so. Broussais, the leader of the so-called physiological school, was bitterly opposed to anything specific, and stigmatized as "ontologists" those who studied medicine from this point of view and attempted to draw up definite clinical pictures of each disease.

Broussais's teaching had an enormous vogue not only in his own country but also in Germany, where for many years, as Professor Faber has shown, the doctrine of specificity was disregarded, so that it was not until the researches of Pasteur had shown that certain diseases were due to distinct organisms that the doctrine of specificity was firmly established. Professor Hans Zinsser points out that as late as 1878, when Koch published his treatise on the ætiology of wound infection, specificity was not generally accepted, and the supposed metamorphosis of bacterial species had first to be scientifically refuted by Cohn, Koch and their pupils before it could be assumed that a given infectious disease was always the result of infection with a definite and constant species of bacteria.

Apart from the two diseases with which his name is most closely associated, Bretonneau took an active interest in all acute infections, so that Lasègue describes him as one of the greatest epidemiologists of our time.

Next to diphtheria and typhoid fever, malaria, from which he suffered himself, seems to have claimed Bretonneau's attention. Not only did he devise a new method of administration of quinine, which Trousseau characterizes as a happy combination of the methods of Sydenham and Torti, but he was also the first to describe the symptoms of quinine intoxication (Trousseau and Pidoux). In the treatment of dysentery, a serious outbreak of which occurred at Tours in 1823, he revived the method of purgation which had been used with such

success by Stoll, Zimmermann and Pringle, but had been replaced with dire results by bleeding and leeching.

Several allusions are to be found in Bretonneau's correspondence to the great epidemics of cholera which visited Europe in 1832, 1848 and 1853. Unlike most of his French contemporaries, Bretonneau vigorously upheld the view that cholera was a contagious disease, and protested against the practice of mixing cholera patients with the other cases in the Paris hospitals.

During the outbreak of yellow fever in 1828 at Gibraltar, which Trousseau was sent to investigate, Bretonneau maintained, in spite of much opposition, that in its nature, march and termination it differed from malaria, and seems to have anticipated its microbic origin.

As regards scarlet fever, during the first twenty-four years of his practice he never saw a single death from that disease, but in 1824 an epidemic attended with such a high mortality visited Tours that he came to regard it as no less deadly a disease than plague, typhus and cholera. The experience of Graves, at Dublin, as Trousseau points out, was similar.

In 1847, the year of a pandemic of influenza, he appears to have been making some investigations on that disease, judging from a letter sent him by Béranger, who exclaims: "If only you could find a vaccine for this disease!" (Triaire, vol. ii, p. 445.)

In conclusion, a few words may be said on Bretonneau's contributions to therapeutics. His work in this field is all the more important as, owing to the influence of Broussais, therapeutics had been reduced to antiphlogistic measures, in other words, bleeding, leeching and a starvation diet.

Apart from his being the first to employ tracheotomy in diphtheria, his treatment of dysentery by purgatives, and his method of administration of quinine in malaria, to which I have already alluded, Bretonneau is to be credited with the recommendation of cold affusions for scarlet fever, the resuscitation of martial preparations in anæmia, the use of belladonna in constipation and enuresis, the introduction into France of cod-liver oil for rickets and the local treatment of hæmorrhage by hot water, and of anal fissure by injections of rhatany. He was also a reformer in the matter of diet. Like Graves, between whom and Bretonneau, Lasègue has drawn an interesting parallel, he "fed fevers," instead of insisting on the starvation diet advocated by the Val-de-Grâce school.

To recapitulate: Bretonneau was a remarkable personality whose services to medicine may be summarized as follows: (1) He established the specific character of diphtheria and typhoid fever; (2) by his doctrine of specificity he foreshadowed the germ theory of disease; (3) by his reaction to the physiological school headed by Broussais he made several valuable contributions to therapeutics; (4) his researches, especially those connected with diphtheria, show the value of the study of the history of medicine.

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Section of the History of Medicine.

President—Dr. ARNOLD CHAPLIN.

The Scamnum, as described by Guido Guidi, illustrated by an Actual Specimen of the Sixteenth Century.

By C. J. S. THOMPSON, M.B.E.

THE story of the discovery of this ancient surgical appliance and how it came to be preserved and unrecognized for centuries in a monastic institution is not without a touch of romance.

A few months ago the table was found in the ancient monastery of Pennabilli, which stands in a wild mountainous region in Central Italy between Monte Montane and Monte Fumajolo, about sixteen miles from Urbino. For a considerable time it had been in use as a refectory table, and when discovered it was camouflaged under a flat top with drawers below on either side. A careful examination showed that the top and drawers were of a more recent period than the legs, and on the former being removed it was revealed in its present condition, proving beyond doubt that it was originally a luxation table.

On comparing this table with those described and depicted by various early writers on surgery from the tenth to the eighteenth century, I am led to believe that it is similar to that described by Oribasius, and first depicted in a manuscript by Nicétas, a Byzantine doctor of the tenth century, which is now in the Laurentian Library at Florence. It was afterwards described and represented in the translation of this manuscript by Guido Guidi, in Florence, about 1540. This table, therefore, may be said to be a direct descendant of the Scamnum of Hippocrates, and it is so called by Guido Guidi, who reproduces it in a wood-cut in the first edition of his "Chirurgia e Graeco in Latinum a se conversa," printed in Paris in 1544.

Our table (or bed as it is sometimes called) is constructed with a solid top of walnut 1½ in. thick. It measures 7 ft. long by 2 ft. 4 in. wide. The four corner-posts are fitted with iron cogs, controlling the winding bars which connect each pair of corner posts. These are controlled by pins which act as brakes that can be inserted when required. Down the centre of the top are eleven square holes, 2 in. wide by 1½ in. in depth. The top is solid and in one piece, and is supported by four turned legs with stretchers, the whole standing 2 ft. 4 in. high. At either end are iron staples, probably used as stanchions or to make the table secure and prevent it from slipping. It is obvious that the four corner posts have been cut down several inches so that the new top could rest upon them when the table was adapted for the purpose of the refectory. With the exception of the metal bars and handles at each end which I have added, the table was practically intact and in its original condition. It may be noted that the appliance described and depicted by Guido Guidi in his translation, according to the measurements he gives, was 10 ft. 10½ in. long.

Guido Guidi (Vidus Vidius) was born in Florence in the year 1500. Of his early days little is known, but he probably studied medicine and surgery in his native city, where he soon made a reputation as a keen anatomist and skilful surgeon. At the age of 42 he was appointed physician to Francis I and removed to Paris, where he became Professor of Medicine in the Collège de France. Of the six years

he spent in France we owe our knowledge to his friend Benvenuto Cellini, who described him in his autobiography as "the most cultivated, the most affectionate and the most companionable man of worth I ever knew in the world." While in Paris he lived with Cellini, who with typical generosity gave him a suite of apartments in his house and became godfather to his daughter. "We enjoyed our mutual friendship," he states, "during all the years I stayed in Paris, and often did we exult together on being able to advance in art and knowledge."

Cellini tells us in his autobiography that near the house in which he lived in Paris there were some little dwellings inhabited by different sorts of men, among whom was a printer of books of much excellence in his own trade. This was Petrus Galterius, who was the first to print Guido's "Chirurgia" published in 1544. This was the translation from Greek into Latin he had made of the collection in manuscript formed by Nicétas in the tenth century of the works of ancient Greek surgeons. This translation was probably undertaken by Guido Guidi at the instigation of Cardinal Nicolas Ridolfi, who acquired the Greek manuscript when it was brought from the East by Lascaris at the end of the fifteenth century. In the dedicatory epistle to Francis I with which he headed the Latin manuscript, Guidi says:—

". . . This talent for translating, Nicolaus Rodulphus, most distinguished of cardinals, not only my patron but my Maecenas, made more effective for me. For even as he is most diligent in collecting the best books of all kinds, he furnished me so freely with these Greek books that I should translate them in the first place, and then exhorted me to dedicate them to your name. . . ."

This collection consists of seven commentaries, including four by Galen, dealing with the works of Hippocrates, and two of the work of Oribasius in which are described the appliances and machines used by him.

Guido Guidi's manuscript in Latin, which is now No. 6866 in the Bibiothèque Nationale of Paris, is very beautifully illustrated with fine drawings by Santorinos Rhodius and le Primatice, two great artists of the time. In a passage of advice to the reader which precedes in the manuscript the translation of "De Machinamentis" by Oribasius, Guido Guidi alludes thus to the drawings, and gives the names of the artists by whom they were executed.

". . . In the interpretation of these things, not only to explain what should be stated, but that I might even set forth the very workings more clearly than by words, and should put them as if before your eyes, I studied both to paint and make the models. The amount of labour that I have undertaken and the extent to which I have exhausted myself has been witnessed by Joannes Santurineus Rhodius, besides several others, and my friend the Cardinal himself. Also there is the witness of Franciscus Primadicius Bononiensis, the most distinguished painter of the King of the Gauls, of whose works I have at times made use. . . ."

The artists to whom Guido Guidi attributes the fine drawings which accompany the translation were not content to reproduce the crudely executed drawings which they had before them in the manuscript belonging to Cardinal Ridolfi, but they interpreted them more artistically, while keeping in mind the design of the original, and elaborated in detail the models of the surgical appliances, doubtless made under the direction of Guido Guidi. Those who have examined the manuscript will agree with me that the drawings of these artists are remarkable not only for their accuracy in the delineation of the human form, but also for their artistic beauty.

After the death of Francis I in 1547, Guido Guidi returned to his native city, where he was appointed chief physician to Cosmo de Medici. The original manuscript probably remained in his hands, and was still in his possession at the death of Cardinal Ridolfi, which took place in 1550. Guido is said to have been one of the

instructors of Vesalius, and later on was appointed professor at Pisa, where he took Holy Orders. He died in that city on May 26, 1569.

From the fact that this table was discovered in a monastery within fifty miles of Florence, it is probable that it was made in that city in the time of Guido Guidi, and may have been one of the models constructed under his direction.

The Scamnum Hippocratis.

By Sir D'Arcy Power, K.B.E., F.R.C.S.

The Scamnum Hippocratis or Hippocratic bench is the apparatus used by our ancient brethren to reduce the fractures and dislocations of the lower limb which occurred so often in the wrestling matches of classical times.

The apparatus dates so far back in the history of surgery as to be antecedent to the use of the screw as a mechanical power. The lever, the crank, the windlass and the pulley are employed, but the screw is conspicuous by its absence. The bench was in use from classical times, and seems to have varied little, if at all, in its passage down the centuries. The description given by Vidus Vidius in 1544 is identical with that of the Hippocratic surgeon. But it was passing into disuse in the sixteenth century, for Vidius says he gives details about it because he once saw a distinguished professor at Padua put to shame before his class when he had to confess his ignorance as to how to use it. Scultetus in his "Armamentarium Chirurgicum," published in 1653, merely copies the figures and description of Vidius, and it is described *pro forma* in the "The Chyrurgeon's Store-House," published in 1674.

The original description of the Hippocratic surgeon runs:—

"It will be important for anyone who practises surgery in a populous city to have a quadrilateral board prepared, of about 6 cubits or a little more in length, and about 2 cubits in breadth; nine inches will be sufficiently thick. It should be hollowed out from one end to the other, so that the working of the levers may not be higher than is proper, and at both sides there shall be stout posts provided with axles, and the posts should be securely fixed. Five or six long grooves should also be scooped out, about four inches apart—three inches will be a sufficient width for each and the depth should be the same. And although this number of grooves is enough, there is nothing to prevent them being made all over the bench. And this scamnum or bench should have a pretty deep hole at its centre, square-shaped and about three inches in size. And into this hole, when it is thought to be necessary, a corresponding piece of wood with a rounded end is to be adjusted between the perinæum and the head of the thigh-bone. This upright piece of wood prevents the body yielding to the force dragging downwards from the feet. The piece of wood sometimes serves also to secure counter-extension upwards, and sometimes if it has some degree of movement from side to side it may be used as a lever to push the head of the bone outwards when extension and counter-extension have been made. This is the reason for scooping out several holes in the bench, so that this piece of wood, being erected in the most suitable one, may act as a lever either on the sides of the articular ends of the bone or to press directly on the head during extension, whilst the lever is used to press it inwards or outwards. The lever may be either round or flat, for sometimes one shape and sometimes the other is the better. This method of applying the lever whilst extension is being made is applicable to all forms of dislocated hip, and by means of such an apparatus and of such powers it appears to me that we need never fail to reduce any dislocation."

"Reduction by the wineskin is also favourably mentioned in the treatment of dislocations of the hip, and I have seen certain persons who, from ignorance, tried to reduce dislocations both outwards and backwards by its means, without recognizing that they were rather displacing than replacing the parts. It is clear, however, that he who first invented this method intended it for dislocation inwards. It is proper then to know how the wineskin should be used, if it is to be used at all, because it should be understood that there are many other

means which are more effectual. The skin should be placed between the thighs uninflated, so that it may be carried as far up into the perinæum as possible, and the thighs, beginning at the patella, are to be bound together with a bandage as far as the middle of the thigh, and then a brass pipe is to be introduced into one of the loose feet of the wineskin, and air forced into it, the patient lying on his side with the injured limb uppermost. But it is not a convenient method of applying force, for the wineskin when inflated does not present its most prominent part to the articular end of the femur (which is the place that ought to be more especially pressed outwards), but its middle, which probably corresponds with the middle of the thigh, or still lower down, for the thighs are naturally curved, being fleshy and in contact above, and becoming smaller below, so that the natural configuration of the wineskin forces it from its proper place. And if a small skin be used its power will be small, and will not be able to overcome the resistance of the dislocated bone."

Celsus (Lib. viii, cap. xx) says that, when other means have failed to reduce a dislocated hip,

"the limb may be yet more forcibly extended by placing the patient on the scamnum with pulleys on each side, to which the cords are to be tied. These cords can be tightened by twisting them like a winepress, though care must be taken in doing so not to tear the muscles or tendons. The patient should be placed upon the bench either prone, supine, or on his side, so that the dislocated limb may be uppermost, whilst the fixed portion of the joint is beneath."

Paulus Aegineta (Book vi, cap. 118) in like manner says:—

"Let the patient with a dislocated hip be stretched upon the scamnum like that upon which we stretch those who have dislocated their spine. And along nearly the whole length of the bench let some holes be made not more than three fingers' breadth in width and depth, and not more than four fingers' breadth apart from each other, so that the end of a lever being inserted into them, the limb may be pushed wherever it is necessary. In the middle of the scamnum let another piece of wood be fastened measuring about a foot long, and as thick as the crosspiece in the handle of a spade, so that when extension is made upon the patient this piece of wood comes between the perinæum and the upper part of the thigh. This prevents the body yielding when the feet are pulled upon, and thus the necessity for counter-extension is obviated, and at the same time when extension is made upon the body the piece of wood will push the head of the femur outwards. If the dislocation is not reduced by extension the upright piece of wood should be removed and two other pieces of wood fastened to the sides of it like posts, not more than a foot long. Another piece of wood should then be fitted to them like the step of a ladder, so that the figure made by the three pieces of wood may be like the Greek letter H, the middle piece being fixed a little below the upper ends. The patient being then laid upon the sound side, the uninjured leg is brought up between the two uprights and below the horizontal piece, which is like the step of a ladder, whilst the injured limb is brought above it so that the head of the femur is adapted to it; but a folded garment should first be wrapped round it to prevent bruising of the thigh. Another board of moderate breadth and of such a length as to reach from the top of the thigh to the ankle is then to be bound along the inner side of the thigh. Extension being made either by pegs or some other instrument, the leg is to be pulled downwards, together with the splint, which is fastened to it so that by the force used the head of the thigh-bone is replaced in its proper position."

Hippocrates and his successors also used the scamnum for straightening curvatures of the spine. He gives the following directions, which were transmitted to surgeons for many hundred years, although it is very doubtful whether they were really used. He says:—

"The bench is to be covered with clothing or anything else which is soft, and the patient is to be stoved with vapour if necessary, or bathed with much hot water, and then he is to be stretched along the scamnum on his face, with his arms laid alongside and bound to the body. The middle of a thong which is soft, sufficiently broad and long, and composed of two cross straps of leather, is to be carried twice round the middle of the patient's breast, as near the armpits as possible, and what is over of the thongs at the armpits is to be carried round the shoulders and afterwards the ends of the thong are to be fastened to a piece of wood resembling

a pestle. These pestles, or pegs, are to be adapted to the length of the bench on which the patient is lying in such a way that the wooden peg resting against the bench may make extension. Another such band is to be applied above the knees and ankles and the ends of the thongs fastened to a similar piece of wood. Another thong, broad, strong and like a bandage and of sufficient breadth and length is to be bound tightly round the loins as close as possible to the hips. What remains of this bandage, with the ends of the thongs, must be fastened to the piece of wood placed at the feet of the patient. Extension is then to be made upwards and downwards, equally and simultaneously, in a straight line. Extension made in this way can do no harm if it be performed properly, unless one purposely tried to do mischief. But the physician, or some person who is strong and not uninstructed, should apply the palm of the hand to the hump and then, putting one hand over the other, he should press upon it, noticing whether the pressure is made best directly downwards, upwards towards the head or towards the hips."

"This method of applying force is particularly safe. It is also safe for a person to sit upon the hump whilst extension is made, and after raising himself up to let himself fall down again upon the patient. And there is nothing to prevent a person from placing his foot upon the hump, supporting his weight upon it and making gentle pressure. A man who is practised in the palaestra would be a proper person to do this suitably."

"I once made trial of the following plan. Having placed the patient on his back, I put an empty wineskin below his hump and afterwards filled it with air by means of a brass pipe. But the experiment was not successful, for when the patient was fully extended the bladder yielded and air could not be forced into it. The hump of the patient, too, was apt to slip off the distended wineskin when they were pressed together. But when I did not extend the patient so strongly, the skin was swelled up by the air and the man became more bent forward than was proper. I have written this on purpose, for it is a valuable piece of knowledge to learn what has been tried ineffectually and to record the causes of failure."

Section of the History of Medicine.

President—Dr. J. D. ROLLESTON.

Herophilus of Alexandria.[1]

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ABOUT the end of the fourth century B.C., very shortly after the death of Aristotle, we come to two great names in the history of medicine—Herophilus, who may be called the founder of Systematic Anatomy, and Erasistratus, the first scientific physiologist. They both migrated from their homes in Asia Minor to Alexandria, attracted thither by the prospect of material advancement and the facilities for prosecuting their advanced studies which Ptolemaic Egypt offered to all eminent students. Their works are entirely lost, but some details of their teaching may be recovered from the voluminous writings of Galen, who possessed their books in entirety, and from scattered references in later writers. Of Erasistratus it must suffice for the moment to say that he came very near to discovering the circulation of the blood, and if he had started on the investigation of the arterial system with an open mind he must almost inevitably have reached the right conclusion. As it was, however, he was handicapped by the "pneumatic" theories of his predecessors and contemporaries, and could not shake off their influence. Herophilus is described by Galen as a follower of the "logical" or "dialectical" method, as opposed to mere empiricism. Galen sometimes accuses him of obscurity, but severer censure is generally reserved for his followers [3], who erred by misinterpreting their master's opinions. In the majority of cases he is classed by the historian among the great physicians of antiquity [1, 2], and commended for the soundness of his views, especially for the combination of observation with reasoning powers.

He took a deep interest in general anatomy, which formed the basis of his scientific application of gymnastic exercises to remedial purposes, and also of his dietetics [4]. His chief work was in connexion with the brain and the reproductive organs—the latter being of great practical importance owing to the interest taken by Greeks of this period in gynæcology—while he also wrote minor works on the eye and on the liver. He also had some reputation for the invention of a scientific vocabulary, parts of which remain in use to the present day.

It is certain that he made a careful study of Hippocrates; in fact, he wrote commentaries on the great master's *Prognostics* and *Aphorisms*, and made a collection of *glosses*, i.e., rare words used in his writings [4]. How far his study of the whole Hippocratic Corpus, and the work of his predecessors in general and particularly of Aristotle, had extended, is a matter for conjecture; but since Galen frequently represents him as accepting or rejecting one view or another, we assume that he was thoroughly familiar with the current literature of his subject.

BRAIN AND NERVES.

His work on the brain seems to have been important—the great thing being that, in opposition to Aristotle, he gave it precedence over the heart[2] as the centre of the nervous system. It is true that Alcmaeon had anticipated him (though what his argument was we do not know), but the Aristotelian tradition was definitely in favour of the heart being the seat of the intellect. "Herophilus," says Galen,

[1] The reference numbers in brackets in the text relate to the "Extracts" which follow the paper.

[2] He had on one occasion seen a heart exposed in consequence, perhaps, of a wound. The fact that the patient recovered may have inclined him to assign less importance to the heart [14].

"places the dominant principle of the 'soul' in the ventricles of the brain" [9]. This followed naturally from his belief that the brain is the centre of the nervous system [5]. "Since all the nerves below the head spring either from the hind-brain or the spinal cord, this ventricle must be of considerable size, and receive the animal spirits previously compounded in the anterior ventricles; so that there must necessarily be a passage from them to it. Therefore the latter also appears to be large, and the passage into it from the anterior ventricles is also very large, and through it only is there a connexion (emphysis) between the *cerebellum* and the *cerebrum*."[1]

He proceeds, in this context, to distinguish the *cerebrum* and *cerebellum* and to give an account of the passage connecting them, through which, he believes, the "animal spirits," which have their origin in the *cerebrum*, pass into the *cerebellum*. Galen further notes that his followers applied to the larger portion the general name for the whole.

It is to be regretted that we have not a fuller record of his investigations into the brain; we know, however, that he paid particular attention to the cavities, especially the one in the middle of the floor of the fourth ventricle, which he compared to the cavity in the pens used for writing in Alexandria [6]. The Latin translation of this term is *calamus scriptorius*. We must also mention here his description of the "*torcular*":—

"On the crown[2] of the head the doublings of the meninges meet, converging and conveying the blood to an empty space like a cistern," which he therefore called the "wine press" (Latin *torcular*) [7]. He also described the meninges, which he called *chorioid*, owing to their resemblance to the chorionic envelope surrounding the fœtus [18].

He described the *rete mirabile*, or "*retiform plexus*," as he calls it, at the base of the brain. He had probably found it in the brain of a sheep, as it does not exist in the human body. There is no reason to believe that he supposed it to exist in man also [8].

He had carefully dissected the eye and added much to current knowledge of its structure [13-16]. He traced the sensory nerves which lead from the brain to the eyes, which he called πόροι or passages; and since in mentioning this fact Galen refers to the optic chiasma, it is not unreasonable to assume that Herophilus had observed it [12].

Of the salivary glands [17] he distinguished the parotid and the submaxillary, though perhaps not the sublingual.

Galen charges Herophilus with obscurity in describing the functions of the nerves, but it is possible that Galen himself is responsible for some of the obscurity. He tells us that "Herophilus assigns the power of moving the body to veins, arteries and muscles, considering that the lung alone has a natural tendency to dilatation and contraction" [25]; but elsewhere we find that "He rightly recognized that it is the nervous, not the arterial system, which serves to produce voluntary motions"; but apparently he did not say clearly that the nerve is only the instrument, the moving cause being the power which passes through the nerves, a "power" which Galen himself defines as "soul," i.e., vital force, for all motion of muscles and nerves ceases when "soul" departs [24].

It is made quite clear that Herophilus was among the first to distinguish between motor and sensory nerves; previous observers had been at a loss to explain "how some kinds of paralysis destroy sensation only, others destroy only voluntary motions, while still others destroy both" [11]. Galen recognized that both for theoretical and practical purposes it was necessary, by means of dissection, to trace the two kinds of nerves back to their starting-points. This Herophilus had not done in all cases.

[1] The term here used by Herophilus was παρεγκεφαλίς, and it will be convenient to regard this as "hind-brain," a term already in use by the time of Galen, rather than *cerebellum*; for it is to the hind brain in general, and not any particular section of it, that these passages refer.

[2] He follows the terminology of Aristotle, who divided the skulls into bregma, crown and inion.

Liver.

His knowledge of the conformation of the liver was extensive. He applied to its study the methods of comparative anatomy, using in particular the hare for his illustrations. His general description is accurate; he observes that it differs in size and conformation in different individuals of the same species. He had observed cases, both in men and animals, where it occurred on the left instead of the right side [19].

We know little about his theory of the hepatic vascular system, except the following [23] :—

> Nature made veins specially belonging to the whole mesentery, which are dedicated to the nourishment of the intestines themselves and do not pass to the liver; for, as Herophilus said, these veins end in glandular bodies, while all others are carried back to the *portæ*; secondly, she also provided numerous vessels about the *omentum*.

These "veins" are undoubtedly *lacteals*, and to this passage I shall refer again later.

From another passage we gather that though he was in doubt as to the starting-point of some veins [20], he, like many others, began his investigations with the liver.

Lungs and Vascular System.

His treatment of the lungs is closely connected with the question of pulsation, which was to him of great importance.

> The lung (he says) has a natural tendency to dilatation and contraction, which it passes on to other parts; its functions in respiration are to receive the fresh air from outside and distribute it through the body; then to withdraw and finally to expel it; thus it has two motions of dilatation and two of contraction [25].

His knowledge of the pulmonary blood-system was not, so far as we can see, extensive: he called the pulmonary artery the arterial vein, asserting that in the lungs the veins are like arteries in general appearance and the arteries like veins. This statement he based on their comparative thickness, estimating that elsewhere the walls of an artery are six times as thick as those of a vein [27]. We may note that the term "*arterial vein*" remained in use till, and even after, the time of Harvey.

It is thus clear that though he did not fully understand their functions, he distinguished between arteries and veins, and in contrast to the majority of early anatomists, who maintained that the former were only air-passages, he firmly held to the opinion that the arteries contained blood [26].

The chief importance of the arteries was, for him, their connexion with pulsation. He appears to have been the first medical writer who made a systematic investigation of the pulse and recognized its importance as an aid to diagnosis [38]. Some writers had considered pulsation to be in part voluntary; he insisted that it was entirely involuntary, and was caused by the contraction and dilatation of the arteries [28]. Of these processes the contraction was an "energy" of the arteries, while the dilatation was their normal condition [34].

We must not be misled by the use of the word "energy" into thinking that he ascribed to the arteries a contractive power of their own. The "energy" or "activity" is simply a movement from the normal condition, but it is due not to any innate power in the arteries themselves but to an impulse received from the heart [31], which alone, according to Galen's representation of Herophilus, possesses the necessary motive power; in other words, Herophilus recognized, however vaguely, the importance of the heart as the centre of the blood system, and the connexion between the heart and the pulse-beats; but whereas as a matter of fact, as we know now, the contraction is the return of the elastic walls of the artery to their normal condition, and the dilatation is due to the pressure of blood from the heart, he went wrong

on this point, using the elasticity in the wrong direction, and possibly imagining that nerves extending from the heart to the arteries were responsible for the dilatation [31].

The origin of the mistake is clear; finding, on dissecting a subject, that the arteries were not collapsed, he mistook the normal (or minimum) distension for the maximum, and must have supposed that from this state, which is apparent in the dead creature, they had the faculty of contracting still further in order to produce pulsation.

Rufus of Ephesus makes the connexion still clearer, telling us that, according to Herophilus, "pulsation occurs in consequence of the filling and emptying of the arteries" [28]; and numerous passages in Galen show that Herophilus investigated and described various forms of involuntary motion, with results which are thus summarized: "Pulsation occurs only in the arteries and the heart; palpitation, spasm and tremor in the muscles and nerves" [28], [9].

Before the invention of any accurate machinery for recording time it was not possible to make careful observations of the rate of the pulse, since there was no means of standardization, though Herophilus is said on doubtful authority to have used the clepsydra or water-clock as an apparatus for his research [30]. His great effort was to ascertain and classify the varieties of rhythm, which he expressed in mathematical notation with such elaborateness that it was a current jest that a knowledge of music was indispensable to anyone who wished to understand his physiology.

In addition to normal pulses which follow natural rhythms he distinguished various divergent pulses—the pararrhythmic, the heterorrhythmic and the ecrhythmic, of which the first shows only a slight divergence from normality, the second a greater, and the last the greatest [36].

Normal pulses are classified according to the age of the subject; thus:—

> If a child ever has an interval of ten time-units between two beats, it is a sign of extreme refrigeration and indeed of a moribund condition, and on the other hand, if an old man has the pulse of a child, in which the times of dilatation and contraction are equal, this is a sign of a feverish condition [40].

The rhythm of a pulse is determined by observing and comparing the time taken in dilatation and contraction. Galen considers the results obtained by this method unsatisfactory and inconclusive, for:—

> Though he described the rhythms usually apparent in each age, he did not tell us in connexion with what natures he observed them; and secondly, it is evident that he is confused and incoherent about the diagnosis of the contraction and the periods of quiescence [37].

That is to say, Galen considered, first, that the nature of the pulse might vary in different classes of individuals of the same age, according, presumably, not to temporary conditions of health but to difference in permanent physical characteristics, and secondly, Galen assumed two periods of quiescence, one while the artery is full, another while it is empty, whereas Herophilus [35, 37] assumed constant reciprocal motions, reckoning as the period of dilatation the time when he could actually feel dilatation taking place as he measured the beats by pressure of the finger tips, and counting as the period of contraction the whole interval between the end of one beat and the beginning of another. Modern physiologists tell us that in this latter point Herophilus was right, for there is no period of quiescence such as Galen imagined.

His general classification of pulses was with reference to size, speed, strength and rhythm, in analysing which he took into account order and disorder, regularity and irregularity [39]. It is obvious that the latter four conditions do not pair with the previous characteristics, and he was sometimes accused of confusion; but his meaning is quite clear; any one of the first four might be, in a particular

case, either normal or abnormal; and what is orderly for one age may be "disorderly" for another, as has been shown above.

Many "cavillers," as Galen calls them, misrepresented Herophilus for contentious purposes. They were apt to pick out statements from his writings and, producing them apart from their context, characterize them as absurd; thus his opponents maintained that "children's pulse is small," while he, intending to compare the extent of the dilatation with the size of the vessels dilated, asserted the opposite. They refuse to recognize that the same amount of dilatation which is enormous, for instance, in an animal bladder, would be imperceptible in a balloon; and on these lines Galen fully justifies Herophilus [43].

The observations of the rhythm began with new-born children, in whom Herophilus claimed to observe that the times of dilatation and contraction were the same, and therefore the rhythm was described as "equal, or 1 : 1" [41].

At the other end of the scale would come the pulse of the very aged, in which, taking as a unit the time occupied by dilatation, the interval between two dilatations is represented by 10, i.e., the contraction takes ten times as long as the dilatation.

Galen doubts the accuracy of this observation, wrongly maintaining, as noted above, that we ought to take into account the possibility of periods of quiescence between the two activities. He also appears to doubt whether the period of contraction can be accurately observed [41].

We may record here two of his ingenious names for particular kinds of abnormal pulsation—the "crawling" or "formicating pulse"—*μυρμηκίζων*, from *μύρμηξ*, an ant; and the "capering" pulse (*δορκαδίζων*), with a supposed reference to the habits of the dorcas, a kind of gazelle. This latter is characterized by irregularities occurring in one dilatation, when the motion of the artery is at some point interrupted [44, 45].

Galen takes some trouble to clear away a misunderstanding which arose through the obscurity of some of Herophilus' language. Among the criteria for distinguishing different kinds of pulse he had mentioned "volume" or "multitude" (*πλῆθος*). Some of his followers, apparently, interpreted this as "fulness" (*πληρότης*); Galen maintains that he nowhere refers to a "full pulse" but that by volume he means "either frequency or speed or anything rather than fulness"; but in spite of Galen's efforts the passage still remains obscure [47].

Sexual Organs.

He seems to have contributed little to the knowledge, current since the time of Aristotle, of the male organs of generation. Rufus of Ephesus affirms that he could not explain the passage of the semen to the penis [48]; Galen, however, attributes to him the knowledge of its passage from the epididymis along the seminal ducts [49], which he called the "varicose parastates," perhaps limiting the application of the term to "those parts which touch the testes." He also distinguished two "glandular seminal bodies," i.e., *vesiculæ seminales* [50].

With the female organs he dealt more fully, embodying his views in a work which, though primarily intended as a practical treatise on midwifery (*μαιωτικόν*), seems to have approached the subject from a scientific standpoint. We find him first discussing whether women are subject to special pathological conditions, and very reasonably deciding that they are not, except in so far as the processes of conception, gestation, &c., differentiate them [53].

He was interested by the resemblances between the male and female anatomy. He distinguished the ovaries, which by analogy he calls "testicles," attached obliquely to the sides or shoulders of the uterus (the *cornua uteri*), not contained in a single scrotum, but separate. They are contained in a fine membrane (the broad ligament), and are small and rather flat, like glands. The flesh is hard, as in the case of males. They have a nerve-system adjacent to the covering. They are attached to the uterus by membranes (utero-ovarian ligaments), and each is connected with it by a vein and an artery. The seminal

ducts are not easily discerned, but they are attached to the sides of the uterus externally. The first part is twisted, just as in the case of the male, and practically all the rest, up to the end, is curled (varicose, κιρσοειδής) [51].

It is stated by McKay that Herophilus had not observed the Fallopian tubes, but in my opinion he must refer to them here. It has been suggested to me that he mistook for seminal ducts either the round ligaments or the ducts of Gärtner, but neither of these interpretations suits the passage just quoted. Taken by itself, it would naturally refer to the Fallopian tubes, and the description suits the appearance of these tubes in certain lower animals. His subsequent remark that "a varicose parastate has not been observed in the female," is repeated by Rufus, who says that he has observed it in dissecting the uterus of a sheep. It is just possible that by παραστάτης κιρσοειδής Herophilus and Rufus referred to the *plexus pampiniformis* in the male, which is very liable to become varicose.

The spermatic arteries and veins, all regarded as "vessels which nourish the uterus," are described with reasonable accuracy as being "inserted into the membranes" (i.e. the broad ligaments), and "starting from those vessels which lead to the kidneys"—in modern terms, the arteries arise from the *aorta* in the neighbourhood of the kidneys, the veins enter the *vena cava*, also near the kidneys.

He describes the uterus as resembling the bladder in general form, but it has "mastoid processes" at the sides [54], inclining towards the flanks; these are of semicircular shape. Diocles had called them "ceratid processes," and Aristotle simply "ceratia" (horns).

The cervix is described as muscular and cartilaginous,—increasingly so in the case of multiparæ, and he compares its form to the upper part of the throat [55].

"He did not hesitate to write," says Soranus, "that the mouth of the uterus would not admit even a probe before the woman has been delivered; that there is not the least aperture after conception has taken place, and that the opening is wider at the time of menstruation; after parturition it grows hard like the head of a cuttle fish" [57].

He considered that the fœtus had a physical but not a respiratory motion [59], and was nourished by the umbilical cord [60], of which he gives a correct account, except that he assigns to it four vessels (two veins and two arteries) instead of three. These carry material for blood and breath to the embryo. The veins connect with the *vena cava*, the arteries with the *great artery* which runs along the spine.

He made observations on the various relations of menstruation to general health [58], and gave an excellent summary of the causes of difficult labour [61]—first among which he considers displacement of the embryo and insufficient dilatationof the cervix.

He described the causes of *prolapsus*, rightly observing, as against other ancient authorities, that only the *cervix*, not the entire uterus, can protrude. He had observed an acute case in which, owing to the inflammation, the passing of a probe was impossible [62].

Terminology.

Herophilus, though expert in the use of terms, did not find and could not always invent names for his new discoveries, and it is probable that some of his work is obscure in consequence. Thus, although in general he has a quite definite idea of a vein as a blood-vessel, he seems also to have used it in another sense, namely, for what we call *lymphatics*—for these he had no special name; the *lacteals* are described as "*veins*" belonging to the mesentery, which nourish the intestines and do not pass into the liver, being contrasted with others (the hepatic set) which are carried back to the portal fissure [23]. These lacteals end in glandular bodies, i.e., the lymphatic glands.

In a continuation of the same passage he describes "numerous vessels about the omentum, particularly designed to give nourishment to all the adjacent parts." This description is too vague for certain identification; according to the standard of his knowledge it would fit either veins or arteries; the probability is that he referred to both.

A few words may be added about his contributions to terminology. A good deal of his vocabulary was taken from his predecessors, but he invented many names for parts hitherto undescribed, and in some cases endeavoured, though not always with success, to supersede the older nomenclature.

His use of "parastate" has already been mentioned; he applied the same term also to the hyoid bone, because it stands beside the tonsils [63]. He first used the expression "chorioid tunic" or "chorioid membrane" for the covering of the ventricles of the brain [18]. The third tunic of the eye was originally called by him "arachnoid" [16], but he compared it to a casting net, and called it "retiform," a word from which our modern "retina" is derived. The "*torcular*" and "*calamus scriptorius*" in the brain, and the "*duodenum*" are among his successes. His attempt to re-name the *tibia* the *pecten* did not meet with the approval of posterity [64]. The term was, however, in vogue during the Middle Ages.

VIVISECTION.

The charge of having practised vivisection on living men was in ancient times brought against Herophilus and his contemporary Erasistratus. The evidence on which it is based comes from Celsus (A.D. 30) and Tertullian (A.D. 155-222).

The former definitely states that the members of the Methodist School "think it is necessary to dissect dead bodies and examine their viscera and intestines; and they think that Herophilus and Erasistratus had taken by far the best method for attaining that knowledge, for they procured criminals out of prison by royal permission, and dissecting them alive contemplated, while they were even breathing, the parts which nature had before concealed" [65].

Celsus decides that such practices are both cruel and unnecessary; and from his further remarks in the same passage it is evident that in his time a certain section of medical opinion maintained that even dissection of the dead subject was as unnecessary as it was disgusting.

Tertullian, in the next century, repeats the charge, and though his language is not quite so definite, his meaning is clear according to the natural interpretation of the words. After calling Herophilus a "butcher," who dissected[1] six hundred persons that he might scrutinize nature—a passage which probably refers only to dissection of the dead—he continues, "death in his hands was not simply death, but led to error from the very process of cutting up" [66].

This, indeed, looks like a repetition of the empirical contention that, since death changes the appearance of the organs, all dissection is useless; but we may take it that Tertullian at least meant to imply, if he did not absolutely state, that the bodies cut up were living.

A second passage of the same author is also ambiguous. He reprobates the use of an instrument (ἐμβρυοφράκτης) for embryotomy, and says that it was used by Herophilus, "who dissected adults also"—implying, at any rate, that they, like the embryos, were alive [67].

It is strange, however, that Galen makes no reference to this supposed practice; and on this account many modern authorities have doubted the authenticity of the story. Galen was an open-minded writer who, on the whole, thought very highly of Herophilus, while he differed from and severely criticized Erasistratus in many details; but at the same time Galen pointed out the mistakes of the former and gave the latter full credit for those parts of his work of which he approved. If he had known of the practice of human vivisection by these authorities, it is inconceivable that he should not have referred to it either for praise or blame.

The silence of Galen about Herophilus leads us to wonder whether, after all, it was not only a charge trumped up by partisan writers who, like some people of modern times, so disapproved of dissection in any form that, consciously or unconsciously,

[1] In Latin of all periods "600" is used vaguely, as we use "hundreds," to denote a large number.

they confused dissection of the dead with dissection of the living. Erasistratus, however, is cleared of the charge by Galen himself, who in reference to the anatomy of the brain says:—

"If he had experimented with living animals, as I have done not once or twice but many times, he would have definitely recognized that the hard, thick membrane exists as a protection for the brain, &c." [67B].

Galen is here speaking of animals only, but if Erasistratus had never dissected living animals it is inconceivable that he should have dissected living men.

It was in anatomy that Herophilus was chiefly interested, and to which he contributed so much that, according to Galen, "no one made any advance on his method down to the time of Marinus and Numesianus" (writers of the second century) [68]; but this engrossing pursuit did not preclude a practical interest in various other branches of medical science. We may believe that in all his work he had practical ends in view, and he thought that any advance in knowledge of the body was a useful contribution to the science of teaching. His researches into pulsation were not academic exercises; we are definitely informed that they were undertaken for purposes of diagnosis. His work on *Dietetics* was evidently an important practical work; and at the same time he considered that drugs [69], of the use of which he made a careful study, were "the hands of gods" (*θεῶν χεῖρες*), and, if we may trust Celsus, would never treat any disease without medicines [70]. A few of his prescriptions have been preserved [71]. His conception of his mission is expressed in an aphorism attributed to him by Sextus Empiricus:—

"Wisdom is indemonstrable, art uncertain, strength powerless, wealth useless and speech impotent if health be absent," [78]

and it was the health of his fellow-creatures that he endeavoured by all his studies to promote.

EXTRACTS.

References to Galen are marked G and are given by volume and page to Kühn's edition.
Other references are to the following editions by pages:—
Chalcidius, edited by J. Wrobel, 1876.
Marcellinus, edited by H. Schöne in *Basler Festschrift*, 1907.
Rufus, edited by Daremberg and Ruelle, 1879.
Soranus, edited by V. Rose, 1882.

[1] G., x, 28; iii, 21; xiv, 683; v, 879, 898; xv, 134.

[2] G., i, 109.

[3] G., viii, 724; xviii, 13, 14.

[4] G., xix, 64, 65.

[5] G., iii, 665.

[6] G., ii, 731.

[7] G., iii, 708.

[8] G., iv, 155. The carotid arteries which mount to the brain, before they pass through the hard meninx (dura mater), are divided by it into many branches, twisting round in many layers, like a lot of nets lying one on top of the other; they occupy a considerable space, which they called the base of the brain.

[9] G., xix, 315; cf. Plut. *Epit.*, iv, 5; Tertullian, *De Anima*, c. xv, denies the truth of this.

[10] G., v, 181. "As Herophilus has said, perforation of the pericardiac tunic does not involve any special danger."

[11] G., viii, 212. When some part is wrenched its motor nerves or muscles must be affected. So a man who from dissection knows the starting-points of the nerves which lead to each part will be better able to cure loss of sensation or motion in each part. This was left undetermined by Herophilus and Eudemus, who were the first after Hippocrates to write accurately about the dissection of the nerves, and caused much difficulty to the doctors . . . as to how some kinds of paralysis destroy sensation only, others voluntary motion, and others both.

[12] G., iii, 813. In the case of the sensory nerves which lead from the brain to the eyes, called by Herophilus the ducts (*πόροι*), . . . it is remarkable that they originate in different places, but as they continue are united, and then again separate and are split up.

[13] Rufus, p. 171. The perforated body (iris) is smooth externally, where it touches the cornea, but rough on the interior surface, as Herophilus says, since, being formed of a tissue of vessels, this surface resembles the skin of a grape.

[14] Celsus, vii, 7, 13.

[15] Chalcidius, ch. 246, p. 279.

[16] Rufus, p. 154. The third (tunic of the eye) encloses the "vitreous humour"; its ancient name is "the arachnoid," and it is so called on account of its fineness; but since Herophilus compared it to a casting net some authorities call it "retiform."

[17] G., iv, 646. Besides these there is the secretion which comes from the brain through the palate, and the saliva from the glands near the root of the tongue, and the (contents) of the belly pass into the intestines, as well as the bile fluid from the liver, and again, from certain other glands assigned to this purpose comes a sticky fluid like saliva. Anatomists from the time of Herophilus and Eudemus have been much exercised about these glands.

[18] G., ii, 719. Herophilus and his followers name these the "chorioid masses," deriving the name from the *choria* which surround the fœtus externally and are layers of veins and arteries held together by delicate membranes. Cf. Rufus, ch. 55, p. 153, Dar.

[19] G., ii, 570. Herophilus describes it accurately as follows: "The human liver is large in comparison with that of some other animals comparable to man in size. Where it touches the diaphragm, it is convex and smooth, but where it touches the belly and the external curve of the belly, it is flattened and uneven. It is like a cleft at the point where in the case of embryos the vein from the navel grows into it. It is not alike in all [individuals] but varies in different subjects in breadth, length, thickness, height, number of lobes and unevenness in front, where it is thickest, and in its surrounding extremities . . . where it is thinnest. In some cases it has no lobes at all, but is altogether round and not straight at all; in others it has two or more—in many cases four." Herophilus was right in these statements, and the further assertion, in the second book of his "Anatomy," that in the case of a few men and a considerable number of other creatures, it occupies a position on the left, is also true. He referred only to the hare, and left it to us to investigate the case of other animals, which I have thought fit to describe in the preceding essay.

[20] G., v, 543. No anatomist who proposed to write of the dissection of the veins has been able to find a suitable starting-point for his treatise if he neglected the liver: whether they said that they were in doubt as to the starting-point as Herophilus did, or that they were fully informed, as did my friend Pelops, among others; they nearly all began their treatises with a discussion of the liver.

[21] G., ii, 780. From there (the portal fissure) grows a great vein which extends obliquely downwards and to the other parts of the animal, near the middle of what Herophilus called the duodenal process—this is the name which he gives to the beginning of the intestine, before it becomes twisted.

[22] G., viii, 396. The process which Herophilus calls the "*dodecadactyl*," naming it on account of its length.

[23] G., iii, 335.

[24] G., vii, 605. And here I blame Praxagoras and Herophilus, the former for calling tremor an affection of the arteries, and Herophilus for trying to show that it originates in the nervous system. Praxagoras is far from the truth, and Herophilus was mistaken in referring the affection of the faculty to the instruments. For he knew that the nervous system, not the arterial, is subordinate to voluntary motions; but since it is not the body of the nerve itself which causes their motion, but it (the body) is an instrument, and the moving cause is the power which passes through the nerve, here I blame him for not distinguishing power and instrument . . . now in the case of dead bodies, neither muscles nor nerves are subject to any such affections as Herophilus and Praxagoras suppose, but all their motion ceases when soul departs, and muscles and nerves are its instruments; so it is not the property of either muscle or nerve to produce motion, but of soul.

[25] G., xix, 318. Herophilus assigns the power of moving the body to nerves, arteries and muscles; he considers that the lung alone has a natural desire for dilatation and contraction, and next the others. The activity of the lung consists in drawing the breath from without; but when, being filled up from without it cannot draw any more into the thorax, it expels the surplus and drives it out to the outer air. Thus there are four motions of the lung, the first by which it receives air from without, the second by which the air thus received from without permeates within the lung, to the thorax; the third whereby it receives again into itself the air expelled by the contraction of the thorax, the fourth by which it

expels the air which has come again into itself; of these motions two are dilatations, two contractions.

[26] G., iv, 731. So that when they are at a loss how the breath is to be carried from the heart to the whole body *if the arteries are filled with blood*, it is not difficult to resolve their perplexity, by saying that it is not forced but is drawn, and not only from the heart but from all quarters, as was admitted by Herophilus and before him by Hippocrates and numerous others.

[27] Rufus, p. 162, D. Herophilus applies the term "arterial vein" to the very large and thick vessel leading from the heart to the lungs; for in the lungs conditions are the opposite of what they are elsewhere; the veins are there powerful and in nature very similar to arteries, while the arteries are weak and bear a close resemblance to veins. Cf. G., iii, 445.

[28] Rufus, p. 220, D. Herophilus, who had studied the subject more carefully, found the differences to be rather in quality, pulsation occurring only in the arteries and the heart, palpitation and spasm and tremor in the muscles and the nerves. The pulse, according to him, is born with the animal and dies with it; these other functions do not. Pulsation occurs in consequence of the *filling and emptying of the arteries;* not so the other motions. Again, pulsation is always involuntary, since it is a purely natural movement, but the others are voluntary, for often the parts are compressed or overcharged at will. Cf. G., vii, 594; viii, 717.

[29] G., viii, 498. Aegimius and others call any movement of the arteries palpitation. But the practice of Praxagoras and Herophilus holds good even to the present: they call every perceptible movement of the arteries a pulse. Cf. vii, 594.

Galen's own view is contained in xvi, 335. "Palpitation is an unnatural dilatation occurring in any part capable of dilatation. I say this because bones and cartilages never palpitate, since they are incapable of dilatation. . . . When it occurs in the arteries or heart, a second motion differing from pulsation is set up in them."

[30] Marcellinus, *De Pulsibus*, ch. xi.

[31] G., viii, 702. Some say that the arteries do pulsate, for their walls dilate and contract just like the heart, but they do this not by any innate power of their own, but receive it from the heart. Herophilus is of this opinion.

[32] G., viii, 744. To the statement of Chrysermus that the pulse is due to the vital and "animal" faculty, Heraclides added that the faculty must have exceptional energy, for Herophilus and the "Herophilii" as they are called after him said that other things contributed to the origin of the pulse.

[33] G., viii, 645. Herophilus says that the strength of the vital power in the artery is the cause of a vigorous pulse; Athenaeus says it is the strength of the vital tension.

[34] G., viii, 747. If he (Aristoxenus) followed the precepts of Herophilus exactly, the contraction is an energy of the arteries, while the dilatation is their return to the normal and natural bodily condition. For his meaning is that, just as in the case of the dead, the wall of the artery is distended, so in the case of the living it is distended as far as possible.

[35] G., ix, 463. Herophilus has written about time-units in combination with dilatation and contraction, reducing the ratio to rhythm corresponding to the various ages. For as the musicians[1] establish their rhythms according to certain definite arrangements of time-periods, comparing [the periods of] arsis and thesis, so Herophilus assumes the dilatation of the artery to be analogous to arsis and its contraction to thesis. He made his observations beginning with the new-born child, assuming as the first perceptible time-unit that in which he found that the artery dilated. He says that the time of contraction is equal to this, but is not quite definite in his statement about the intervals of quiescence between them. . . .

Ibid., 465. In so far as Herophilus says that the pulse of the rhythm is equal in the case of the lately born, it seems to me that he properly distinguished the principle of contraction; but in so far as he extends the contraction of the artery in the case of the aged to ten time-units, he no longer has a clear idea, but he recognizes the dilatation by the sensible motions, which we distinguish by feeling the beats against our fingers, and assumes contraction to take place during all the time when he did not perceive motion.

[36] G., ix, 470. By comparing the time of dilatation with that of contraction, as Herophilus claimed, it can be ascertained that the patient is in an abnormal condition, and further, whether the abnormality is serious or slight. For the great eversions of the natural rhythms to unnatural ones indicate that the injury is great, while lesser ones indicate that it is "slighter." The pararrhythmic pulses show a light eversion, the heterorrhythmic a greater one.

[1] Cf. Pliny, N. H., xi, xxxviii, § 88.

the ecrhythmic the greatest. For you must remember that in the first book on "Differences of Pulses" the term ecrhythmic was applied to pulses which have not the rhythm of any age; pararrhythmic to those which are nearly allied, heterorrhythmic to those which have the rhythm of a different age, not nearly related.

[37] G., ix, 278; ii, ch. 3. It is now the place for a discussion of rhythms, on which Herophilus has written at length, rather recording the results of observation and experience than teaching a rational system. He described the rhythms which generally occur at various ages, but, firstly, he does not tell us in what kinds of subjects he observed them; and, secondly, from his teaching it appears that he is confused and disconnected with regard to the diagnosis of contraction and quiescence. For if he thinks that contraction can occur in the case of the aged, extending up to ten time-units, it is clear that he has never perceived the true contraction: for this is sometimes of shorter duration than the dilatation, sometimes of equal duration, at other times, as he writes, of longer duration—though not, as he thinks, five times as long, but only a little longer.

[38] G., viii, 911. Herophilus himself often mentions pulses as a means of prognosis, but it is hard to discover what he means by rhythm. Is it only the ratio of the time of dilatation alone to that of contraction alone, or does he add the time of quiescence subsequent to each of these motions?

Ibid., 912-13. For our present purpose let us assume that rhythm consists of the proportion between the times of the motions, and seek at our convenience what is Herophilus' view.

[39] G., viii, 592. Herophilus classified the general differences of pulses as follows—size, speed, strength, rhythm; and not pairing properly with these, he mentions specifically order, disorder, regularity and irregularity; so he is accused by the cavillers of contrasting species with genus.

[40] G., ix, 499.

[41] G., viii, 786. Since Agathinus said that the contraction of the artery was imperceptible, while Herophilus continually speaks of it as being perceptible, it was difficult and distracting to have to believe one in preference to the other. . . .

[42] G., viii, 853. Perhaps even a child's pulse may be in excess of the normal. Herophilus sometimes calls this pulse considerable. Why then, you may ask, does Archigenes call it "little"? I can explain the reason for the discrepancy, if you first realize that we have established the principle that for the purpose of investigating the causes of pulses a reasonable man must know what is the normal one.

[43] G., viii, 871. They pick out merely the headings of Herophilus, disregarding all the rest, so that they are quite ignorant of what he really wrote.

So they say that children's pulse is small (though Herophilus states the opposite), without taking the trouble to examine and understand what people mean by a small or great dilatation. As a matter of fact we commonly speak of great or slight dilatations of bladders, bellows, bags, bellies, uteri, and anything else which contains a cavity. Often we might say that the belly is slightly distended, whereas if the bladder were distended to the same extent, we should call it a great distension. . . .

[44] G., ix, 453. Herophilus says that a child's pulse is "sufficiently great," Archigenes says it is little. Similarly Archigenes says that the "crawling" (*μυρμηκίζων*) pulse is fast, but Herophilus says it is not fast.

[45] G., viii, 556. The "capering" pulse as Herophilus calls it, is characterized by irregularities in one dilatation; it is a compound [rhythm] not connected with any of the irregularities which are attached to the first five classes of irregularities, but occurs particularly when the artery interrupts its motion in some part. It does not occur simply, for not all this class is called "capering," but when the second motion after quiescence is quicker and more violent than the first. Cf. Pseudo-Soranus, § 280. Rose, Anec. Græc. 1870.

[46] Rufus, p. 224, D. In the case of new-born children the pulse is very slight and we cannot distinguish between the contraction and dilatation. Herophilus says that this pulse is "irrational," by which he means a pulse which has no ratio to another, it has neither the ratio 1 : 2 nor 1 : 1½, nor any other (proportion), but is very slight and gives an impression like the prick of a needle; so that Herophilus was justified in first calling this pulse "irrational."

[47] G., viii, 955. For the benefit of all who wish to learn ancient history, and have time for it, I will state my case fully, and will prove that Herophilus never for any purpose makes use of the "full pulse." I shall begin with the statement which they adduce in the first book

of the commentary on Herophilus *On Pulsations*, which I imagine is the only book they have read. It is as follows: "In general, one pulse seems to me to differ from another in volume, speed, strength and rhythm." Adducing this passage, they ask, "What is volume?" As if we did not know what he meant by it, and maintaining that he must be referring to *fulness*. Well, I can answer them neatly, I think. I maintain that by volume he means frequency. Or, again, I would answer others that by volume he means speed, or anything rather than fulness. Cf., pp. 929, 959.

[48] Rufus, p. 67.

[49] (*a*) G., iv, 565. However, the seminal duct, which some call the varicose parastate, draws the semen from it (the epididymis) and carries it up to the base of the penis. It was on this account, I suppose, that Herophilus thought it had nothing much to do with the production of the semen.

(*b*) *Ibid.*, 582. . . . The seminal vessel, in which Herophilus called the part near the duct the varicose parastate.

[50] Rufus, pp. 158-59. The seminal vessels are four in number, two varicose (vasa deferentia) and two glandular (vesiculæ seminales); they used to be called also the generative glands. Of the varicose, the parts which touch the testes are the *parastates*, though some do not hesitate to call the whole of them parastates. We must consider whether the same formations exist in the female as in the male, for Herophilus considers that the female does not possess the varicose parastates; . . . but we have seen in the uterus of a sheep varicose vessels on either side springing from the testes. These opened into the cavity of the uterus, and mucous fluid escaped from them when pressed.

[51] G., iv, 596. Herophilus says that the "semen of the female is in a sense emitted" (*οὐκ οἶδ' ὅπως ἐκχεῖσθαι*), though he gave a correct account of the testicles in the third book of his "Anatomy." These are his words at the beginning of the book: "Two testicles are attached to the sides of the uterus, one on each side, very little differing from those of the male." Then not long after in a subsequent chapter he writes, "Females have two (testicles) attached to the 'shoulders' of the uterus (cornua uteri), one on the right and one on the left, not contained in a single scrotum but both being separate. They are contained in a fine membranous membrane, and are small and rather flat, like glands. They have a nerve system adjacent to the covering which surrounds them, and the flesh is hard, as in the case of males. They are of great size in the case of mares. They are attached by numerous membranes to the uterus, and by a vein and an artery which pass into them from the uterus. For they are attached by the vein and artery which passes to each of the two [testicles], vein being attached to vein and artery to artery. The seminal ducts are not very apparent, but they are attached to the uterus externally, one on the right and one on the left. The first part is twisted, just as in the case of the male, and practically all the rest, up to the end, is curled (*κιρσοειδής*, 'varicose')."

"Ducts from both the testes are attached, as in the case of the male, to the fleshy part of the neck of the bladder; they are fine and are crooked in the fore part, by which they touch the bones of the hips, at which they terminate, passing from either side into the interior of the vagina. But the varicose parastate has not been observed in the female."

[52] G., ii, 895. There are four other vessels which are found in some women, but not all, according to Herophilus. They start from (*ἀποφυόμενα*) those which lead to the kidneys, and enter the uterus. I have not found this in other animals except rarely in the case of apes; but I do not doubt that Herophilus often found it in the case of women; for in addition to a sound general knowledge of his art, he possessed a particularly accurate knowledge of the results of dissection, which he obtained not, as most people do, by experiments on brute animals but on human subjects. He also tells us that the vessels which nourish the uterus are inserted into the membranes from which we say that the bladder depends, and that these membranes always grow thicker and harder and more callous in the case of *multiparæ*.

[53] Soranus, p. 300. Some suppose that women have special pathological conditions, others, e.g., Erasistratus and Herophilus, maintain the contrary . . . there are seven primary and simple pathological conditions, and so no special one can be assumed in the case of women. Herophilus in his "Midwifery" says that the womb is constructed of the same tissue as the other parts and controlled by the same powers and has the same substances near it; and a diseased condition in it is due to the same causes. Therefore there is no special pathological condition in women except conception, gestation, parturition, lactation, and their opposites.

[54] G., ii, 890. As to the form of the uterus, in general and particularly in its base, it

resembles the bladder but is dissimilar to it in having "mastoid processes" at the sides inclining towards the flanks. Herophilus compares their shape to the curve of a semicircle, Diocles to growing horns. On this account he called them the "ceratid" processes.

[55] G., ii, 897.

[56] G., iii, 209.

[57] Soranus, p. 178. After parturition the *os uteri* becomes more callous, like the head of a cuttle-fish, or, as Herophilus says, it resembles the top of the trachea, being hardened by the passage of the substances discharged and brought to birth.

[58] Soranus, p. 192. Herophilus says that sometimes and for some women the purgation is harmful, for some enjoy uninterrupted health so long as they are free from the purgation, and often on the other hand in the process of purgation they grow paler and thinner and liable to pathological disturbances; but sometimes and in some cases it is beneficial, so that being formerly pale and ill-nourished they subsequently gain colour and are well-nourished.

[59] G., xix, 330.

[60] Soranus, 225. The umbilical cord is composed of four vessels, two veins and two arteries, by which material for blood and breath is conveyed to the embryo. Empedocles thinks it grows into the liver, Phaedrus into the heart; but most people think that the veins grow into the liver and the arteries into the heart. Herophilus thinks the veins grow into the hollow vein and the arteries into the great artery which runs along the spine, and before they join this they go obliquely alongside the bladder on both sides.

[61] Soranus, p. 349. Herophilus in his "Midwifery" says that difficult labour is also due to frequent parturition.

It is due to the embryo being in a slanting position or to the insufficient dilatation of the cervix or os uteri, or to the fact of the membrane containing the embryo, in which the waters collect, being too thick, so that it cannot burst before birth. Embryos, he says, have been seen to be born without the membrane bursting. In such cases labour is difficult; and difficult labour is also due to slackness of the uterus or the body.[1] Barrenness is due to the displacement of the uterus in the body. Difficult labour also occurs owing to external influences and occurrences and actions and to the discharge of excessive quantities of blood and fluid from the body. Difficulty in parturition also occurs owing to special distension of the womb by the embryo or owing to a chill or fever, or a tumour or abscess in the intestines or in the stomach. Cavity[2] occurring in the loins and spine is a cause of difficulty of labour, which may also be due to a fatty condition in the stomach or ischial region which causes pressure on the uterus; or to the death of the embryo. So far Herophilus.

[62] Soranus, p. 372. Some say that the whole uterus suffers prolapsus, when the membranes and muscles supporting it are ruptured by a blow or something of the kind, or relaxed and as it were paralysed; but the schools of Hippocrates and Herophilus maintain that it is only the cervix. The hardened body is found to prolapse from the soft parts, resembling the head of a cuttle-fish, as Herophilus said, and being scarcely capable of admitting a probe.

[63] Rufus, p. 155.

[64] *Ibid.*, p. 149.

[65] Celsus, I, *pref.*

[66] Tert., *De Anima*, c. 10. "Herophilus, the physician, or rather butcher [*aut lanius*], dissected six hundred persons that he might scrutinize nature: he hated man that he might gain knowledge. I know not whether he explored, clearly, all the internal parts of man, for death itself would change them from their state when alive, and death in his hands was not simply death, but led to error from the very process of cutting up."

[67] *Ibid.*, c. 25.

[67B] G., v, 603.

[68] G., xviii, 136.

[69] G., xii, 966. By persuading them to use the drugs (which they had been afraid to use) I proved to them and to you the truth of both sayings of Herophilus: for if you say that drugs by themselves are no use, you will say rightly; for they are of no use unless they find someone to use them properly; and again, you will be right if you say they are like the "hands of the gods"; for they are of great help if the person using them has been trained in the logical method in addition to being himself intelligent.

[1] The abdominal muscles.

[2] Cavity, &c., possibly refers to *spina bifida* in the fœtus.

[70] Celsus, V, *pref.* The ancient physicians attached much importance to these remedies... particularly Herophilus and his successors; so much so that they never treated any disease without medicines. They have also written a great deal about the properties of medicines. Cf. G., xi, 795; Pliny, N. H., xxvi, ii, § 6; Celsus, *T*, pref.

[71] e.g., G. xii, 843; xiii, 79, 308; xiv, 444.

[72] G., x, 184. Herophilus and Erasistratus are not successful in treating ulcers.

[73] G., v, 685. So not only Plato, but Aristotle, Theophrastus and the other disciples of Plato and Aristotle who have followed the theory of Hippocrates about the juices, and also the most notable of the ancient physicians, Diocles . . . and Herophilus. Cf. xviii, A, 187.

[74] G., xiv, 688. Herophilus defines medicine as a knowledge of healthy, morbid and neutral conditions; for it implies a knowledge of these three—of healthy, namely, all that induce this condition in the human subject, from the proper harmonization of which health results; of morbid, which dissolve the health-giving harmony; in the class of neutrals are all the helps which are prescribed in cases of sickness, and their material; for these, before they are employed by the physician are neutral, i.e., neither healthy nor morbid.

[75] Plut., *Epit.*, v, 29. Herophilus says that some people at times become feverish from no antecedent cause.

[76] G., xix, 321. Herophilus thinks that some dreams are inspired by God and come to us inevitably, while others are physical, arising when the soul pictures things which will be useful, or events in the future; others are of a combined nature, arising naturally. The occurrence of [mental] pictures, when we see what we want to see, as happens in the case of lovers who embrace their mistresses in dreams.

[77] Plut., *Plac.* i, 23, 6. Herophilus says that, of motion, some is cognizable by reason, other apprehensible by sense.

[78] Sext. Emp., *Eth.* xi, 50.

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Section of Medicine.

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Metabolism and the Vegetative System.

By Erich Leschke, M.D.

(*Professor of Medicine of the University of Berlin.*)

(1) Historical Introduction.

Since Claude Bernard's puncture of the medulla oblongata and Eckhardt and Kahler's researches on experimental polyuria, the existence of relations between the sympathetic nervous system and metabolism has been established beyond doubt. But clinical medicine has derived little profit from these experimental data. Kahler endeavoured in vain to locate the causes of diabetes insipidus in the medulla oblongata; others have been equally unsuccessful in connexion with diabetes mellitus. With the exception of a few rather doubtful cases lesions of the sympathetic centres in the brain-stem have not been found to be definite pathogenic factors in the causation of metabolic disturbances.

Since then, up to the present time, the whole interests of clinicians, pathologists and physiologists have been absorbed in the study of the well-marked and fundamental influence of the endocrine glands upon metabolism. When I began my experimental and clinical work on the hypothalamic centres of the autonomic nervous system and their influence on metabolism it was heresy to suppose that such troubles as diabetes insipidus, dystrophia adiposo-genitalis, forms of glycosuria, &c., could be of nervous rather than of glandular origin, yet in 1920 when I tried to establish the theory of a clinical pathology of the diencephalon I had to undergo the severest criticism at the hands of most of our leading endocrinologists.

The progress of experiment and of clinical experience, however, has shown us during the last few years that we are justified in following the line of research inaugurated by Claude Bernard, but at a higher level, in the interbrain or diencephalon instead of in the medulla oblongata.

Advance in medicine is usually slow; often in the history of discoveries there are predecessors whose work has been forgotten, mostly on account of some deficiency in exactitude and force of conviction. For Loeb as long ago as 1884, and T. Ott in 1891 and 1902, attributed some forms of glycosuria and disturbances of temperature regulation to the diencephalon.

The greatest obstacle to experimental investigation of this part of the brain lay in the extreme difficulty of access by the usual methods; this could only be surmounted by using the same technique as that employed for hypophysectomy. The greatest difficulty attaching to clinical (and pathological) investigations was and still is the fact of the neighbourhood of the pituitary body. Even at the present time there are most interesting cases of diabetes insipidus, dystrophia adiposo-genitalis, &c., reported, in which the pituitary gland has been examined most carefully (with positive or negative results), but the diencephalon has been disregarded.

The experimental difficulties were first surmounted by Caselli, who, after electric stimulation of the suprahypophyseal portion of the base of the brain, found slowing of the pulse, increase of blood-pressure and, in one case, glycosuria. But it was not until 1909 that Karplus and Kreidl obtained clear evidence of the presence of sympathetic centres in this region, the irritation of which was followed by dilatation of the pupil and palpebral retraction as well as by vasomotor changes and sweating. In 1912 B. Aschner, after stimulation of the hypothalamic region, obtained the same effect as Claude Bernard after his *piqûre:* namely, glycosuria and polyuria, as well as contraction of the uterus, rectum and bladder. Four years later he assumed the presence of centres for the sympathetic and regulation of metabolism and intestinal function, but without adducing further experimental or clinical evidence.

In the same year (1912) Schmidt and Krehl demonstrated the presence of heat-regulating centres in the diencephalon. After undergoing transverse section of this part of the brain warm-blooded animals lost their power of heat regulation. These statements were confirmed by myself shortly afterwards, and at the same time as Schmidt I showed that heat-regulation was due to the function of a limited part of the brain-stem, namely, the tuber cinereum. Together with Citron I demonstrated the absence of fever in animals experimentally made "cold-blooded," following introduction of fever-producing agents (anaphylatoxin, colloidal suspensions, salt solutions, &c.).

Working with E. Schneider I found a certain diminution of protein metabolism after irritation of the hypothalamic region, while E. Grafe, in accordance with this fact, observed a marked increase after transverse section.

From the beginning of my work on the regio subthalamica, in 1912, until now I have endeavoured to bring forward experimental and clinical evidence in favour of the important rôle of this part of the brain in the causation of different metabolic disturbances.

(2) Anatomical Remarks.

From the phylogenetic point of view the diencephalon certainly belongs to the oldest parts of the brain. In lower vertebrates it seems to play a leading part as the highest centre of nervous regulation. L. Edinger calls it the "archithalamus," whereas the "neothalamus" only develops later in connexion with the progressive evolution of the telencephalon.

As for the histology of this region, a large number of nuclei and fibrous tracts have been described. It would be going too far to give a detailed description here, the more so as we only know very little of their physiological function. I therefore refer only to the work of L. R. Müller and Greving.

(3) Diabetes Insipidus.

B. Aschner first observed polyuria following his *piqûre* of the infundibular region, and I confirmed these statements at the same time as Roussy and Camus. The curves demonstrate the increase in excretion of water and the decrease in the quantity of urea. In fact there is the same failure to render the urine less dilute, as in human diabetes insipidus. By injection of irritant substances this polyuria can be kept up for weeks.

It therefore seemed to me that it would be interesting to repeat what Kahler had done with negative results: i.e., the search for clinical evidence as to the pathogenic rôle of the infundibular region in the causation of diabetes insipidus. When I began this work, the general view was, that the disturbance was caused by a failure in renal concentration (E. Meyer) due to hypopituitarism (E. Frank). I myself had done some work which favoured this hypothesis, as I had been able to isolate from

the posterior lobe a crystalline substance which I erroneously supposed to be a polypeptid, but which probably is an amino-substance somewhat related to histamin.

It was a simultaneous experimental and clinical observation which led me to abandon the pituitary theory. For I found that neither experimental hypophysectomy, nor complete destruction or atrophy of the pituitary gland in human beings produced polyuria, in spite of the entire absence of the urine-concentrating amino-substance. I had the opportunity of seeing the first cases of cachexia hypophysipriva in Hamburg, described by Simmonds.

The absence of any polyuria, hypothermia, hypoglycæmia, or obesity (but on the contrary, the presence of marked cachexia), impressed me considerably and encouraged me to search for some other pathogenic factor.

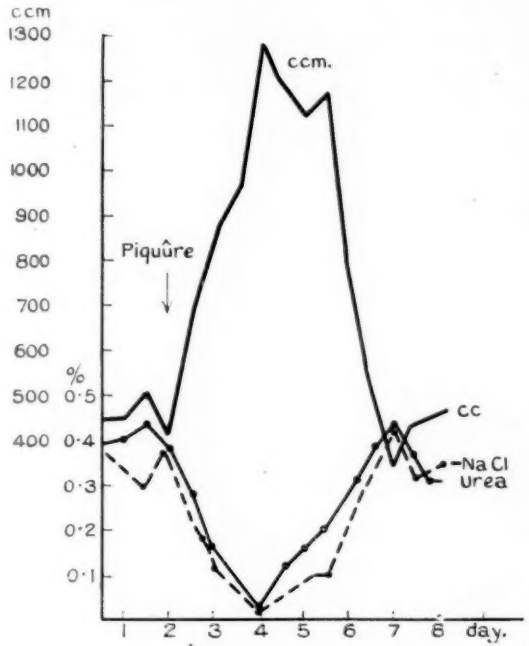

FIG. 1.—Experimental polyuria following *piqûre* of tuber cinereum.

In clinical pathology it is chiefly those alterations which involve the infundibular region and the tuber cinereum that lead to diabetes insipidus. In the well-known case of E. Frank (diabetes insipidus after gunshot wound), which induced this author to accept the pituitary theory, careful examination showed that the bullet was not in the sella, but on the top of it, i.e., in the infundibulum, or tuber cinereum.

In other cases we find tumours of this region, inflammation, syphilitic meningitis with gumma, tuberculoma of the infundibulum, tumours of the hypophysis and third ventricle, apoplexy, hydrocephalus internus, epilepsy and migraine. Some interesting cases have been described in this country by F. Parkes Weber and others.

More recently a few cases have been reported in which diabetes insipidus arose after an attack of epidemic encephalitis. I have also seen the opposite condition: disappearance of diabetes insipidus after encephalitis. Now we know that epidemic encephalitis is a cerebral disease, not a glandular one.

The frequent occurrence of diabetes insipidus after injuries of the skull is easily explained. The infundibular region, fixed to the sella turcica, is a part of the brain most exposed to *coup* and *contrecoup*. The uncomplicated persistence of diabetes insipidus for life in many of those cases, the presence of other nervous troubles (vasomotor, cutaneo-secretory, epileptiform, migraine trouble, lesions of the chiasma) and the absence of genuine pituitary symptoms are in favour of the diencephalic theory. I have seen a Bulgarian officer who after a shell explosion, and wounding of his forehead, immediately developed bitemporal hemianopsia, and persistent diabetes insipidus.

In another case of a prominent physician whom I saw after a severe fall, there was polyuria of 5 to 8 litres in the beginning, 3 litres later, glycosuria (4 to 7 per cent.), which completely disappeared after a few months, attacks of migraine with slight transient hemiparesis and agraphia, hemihidrosis and vasomotor hemiparesis, during which attacks the polyuria again increased.

Finally, I have remarked that in a few cases injection of even large doses of pituitrin fails to increase the urine concentration.

I have dwelt perhaps somewhat too long on the subject of diabetes insipidus. But it seems to me to be an interesting example of autonomic nervous regulation of metabolism. Water, and salt metabolism (molar metabolism), must be regulated in every instance in order to prevent dangerous alterations of molar blood concentration. The kidneys work too slowly to correct instantaneously the balance at every moment. Their duty is to excrete the excess of either water or solids. Instantaneous regulation, however, takes place in the tissues through the capillaries. This regulation works so marvellously that even after abrupt change in the percentage of the solids (by intravenous injection of hypertonic salt solutions) the normal level is immediately restored. This regulation is influenced by the autonomic nervous system. Disturbance of this regulation leads to polyuria—in slighter cases only to diminution in the concentration of the urine. In rare cases it leads even to salt retention with latent or manifest œdema.

The best tests in order to detect slight disturbances of this kind in cases where some diencephalic trouble is suspected are: (1) the concentration test: (2) the estimation of salt and water excretion versus retention, by urinary analysis and taking the weight, after ingestion of 15 grm. NaCl.

It is of the greatest importance to make a thorough histological examination in cases of diabetes insipidus coming to autopsy. I have seen one case examined by F. H. Lewy in our clinic with a normal pituitary and simple degeneration of the nucleus supra-opticus. Similar cases have been published by Roussy and Lhermitte in Paris (syphilitic basal meningitis, destruction of tuber cinereum, normal hypophysis). Another case was reported to me during my stay in New York. Histological examination in these cases will help us to localize the sympathetic "centres" (if it is allowable to use this inexact term) in the diencephalon better and more accurately than we are able to do at present—and to establish their relation to the pituitary body. From the tuber cinereum nerve-fibres go to the nucleus of the parahypophysis. This fact would explain the occurrence of diabetes insipidus in cases of metastatic tumours of the pituitary body which are too small to effect a compression of the diencephalon but cause a reflex disturbance.

(4) Dystrophia Adiposo-Genitalis.

A review of the literature of Fröhlich's syndrome gives a very disappointing result. If we confine ourselves to the post-mortem findings we see great

discrepancies. There is hardly any part of the hypophysis or its neighbourhood which has not been accused of being the cause of this interesting anomaly.

As regards the posterior lobe, I believe, from the evidence of animal experiments and judging Fahr's case of isolated posterior lobe destruction by an epithelioma, that it has not much influence either on growth or on metabolism.

Destruction or atrophy of the anterior lobe nearly always gives rise to one of Fröhlich's symptoms, viz., genital atrophy. Obesity following hypophysectomy has been observed by Biedl, Cushing, and Aschner—but lesions of the infundibular region cannot be excluded from these experiments, the more so as Victor Horsley and Handelsmann, in their experiments, did not obtain obesity. In human pathology, at any rate, destruction, or atrophy, of the anterior lobe has quite the reverse effect: viz., cachexia (Simmond's disease).

On the other hand—all the classical phenomena of Fröhlich's disease can experimentally be reproduced by a mere lesion of the diencephalon between the tuber cinereum and the corpora mammillaria (Bailey and Bremer, and my own observations).

Biedl enunciated the theory that the pars intermedia is involved in the cases of pituitary origin. This theory is difficult to prove or to disprove, for the intermediary cells extend along the infundibulum and the diencephalic base. It is quite impossible, experimentally, to destroy these cells without damaging the diencephalon as well.

The post-mortem findings in more than ninety cases clearly demonstrate how unsatisfactory the pituitary hypothesis has become. In some cases the lesion is found in the anterior lobe, in others in the posterior, in a third group in the pars intermedia and ductus infundibularis. In a good number of cases (more than twenty) the pituitary gland has been found normal, or so slightly affected that it cannot be regarded as impaired functionally. Striking cases have just recently been published by Lude, Raab, Gottlieb, *et al.*

Erdheim, in 1904, first suggested that a disturbance of some nerve centre was the cause of Fröhlich's syndrome. Marburg even used the term "cerebral adiposity." But in the following fifteen years the pituitary body excited much interest, and indeed was so often found affected in these cases that the pituitary theory seemed to have gained the field. When I attacked it in 1920 I was severely criticized. In the past four years, however, views have changed under the weight of facts, and the importance of the diencephalon in the regulation of metabolism is now beyond doubt, whereas the rôle of the hypophysis in the development of dystrophia adiposo-genitalis and obesity has still to be determined more accurately.

Biedl and his assistant Raab have differentiated two forms of dystrophia adiposo-genitalis, one of hypophyseal and the other of cerebral origin. I am not sure whether this solution of the problem is a very happy one, as usually nature does not work in entirely different ways in order to produce the same symptoms. I rather believe that the diencephalic centres always play the foremost part in the development of dystrophia adiposo-genitalis, but I do not deny that the pituitary body has a great influence upon the function of those centres and that its internal secretion is indispensable not only for normal growth and sexual development, but also for normal metabolism.

During the last few years I have seen interesting cases of obesity with and without genital atrophy following epidemic encephalitis and serous or syphilitic meningitis. Many other authors have reported the same for epidemic encephalitis (Economo, Bertolani, Nobécourt, Livet, Feudel, Bychowski, Stiefler, M. Mayer, Roger et Aymès, Raab, &c.). One of my most interesting cases of obesity (weight 340 lb., unaffected by treatment or diet) was a case of syphilitic meningitis. I believe such cases of diencephalic origin are the most resistant to any form of therapy. My case, for instance, took more than 2,000 thyroid tabloids and had had more than 100 polyglandular injections without the slightest effect.

Obesity and general atrophy are not necessarily combined. There are cases which present only one of the two symptoms. It is more than probable that the sympathetic centres regulating metabolism and sexual development are not identical, though closely related.

It is interesting from the practitioner's and from the scientific point of view to note not only those cases with well-marked symptoms, but also the "formes frustes." For instance, a few weeks ago I saw a patient whom I have known for nine years. He had had a severe motor-car accident. Immediately afterwards he manifested polyuria and gained 7 lb. in twelve days; during all the preceding years he had observed his weight carefully and was sure that it had remained the same.

The cause of diencephalic obesity is not a lowering of basal metabolism. I have studied a certain number of cases with Dr. W. Arnoldi. It is not sufficient to determine the basal metabolism only, as is the fashion nowadays. This procedure seems to me like judging the function of the heart by only counting the pulse at rest. What we need is a functional diagnosis of metabolism with estimation not only at rest, but after meals, and even after different sorts of meals, not only as regards oxygen consumption, but also the carbon dioxide production, change of blood-sugar, and output of nitrogen as well. If we do this, we see that these cases of obesity have a tendency to oxidize their sugar insufficiently, to change it at once and deposit it in the form of fat. Their respiratory quotient $\frac{CO_2}{O_2}$ is high; their oxidation processes are slow.

If we apply different tests in order to examine the sympathetic system (which vary in value, but on the whole help us a little to judge the individual form of reaction, and perhaps somewhat of the peculiarity of our patients), then we find a disturbance of the vegetative nervous system balance, with generally more marked irritability of the para-sympathetic part (vagotonia, to use this term with the above-mentioned restriction).

Biedl, who now recognizes the cerebral type of dystrophia adiposo-genitalis, has drawn attention to one special syndrome of the dystrophia adiposo-genitalis combined with other abnormalities of development such as polydactyly, retinitis pigmentosa, cerebral lesion and mental disorders. This syndrome has been already described by J. F. Laurence and R. C. Moon in 1866, but it is Biedl's merit to have established it as a definite morbid entity, which—following the proposal of F. Solis Cohen and Ed. Weiss, who last month published four cases in one family—we might call the Laurence-Moon-Biedl syndrome.

Similar cases, mostly isolated ones, have been reported by Raab, de Schweinitz, Engelbach and A. Mahon, Douglas McAlpine, Jaksch, Bardet, Bertolotti, Razahal, Farnès, Sievert, Madigan and Thomas Verner, Moore, and by F. Parkes Weber.

Another variation of dystrophia adiposo-genitalis, or rather pluriglandular insufficiency, with troubles of sympathetic innervation and a peculiar form of pigmentation of the skin (figs. 2, 3) has been observed already by me in four cases, two of which I have demonstrated at the Berlin Medical Society. A case was demonstrated by Dr. Ullmann. The characteristic features are: Physical under-development, a certain degree of psychical infantilism but without mental deficiency (my first case was a Russian lady doctor), genital dystrophy with or without obesity, diminished response to the administration of suprarenin in relation to blood-pressure and blood-sugar, want of specific dynamic effect on metabolism after glucose ingestion; finally a most characteristic pigmentation with hundreds of small nævi (developing in the first or second decade of life) like those of Recklinghausen's disease, mostly discrete, some of them in the form of "taches de café au lait." The differentiation from true Recklinghausen's disease is shown by the absence of neurofibromata and the presence of the above-mentioned signs of endocrine and sympathetic disturbance. The differentiation from urticaria pigmentosa is shown by the absence in microscopic specimens of basophilic cells, which are generally a prominent feature of true urticaria

pigmentosa. I am convinced that this syndrome, which I have described and propose to call "dystrophia pigmentosa," is not uncommon, and that once attention is drawn to it, it will be found as well in other cases.

One of my cases, a boy of 16, had all the characteristic signs of dystrophia adiposo-genitalis and congenital optic atrophy with amaurosis of one eye, and concentric narrowing of the visual field in the other eye—probably on account of hereditary syphilis.

FIG. 2.—Dystrophia pigmentosa (Leschke).

(5) DIABETES MELLITUS.

At first sight it would seem wrong to associate the diencephalon with diabetes, now that its pancreatic origin has been definitely proved by the success of insulin therapy. But we must not forget that pancreatic activity, like any other glandular function, is under the regulating influence of the sympathetic nervous system. Besides, the effect of nerve irritation on sugar excretion is well known to all of us.

The classical work of Harvey Cushing has given rise to much discussion as to the rôle of the pituitary body in carbohydrate metabolism. But I am convinced that the frequency of disturbance of sugar tolerance and blood-sugar level are not due to loss of pituitary function but rather to involvement of the interbrain.

Among 190 cases of acromegaly I found glycosuria or hyperglycæmia in 82, i.e., 43 per cent., and its frequency in other cases of pituitary or basal tumour or basal meningitis is in about the same proportion. Some cases of epidemic encephalitis with glycosuria also have been reported.

FIG. 3.—Dystrophia pigmentosa (Leschke).

Lépine observed a case of diabetes with 4·5 per cent. sugar and 2 to 3 litres of urine, accompanied by arteriosclerotic degeneration of the corpus striatum. I have demonstrated degeneration and cyst formation in the hypothalamic region of two cases, but I am not sure of their significance. Th. Brugsch, F. Dresel and F. H. Lewy, in our

clinic, have made a careful study of the corpus striatum, and so far they have found distinct degenerative changes in the corpus striautm in every one of the fifteen cases that have come to autopsy during recent years. It is necessary to investigate at autopsy the basal ganglia in every case of diabetes in order to establish definitely the rôle of the interbrain and its relation to pancreatic insufficiency.

The simultaneous occurrence of glycosuria and polyuria in some cases (where the polyuria is not the effect of an abnormally high output of sugar) must be explained by simultaneous alterations of carbohydrate and water metabolism. At times I have found a true combination of diabetes mellitus and insipidus. The impairment of urine concentration is obscured by the presence of sugar, which gives apparently normal figures of specific gravity, but this becomes evident when concentration tests

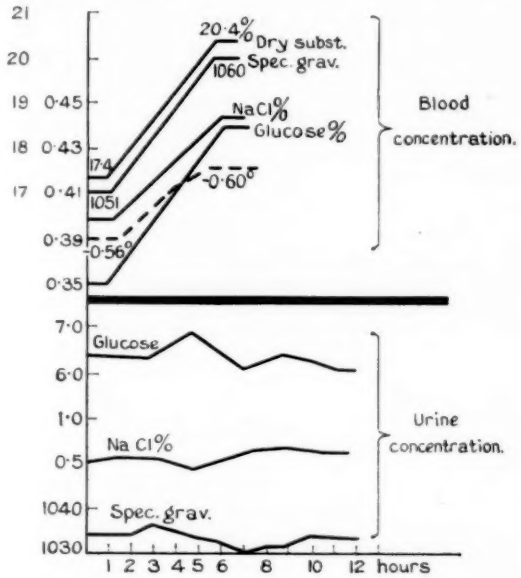

FIG. 4.—Concentration test in diabetes mellitus with polyuria (diabetes insipidus).

are made and the amount of solids estimated by chemical methods. The chart (fig. 4) shows the want of urine concentration and the increase in blood concentration in one striking case after abstaining from drinking, though I admit that usually the changes are less marked.

The frequent concurrence of diabetes and obesity has to be explained in a similar way. Both conditions have been observed to develop after tumours and inflammations of the diencephalon and its base. From animal experiments and from clinical investigations we have come to the conclusion that disturbances of the regulative function of the interbrain between the tuber cinereum and corpora mammillaria give rise to lowering of carbohydrate metabolism and obesity, those of the corpus striatum to hyperglycæmia and glycosuria. In both conditions there is a defective utilization of carbohydrates: in the first one sugar is deposited in the form of fat (high respiratory quotient), in the second one it is excreted by the urine (low respiratory quotient). I refer to the work of W. Arnoldi done in our clinic.

Without going into further details I omit the influence of the interbrain on temperature-regulation (hyper- and hypopyresis), on vasomotor activity (hyper- and hypopiesis), and secretion of sweat.

(6) Conclusion.

There is a close relation between endocrine glandular activity and sympathetic nervous system regulation. In Basedow's (Graves') disease we meet with the simultaneous occurrence of increased thyroid and sympathetic activity, in Addison's disease with lowered suprarenal and sympathetic activity, in dystrophia adiposo-genitalis, with above-mentioned variations. In diabetes insipidus we have discussed the relations between the pituitary body and the subthalamic sympathetic nuclei, in diabetes mellitus the relations of the diencephalon to the pancreas and pituitary body. Fr. Kraus has included this synergism between endocrine and sympathetic function under the term "vegetative system," a term which I think very appropriate, as it reminds us of the co-operation of both systems so essential for the functional integrity of life.

For the consideration of some other problems—chiefly those regarding the individual peculiarities of our patients—we need a more synthetic point of view, like the one of the Hippocratic school in olden times, which has been too neglected during the past century in favour of the analysis of symptoms. I should be glad if my work on the relations between sympathetic and endocrine activity will help a little to get a better understanding of some physiological and pathological problems of metabolism. I am quite aware of the hypothetical character of many of the views expressed, but there should be imagination and play of ideas at the root of progress in science. I hope that, following our line of research, which combines animal experiment, clinical observation, and pathological investigation, we shall slowly but steadily arrive at a better and more complete understanding of the physiology and pathology of the vegetative system.

Dr. F. Parkes Weber said he had recently had the opportunity of examining a few cases allied to the rather rare types referred to by Professor E. Leschke. Since January, 1923, he had occasionally seen a man (Hebrew), then aged 39 years, with retinitis pigmentosa, obesity and sexual impotence—an isolated (non-familial) case, and not a typical case of dystrophia adiposo-genitalis. His attention had been specially drawn to the subject by a letter from Dr. S. Solis-Cohen and Dr. E. Weiss, of Philadelphia, asking him if he could add any subsequent data regarding the family-group of cases published by J. Z. Laurence and R. C. Moon in the *Ophthalmic Review*, London, 1866, ii, pp. 32-41. Solis-Cohen and Weiss had just published their family-group of similar cases, in the current number of the *American Journal of Medical Sciences*. At the recent meeting of the Section of Neurology of the Royal Society of Medicine (March 12, 1925) Dr. Douglas McAlpine demonstrated an isolated (non-familial) case of retinitis pigmentosa with hypopituitarism (but not typical dystrophia adiposo-genitalis) and polydactylism in a boy, aged 15¾ years.

Recently, Dr. Adolphe Abrahams had shown Dr. Weber a man, aged about 40 years, whose whole body was covered with small blackish pigment-nævi (lentigines), and who had a peculiar excess of the soft parts of his fingers and toes[1] with ulnar deviation and over-extensibility of the fingers, but had not typical dystrophia adiposo-genitalis as mentioned by Professor Leschke. He (Dr. Weber) regarded the case as allied to Recklinghausen's disease and tuberous sclerosis of the brain, the patient having likewise a gradually progressive hemiplegia of the right side, which might be explained in that way. There was no headache, vomiting nor optic neuritis. The pigment-nævi might be regarded as taking the place of the facial nævi of the adenoma sebaceum kind, which had been noted in a good percentage of cases of tuberous sclerosis of the brain.

In regard to the consideration of the two rare combination-types referred to above, he

[1] In connexion with this peculiarity of the patient's fingers and toes, which was of congenital or early developmental origin, I cannot help thinking of the rare cases of clubbed fingers of congenital or early developmental origin, which may be familial. Cf. F. Parkes Weber, *Brit. Med. Journ.*, 1919, ii, p. 379. In another at first supposed familial case (*Med. Press*, London, 1921, clxii, p. 27), in a man, aged 51, the clubbed fingers apparently were a sign of commencing secondary osteo-arthropathy.—F. P. W.

(Dr. Weber) wished especially to draw attention to the following points: the retinitis pigmentosa might be represented by an atypical pigmentary degeneration of the retina; the dystrophia adiposo-genitalis might be incomplete and atypical; the symptoms of Recklinghausen's disease might be incomplete and atypical (for instance, innumerable cutaneous lentigines); the other occasionally associated developmental abnormalities, such as polydactylism or congenital malformation of the heart, might be various in kind and degree. He (Dr. Weber) believed that the association of polydactylism with definite or incomplete dystrophia adiposo-genitalis and retinitis pigmentosa was to be regarded as analogous to the occasional association of congenital cardiac abnormalities with Mongolism.

Hypoglycopyresis.

By Otto Leyton, M.D.

In the past, diseases have been named either after the men who described them or after the most prominent signs or symptoms. For instance Parry, Graves and Basedow were the earliest writers upon exophthalmic goitre, and at different times and in different countries the condition has been called Parry's disease, Graves' disease and Basedow's disease. Gull and Ord described a condition due to atrophy of the thyroid, and some called it Gull's disease, others Ord's disease, then later it was named after the most obvious sign, myxœdema.

The disadvantage of calling a disease after a man is twofold; first, it adds to the burden which our memories have to sustain, and secondly it leads to confusion because in different lands the name is not identical. For instance the disease we in England used to call Graves' disease is called Basedow's disease in France and Germany.

Certain disadvantages attend the naming of a disease after the most prominent symptoms, because at the beginning of the disease these symptoms may be absent, leading to a contradiction in terms, such as exophthalmic goitre with neither protrusion of the eyes nor enlargement of the gland. For this reason names based upon the morbid changes in the tissues which cause the disease have been adopted. Graves' disease (exophthalmic goitre) is now called hyperthyroidism; Gull's disease (myxœdema) is termed hypothyroidism.

Diabetes mellitus owes its name to the signs polyuria and glycosuria. The onset of diabetes mellitus may be insidious or acute; at present we have no proof that those cases which appear to be acute are not really insidious cases aggravated by some toxæmia. My observation, recorded in 1917, that the introduction of small quantities of certain bacterial poisons depresses the carbohydrate metabolism most profoundly, is now generally accepted. That is one of the reasons for extirpating any focus of infection in a patient suffering from diabetes mellitus. When the onset is insidious there is a stage at which there is neither polyuria nor glycosuria but only a slight diminution in the efficiency of carbohydrate metabolism. This I term hypoglycopyresis. The simile between hypoglycopyresis and hyperthyroidism is most striking, since in mild hyperthyroidism there may be a complete absence of symptoms except when the patient is submitted to an emotional orgy, and so, too, in early hypoglycopyresis there is neither polyuria nor glycosuria unless the patient submits himself to some carbohydrate orgy. When the pancreas is the cause of the hypoglycopyresis then the term "subinsulism," meaning the inefficiency of the beta cells of the islets of Langerhans, may be used, but it must be realized that subinsulism is probably not the only cause of the depression of the carbohydrate metabolism.

Naturally the question arises, how is the condition recognized if there be no polyuria and no glycosuria? So far, the two cases investigated have had intermittent glycosuria due either to a carbohydrate orgy or to a temporary fall in the renal threshold for dextrose. In the future I hope to recognize hypoglycopyresis in

The second case I have to record provides evidence that the disease is progressive, and, since subinsulism is usually progressive, we have reason to suppose, but no proof, that the condition is of pancreatic origin.

L. H., aged 37. Sugar had been found in this man's urine six months before he came under observation. On cross-questioning him he admitted to having experienced thirst, but no more, I think, than an ordinary individual would admit to. He did not suffer from polyuria at any time. The patient was found to have sugar in his urine, and was placed upon a diet of 45 grm. carbohydrate, 78 grm. protein, 130 grm. fat. Although his blood-sugar fell to 0·09 per cent. one hour after the mid-day meal, his urine nevertheless continued to contain small quantities of sugar. He was therefore given 25 grm. of dextrose in order to attempt to discover what was his renal threshold for dextrose. It proved to be below 0·12 per cent.

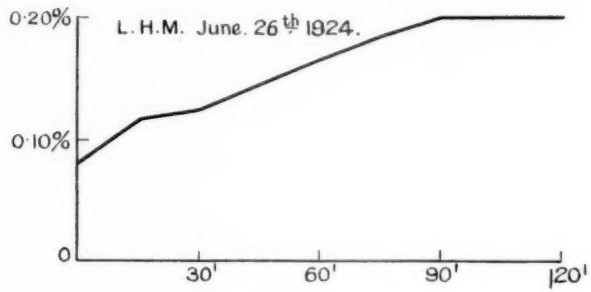

Chart IV.

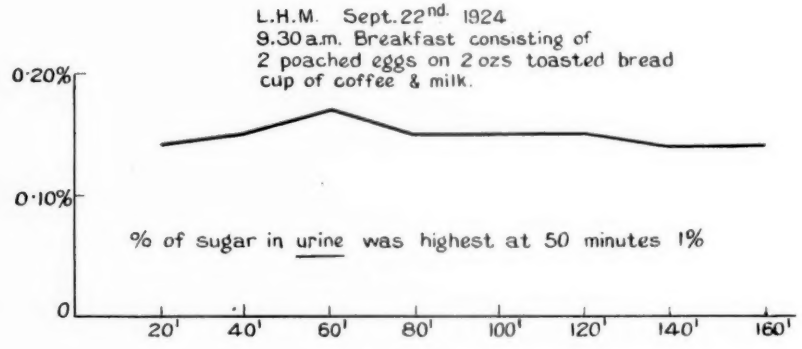

Chart V.

After he had taken 25 grm. of dextrose, his blood-sugar apparently did not rise above 0·14 per cent. Following a meal containing, amongst other things, suet-pudding and plenty of syrup, his blood-sugar rose to 0·33 per cent., showing that his carbohydrate metabolism was below normal. Some days later a 20 per cent. solution of dextrose was injected intravenously at the rate of 3 c.c. per minute for two hours. The blood-sugar started at 0·08 per cent. and rose to 0·20 per cent. after ninety minutes, at which point it remained during the following half hour. This seemed to prove that the patient was able to store and burn sugar at the rate of nearly 0·6 grm. per kilogram body weight per hour (Chart IV).

In other words his carbohydrate metabolism had an efficiency of 75 per cent. He suffered from hypoglycopyresis, not sufficient to cause thirst or polyuria, but sufficient to cause hyperglycæmia after taking an easily absorbable sugar in large quantities; he also possessed a low renal threshold for dextrose. Glycosuria was present almost continuously.

This patient suffered from duodenal ulcer as well, and it was thought wise that an operation should be carried out. After the operation his renal threshold for dextrose apparently rose. For a time his urine was free from sugar, and at first sight it appeared as if the operation had benefited his carbohydrate metabolism. After a meal containing 50 grm. of carbohydrate in the form of bread, the sugar in his blood did not rise above 0·17 per cent. (Chart V).

Upon injecting a 20 per cent. solution of dextrose intravenously at the rate of 3 c.c. per minute, in a manner exactly similar to that carried out before the operation it was found that the sugar in his blood rose to 0·37 per cent. instead of 0·20 per cent. as before the operation (Chart VI).

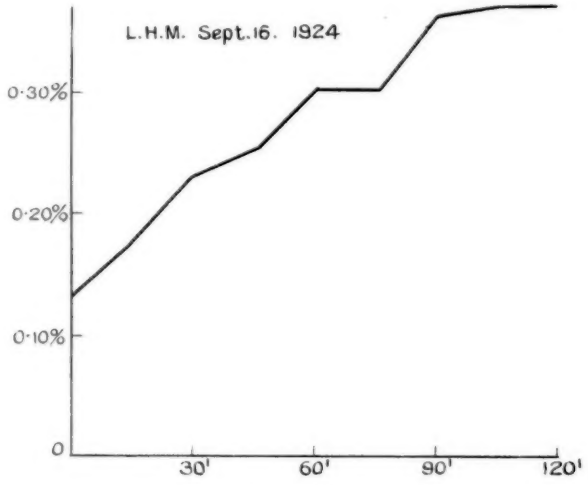

CHART VI.

Probably the effect of the anæsthetic had led to a depression in the carbohydrate metabolism. To what extent exactly it had become depressed—whether it had dropped to 50 per cent.—I did not have an opportunity of determining, the patient being anxious to get back to work.

In my opinion the record of these two cases tends to show that one may meet with an instance of hypoglycopyresis without any of the usual signs of diabetes mellitus—polyuria, glycosuria, or even hyperglycæmia after meals whilst upon an ordinary diet.

A CLINICAL MEETING of the Section was held at Charing Cross Hospital by the invitation of the medical staff, who showed a series of interesting cases. Drs. Forsyth, Gordon Holmes, Adie and Chandler presented cases involving problems of diagnosis and treatment, which were discussed by various Members and visitors.

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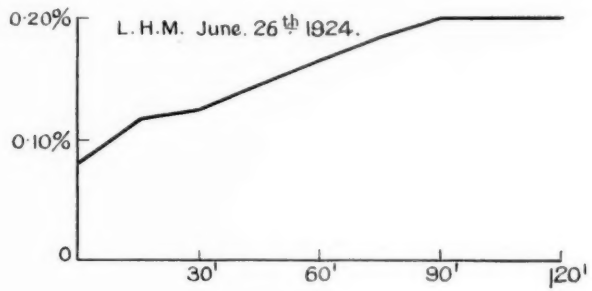

Chart IV.

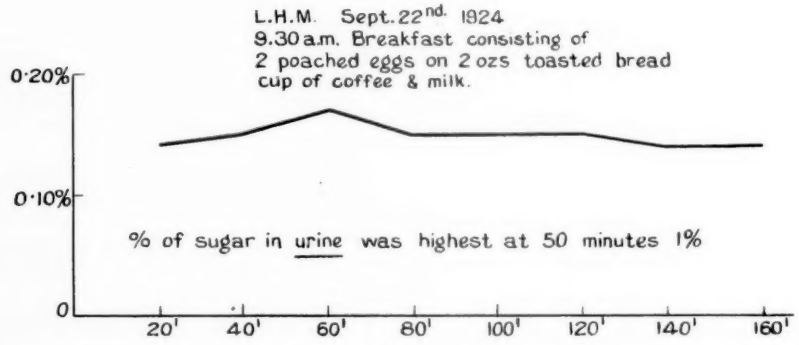

Chart V.

After he had taken 25 grm. of dextrose, his blood-sugar apparently did not rise above 0·14 per cent. Following a meal containing, amongst other things, suet-pudding and plenty of syrup, his blood-sugar rose to 0·33 per cent., showing that his carbohydrate metabolism was below normal. Some days later a 20 per cent. solution of dextrose was injected intravenously at the rate of 3 c.c. per minute for two hours. The blood-sugar started at 0·08 per cent. and rose to 0·20 per cent. after ninety minutes, at which point it remained during the following half hour. This seemed to prove that the patient was able to store and burn sugar at the rate of nearly 0·6 grm. per kilogram body weight per hour (Chart IV).

In other words his carbohydrate metabolism had an efficiency of 75 per cent. He suffered from hypoglycopyresis, not sufficient to cause thirst or polyuria, but sufficient to cause hyperglycæmia after taking an easily absorbable sugar in large quantities; he also possessed a low renal threshold for dextrose. Glycosuria was present almost continuously.

This patient suffered from duodenal ulcer as well, and it was thought wise that an operation should be carried out. After the operation his renal threshold for dextrose apparently rose. For a time his urine was free from sugar, and at first sight it appeared as if the operation had benefited his carbohydrate metabolism. After a meal containing 50 grm. of carbohydrate in the form of bread, the sugar in his blood did not rise above 0·17 per cent. (Chart V).

Upon injecting a 20 per cent. solution of dextrose intravenously at the rate of 3 c.c. per minute, in a manner exactly similar to that carried out before the operation it was found that the sugar in his blood rose to 0·37 per cent. instead of 0·20 per cent. as before the operation (Chart VI).

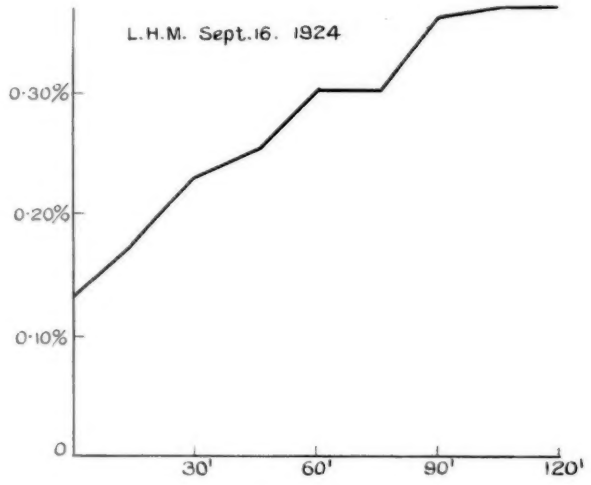

CHART VI.

Probably the effect of the anæsthetic had led to a depression in the carbohydrate metabolism. To what extent exactly it had become depressed—whether it had dropped to 50 per cent.—I did not have an opportunity of determining, the patient being anxious to get back to work.

In my opinion the record of these two cases tends to show that one may meet with an instance of hypoglycopyresis without any of the usual signs of diabetes mellitus—polyuria, glycosuria, or even hyperglycæmia after meals whilst upon an ordinary diet.

A CLINICAL MEETING of the Section was held at Charing Cross Hospital by the invitation of the medical staff, who showed a series of interesting cases. Drs. Forsyth, Gordon Holmes, Adie and Chandler presented cases involving problems of diagnosis and treatment, which were discussed by various Members and visitors.

On January 16, 1925, a preliminary communication concerning the "Electronic Reactions" of Abrams, with special reference to the "Emanometer" technique of Boyd, was presented to a joint meeting of the Sections of Medicine and Electrotherapeutics of the Royal Society of Medicine, by Sir THOMAS HORDER, Bt., M.D., on behalf of: M. D. Hart, A.M.I.E.E., M.Sc., D.I.C.; C. B. Heald, C.B.E., M.D.; Sir Thomas Horder, Bart., M.D., B.Sc.; Lt.-Col. H. P. T. Lefroy, D.S.O., M.C., A.M.I.E.E.; W. Whately Smith, M.Sc.

This communication has been published *in extenso* elsewhere (see *The Lancet*, January 24, 1925, p. 177).

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON :

JOHN BALE, SONS AND DANIELSSON, LTD.,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Laryngology.

President—Dr. A. Logan Turner.

Case of Nasal Growth.

By Irwin Moore, M.Ch.

Patient, a female, aged 19, was first seen by exhibitor, on August 15, complaining of nasal obstruction for the past three months, accompanied by a thick yellow discharge from the right nostril. Recently, vision of the right eye became slightly blurred, and in May she attended the Western Ophthalmic Hospital, where the right lacrymal sac was washed out on account of purulent secretion and glasses ordered.

At 8 years of age patient attended the Paddington Green Children's Hospital, complaining of a mass which had formed in the right nostril during the previous six months. Her tonsils and adenoids were removed at this time by Mr. Elphick, and reference to the notes of her case records that "granulations" were also removed from the right nostril.

Marked broadening of the nose with separation of the nasal bones. The right naris is completely filled by a hard fleshy growth which is presenting in the vestibule, and has pushed the septum over to the left side. Posteriorly the growth is seen occluding the right choana, likewise in this position pushing the septum to the left. Here the growth is of a more polypoid character. No history of any bleeding. There is slight protrusion of the right eye.

X-ray examination shows a dense opacity in the right side of the nose, pushing the septum to the left. This opacity involves the antral wall on the right, and invades the antrum and the right ethmoid cells. (F. M. Allchin.)

Naso-pharyngeal Fibroma extending through Spheno-maxillary Fossa to Cheek.

By Leslie Powell, M.B.

A boy, aged 15. A small nodule can be seen with the post-nasal mirror on the right side which does not appear to be growing.

The swelling of the cheek was thought to be in the parotid.

The naso-pharyngeal tumour was removed in March, 1923, but recurred, and was removed again in July, 1924. The swelling in the cheek was traced back to the spheno-maxillary fossa and was removed in August, 1924. Section: fibroma.

Mr. E. Broughton Barnes, F.R.C.S.Ed. (introduced by Mr. E. D. D. Davis) exhibited (*a*) A Snare through which a Diathermy Current can be passed, intended for use in the Naso-pharynx, through the Mouth; (*b*) Tumour (Angio-fibroma) removed from Naso-pharynx of a boy of 13, with the Snare, entirely Bloodlessly.

DISCUSSION.

Mr. W. STUART-LOW (discussing Dr. Irwin Moore's case) said that he (the speaker) had had similar cases. It was difficult to account for the origin of this condition, but there was in this girl a history of a severe blow two years since with a base-ball, and she states that the nose did not bleed. There must have been hæmorrhage into the tissues, and, most likely, into the maxillary antrum. It was possible that such intra-tissue hæmorrhage was the explanation of the origin of these tumours, as a similar history of injury was obtained in his patients.

As regarded treatment Mr. Stuart-Low said he would have no hesitation in thoroughly avulsing the growth from the nasal passage, at the same time clearing out the antrum and applying the galvano-cautery point to the seat of origin if it were possible to locate it. Hæmorrhage might be severe, but if thromboplastin were injected every three hours for twenty-four hours before operation, bleeding would easily be controlled.

Dr. W. H. KELSON said his remarks were intended as a caution to those dealing with such cases as Dr. Irwin Moore's. He had a boy aged 8 at the hospital with one of these growths, which he removed with a snare and strong adenoid forceps. The boy had previously had very large hæmorrhages, but his progress was satisfactory. A fortnight after operation a final examination before the patient left revealed what seemed like a granulation or a small piece of growth. He (the speaker) touched this with the galvano-cautery, and there was a sudden and very severe hæmorrhage, the boy fell on the floor, and before anything could be done, he died. No autopsy was allowed.

Mr. THOMAS GUTHRIE asked, in regard to Mr. Leslie Powell's case, what was meant by the remark "the swelling in the cheek was traced back to the spheno-maxillary fossa." In several cases of nasopharyngeal fibroma he had noted a swelling of the cheek, and was in doubt as to the cause of it. It did not seem to be due to the growth extending into the cheek; possibly it was associated with a portion of growth filling the spheno-maxillary fossa and causing obstruction of venous or lymphatic channels.

Dr. H. SMURTHWAITE said he did not know whether Dr. Irwin Moore's case was a genuine fibroma. On removal he thought it would prove to be a very thickened polypus. In a chronic case the thickness might be a quarter of an inch, and inside it might be a mucocele. He suggested removing a piece from the front, and treatment would depend on what it was found to be. If a fibroma, Moure's operation could be done.

Mr. A. J. M. WRIGHT said he had a patient, a boy, from whom he removed a naso-pharyngeal fibroma. Before the operation there was an indefinite swelling of the cheek on the side to which the growth was attached. Since the removal of the growth, that swelling had subsided. He thought the swelling must be due to interference with the vascular supply to the part by pressure.

Dr. W. HILL said that with regard to Dr. Irwin Moore's case, he feared Dr. Smurthwaite was somewhat over sanguine if he imagined this was an ordinary mucous polypus, or even so in part, the remainder being fibrous. The ordinary mucous polypus did not usually attack the ethmoid cells and cause proptosis, as in this instance. In one such case he (Dr. Hill) had done a modified Rouge operation, which gave very fair access, but Moure's operation would be better. In the case to which he was referring he was confronted with very alarming hæmorrhage, and the possibility of such in these cases led the operator to approach them with trepidation. In the case of the present patient it would be well to see what could be done *per vias naturales*, stop the bleeding, and then do an external operation.

Mr. G. W. DAWSON (referring to Mr. Powell's case) said a few years ago he had exhibited a similar case, and objection was taken to his calling it fibroma; it was considered that the proper name for it was angio-fibroma, because of the numerous blood channels (specimen shown). That tumour caused a swelling in the cheek; it extended at the back of the upper jaw, and was attached to the main growth by a pedicle.

Mr. F. H. WESTMACOTT said that for some time he had been accustomed to have the patient so prepared that diathermy could be employed if serious bleeding ensued. Even when artery forceps, applied to a vessel, were touched with the diathermy point, hæmorrhage ceased at once.

Dr. W. S. SYME said he agreed with Dr. Smurthwaite that Dr. Irwin Moore's case was ordinary nasal polypus, due to pressure, but he did not think the possibility of its being sarcoma should be overlooked. He advised removal of the growth through the nose and opening the antrum at the same time. It was possible that it originated from the antrum, filled the nose, was of the nature of a choanal polypus, and that the fibrous condition of it was due to pressure. Posterior rhinoscopy showed what looked like an ordinary polypus, but he had had a case with the same appearance and the other part turned out to be sarcomatous.

Mr. NORMAN PATTERSON said he would give radium a good trial in this case, as in some cases the patients had been cured completely by radium, no operation having been required.

Dr. BROWN KELLY said he doubted if Dr. Irwin Moore's case was one of fibroma, because of the patient's age (19) and absence of bleeding.

Dr. JOBSON HORNE said these cases of intra-nasal, para-nasal and post-nasal tumours caused differences of opinions, not only among clinicians, but also among microscopists. The tendency was to diagnose sarcoma where it did not exist. Sarcoma was a term, and a matter of degree, and the tendency of these tumours was to kill by local extension and destruction, and not by metastases. The tumours diminished under radium treatment, so much so that the reputation of radium as a remedial measure in malignant disease had largely been built upon them. He considered radium should be given a full trial before resorting to extreme measures.

Dr. IRWIN MOORE (in reply) said that against the idea of malignancy in the case he was exhibiting was the slow growth of the tumours. The hisory of "granulations" having been removed from the right nostril at 8 years of age suggested that the growth might have originated at that time, recurring later but remaining latent for some years, until the recent onset of active growth. As to its site of origin, a skiagram suggested that the growth probably originated from the right ethmoid region, the right maxillary antrum being apparently not involved. The antro-nasal wall appeared to be bulging outwards.

He intended to remove the growth by a lateral rhinotomy, employing Moure's lateral incision, which would thoroughly expose the growth, and if before operation the nasopharynx was well plugged, he did not anticipate any trouble with hæmorrhage, since it could be easily controlled.

Mr. E. BROUGHTON BARNES (in reply) said that he had designed a snare for the tumour he showed; he had not had any other opportunity of using it. The patient had had two extensive hæmorrhages before he (the speaker) saw him. There was no hæmorrhage at the time of this removal, nor at any other time.

Mr. LESLIE POWELL (in reply) said that at first he did not realize what was the nature of the swelling in the cheek. He removed the nasopharyngeal growth through the antrum, by diathermy. A little of the swelling went down but a well-defined tumour in the cheek remained. He thought this was due to calculi blocking Stenson's duct and referred patient to Mr. Joyce, who turned back a flap and removed the tumour, which proved to be fibroma.

In reply to Dr. Brown Kelly, he said he did not notice anything on the left side, except that the septum was pressed over to that side by the large growth, which had been there two years. The bleeding was not very severe, and when it occurred it was arrested by the use of diathermy. The nodule in the nasopharynx seemed now to have disappeared.

Two Specimens showing Ulceration of the Larynx associated with Vincent's Organisms.

By E. W. GOODALL, O.B.E., M.D. (Introduced by Mr. T. B. LAYTON).

(1) THIS specimen was obtained from a male patient, aged 21, who died in the Eastern Hospital, Homerton, March 22, 1909.[1]

History.—Admitted February 22, 1909, with nephritis following scarlet fever. Had severe ulceration of gums; teeth extensively diseased. About twenty-four hours before death had slight stridor, hoarseness, dysphagia and tonsillitis.

[1] For notes of Case see "Metropolitan Asylums Board," 1909, p. 246.

Description.—Specimen shows deep ulceration of top of epiglottis on left side and deep undermined ulcers over arytænoid cartilages; left ulcer has been opened up with knife. At post-mortem examination ulcers contained yellowish pultaceous matter, slough and necrosed cartilage and emitted a most offensive odour. A smear made from the pultaceous matter showed large numbers of fusiform bacilli with fewer spirilla, also cocci and other bacteria or organisms. A smear made during life (March 1, 1909) from ulcers on gums had failed to show Vincent's organisms.

(2) This specimen was removed post-mortem from a female patient aged 53 who died at the Eastern Hospital, October 14, 1911.

History.—Sent to hospital, October 7, 1911, as a case of diphtheria, but was found to have faucial inflammation, ulceration of tonsils and stomatitis; teeth much decayed; showed signs of septicæmia.

Description.—Specimen shows ulceration of tonsils and a large slough still adherent on each side of epiglottis, at the top (if these sloughs had separated, ulcers like those seen in the previous specimen would have resulted). There is also ulceration over the arytænoid cartilages. Vincent's organisms were found in smears made from ulcers on tonsils.

Dilatation of heart, atheroma of aorta, œdema of lungs and nephritis were found on post-mortem examination.

DISCUSSION.

Mr. PHILIP FRANKLIN mentioned a case under his care, of a man aged 33 who had an infection and ulceration of the tonsils, soft palate, lymphatic tissues behind the posterior pillar and the posterior pharyngeal wall. His general condition was that of a septicæmia. Diphtheria was suspected and an antitoxin given. A swab was taken; this failed to reveal the presence of Vincent's organisms, although Vincent's angina was subsequently suspected. The temperature ranged between 102° and 104° F., the pulse rate varied from 95 to 115. During this period acutely tender spots occurred in the middle of both tibiæ and radii. The throat was treated with tincture of iodine and salvarsan powder, and injections of arthritin given. After three weeks the general condition improved but the local condition did not alter. A Wassermann test was made and proved to be positive. One intravenous injection of salvarsan produced an immediate improvement of the throat condition. The case was undoubtedly one of acute syphilis, probably aggravated by a mixed infection, pneumococcal or streptococcal in nature.

Mr. G. WILKINSON said he had seen many persistent Vincent's angina infections in both throat and gums, during the war period, and in one or two cases resolution occurred after not only the local application of "606," but intravenous injection of a small dose. These cases showed a negative Wassermann reaction. Bar, of Nice, had written an article in which he drew attention to the serious complication of infection by moulds and yeasts along with Vincent's angina. In many resistant cases those organisms might be responsible for the persistence of the lesions.

Mr. LESLIE POWELL said the organisms of Vincent's angina sometimes remained latent in the tonsils when signs of ulceration had disappeared from the mouth. In the War Hospital he removed tonsils after Vincent's angina had been apparently cured, and there was an immediate flare-up of the disease. It did not recur after further treatment with "606."

Dr. E. W. GOODALL (in reply) said that the young man died of acute nephritis following scarlet fever, and that the woman had septicæmia, nephritis and broncho-pneumonia. He considered the laryngeal condition was secondary to that in the mouth and throat. His experience was that cases of Vincent's angina had been more common since the War. Vincent originally described the disease as being confined to the tonsils, but it was now clear that the condition might extend to the larynx, though it was rare for this to happen.

Case of Tuberculosis of the Larynx with an unusual degree of Involvement of the Base of the Tongue.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

C. G., AGED 35, with physical signs of pulmonary tuberculosis but no bacilli in the sputum; complained of difficulty in swallowing, especially on the right side of

the throat. When seen in March of the present year the laryngoscope revealed extreme thickening of the epiglottis with superficial ulceration on its lingual surface. A portion of tissue removed from the epiglottis was found on section to contain giant cells and necrosis of tissue with some fibrosis. He used an inhalation of anæsthesin and orthoform powder, while trichloracetic acid was applied to the ulceration. The pain diminished very considerably, but when patient was seen again more recently there was found to be an excavated ulcer in the base of the tongue in front of the epiglottis, continuous with the ulcer on the lingual surface of the epiglottis.

Case of Hyperplastic Laryngitis (with Microscopical Section).

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, a male aged 34, complained of hoarseness but especially of difficulty in breathing, with stridor, which interfered considerably with his getting to sleep. He was sent to Brompton Hospital as being probably a case of tuberculous laryngitis, but no pulmonary signs were found and repeated examinations of the sputum revealed no tubercle bacilli. Laryngoscopic examination showed general rounded swelling of the epiglottis with fairly normal contour and shiny surface, while the ary-epiglottic folds were extremely enlarged, the swelling being mobile or flaccid to such an extent that they were sucked in, more especially the left one, during inspiration. A portion removed for microscopical examination was found to contain areas of hyperplasia of epithelium, foci of necrosis and chronic inflammatory changes, and a section stained for bacilli contained none. The breathing was only slightly improved, but the subsequent removal of a much larger portion made the breathing quite easy, and this it has continued to be ever since. There is still a little want of clearness in the voice, but otherwise the patient's condition is quite satisfactory. He inhales powder consisting of anæsthesin and orthoform whenever he has any feeling of fullness in the throat.

The exhibitor found a somewhat similar appearance in a case of acromegaly (under the care of Dr. Wyndham Milligan), in which the discomfort was of the same nature and was completely relieved by the removal of a large portion of each hyperplastic ary-epiglottic fold.

DISCUSSION.

Sir STCLAIR THOMSON suggested that the case of hyperplastic laryngitis was undoubtedly one of tuberculosis, and if he were asked for proof of it, he would say, first, the age of the patient, and various negative results of investigation—he presumed the Wassermann test had been done. The patient was not at an age when the condition was likely to be malignant. Still, it could only be settled by direct inspection. In spite of no tubercle bacilli being found, and in spite of the negative microscopical appearance, he did not doubt that the condition was tubercle, and, even if its nature was doubtful, he strongly advised that tracheotomy should be done. Tracheotomy was, he considered, too little resorted to as a curative method; it was simple and safe, and there could be very little objection to it. Mr. Howarth and he had had, together, the case of a retired medical man, aged 70, with a similar condition. It was suggested that this case was malignant; Dr. Perkins gave a negative report in regard to tubercle, but after much persuasion the patient agreed to tracheotomy being done, and this was carried out in June, 1923. That day he (the speaker) had received a letter from the patient stating he had got well, that he had discarded his tracheotomy tube, and had played golf.[1] He had also the case of another medical man, with a similar condition, and he refused tracheotomy. It was five years before he died, and no tubercle bacilli had ever been found

[1] Case shown at December meeting, 1924, see *Proceedings*.

at all. At the end he had to undergo tracheotomy in order to avoid asphyxia, and he might as well have had it done earlier to save his life.

He (the speaker) warned younger members as to removing pieces of growth from larynges in which tuberculosis was suspected for the sake of arriving at a diagnosis which could be made without it. It rarely settled the diagnosis, but opened the way to secondary infection. These patients bore operation badly, and removal of pieces for examination was likely to lead to trouble.

Dr. JOBSON HORNE adversely criticized Sir StClair Thomson's suggestion that tracheotomy should be done as a curative measure in laryngeal tuberculosis; speaking generally, he himself considered that tracheotomy was a thing to be avoided in laryngeal tuberculosis. Cases of the disease occurred in which that operation was necessary to overcome stenosis and obstruction, but such were few in number. It was a pity the two cases had been discussed together as they widely differed. The unusual features about the second case were that the thickening of the epiglottis and the arytenoids was so symmetrical, and the disease was so confined to the extrinsic part of the larynx. He thought the exhaustive report from Brompton Hospital, negativing tuberculosis, was important evidence. Moreover, as the internal operative measures upon the larynx by Sir James Dundas-Grant had been so successful in making "the breathing quite easy," "as it has continued to be ever since," tracheotomy as suggested by Sir StClair Thomson was certainly not called for.

Dr. LOGAN TURNER (President) said that he failed to find any evidence of tubercle in the case of hyperplastic laryngitis, but, clinically, he was inclined to take Sir StClair Thomson's view.

Sir JAMES DUNDAS-GRANT (in reply) said that it would be interesting to know whether Sir StClair Thomson had seen cases presenting this "floppy" character. To himself it did not look like a tuberculous infiltration, but he removed a small portion for microscopical examination. The man had stridor, which prevented sleep, owing to the flaccidity of the hypertrophied ary-epiglottic folds, which were drawn in. He (Sir James Dundas-Grant) then removed a large portion from each, and since then there had been good breathing, and nothing to call for tracheotomy. He would watch the case, and bring it forward again.[1]

The condition was very similar to what he saw in a case of acromegaly; the discomfort was the same. He removed the trouble completely by punching out a considerable portion from each ary-epiglottic fold, and there had been no recurrence.

Postscript.—The family doctor has just written to say (December 15) that the patient is wonderfully improved, and that his vocal cords can be eastily seen.

Section of Laryngology.

President—Dr. A. Logan Turner.

An Instrument for the Application of Diathermy to the Œsophagus.

Shown by A. J. Wright, F.R.C.S.

The electrodes, of various sizes, are designed to be used in conjunction with Jackson's œsophageal bougies, to which they correspond in length and diameter. The method of use is first to pass the largest size of bougie possible into the stricture, and then, having replaced this with an electrode of the same size, to switch on the current for as long as is considered safe in the particular case.

DISCUSSION.

Dr. W. Hill reported a case in which the deep pharynx was involved, also the cervical, œsophageal and the supraclavicular parts. On the fifth day there was found to be hydropneumothorax, with a metallic tinkling with each heart-beat. The patient also had pericarditis, and died on the seventh day. In that case diathermy resulted in perforation into the apex of the pleural cavity. He (Dr. Hill) said that very few people knew how much they were doing with diathermy, the electrode might easily pass beyond the limits of the growth; or at a particular spot there might be thinning, due to fungation and breaking down. The penetration of radium, on the other hand, was known to be weak at a centimetre distance, and there was less risk of penetration, so as to cause perforation.

Mr. W. M. Mollison said he had done diathermy in a few cases of carcinoma of the œsophagus, and he had used the needle at the end of a bicycle spoke, covered with a rubber tube to prevent sparking on the side of the œsophagoscope, as suggested by Dr. Iredell. He did not think much harm was done if the needle was put into that part of the growth projecting into the lumen of the œsophagus. In one case relief persisted six months, and then the man came to have a second treatment, and after that he was not so much relieved as after the first. By means of diathermy one relieved the dysphagia to a considerable extent for a time.

Dr. Dan McKenzie said he had tried diathermy by means of a long terminal in post-cricoid cancer. One of his cases benefited for a time, but in two others abscesses formed at the root of the neck and discharged externally. He had not ventured to apply diathermy to the intrathoracic œsophagus, as it was manifestly impossible to see and to gauge the depth of penetration in the œsophageal wall. If septic cellulitis were to follow the diathermy applied there a fatality would be the result.

Sir William Milligan said there was a considerable future for diathermy in the treatment of œsophageal carcinoma. He had his electrode mounted on a handle at an angle, so that when it was passed down the tube nothing blocked the view. He asked whether any members had experienced complete stenosis of the œsophagus after diathermy. These cases might be treated under the guidance of the fluorescent screen. He had now under treatment an old gentleman with carcinoma, who had gastrostomy done nine months ago. The speaker diathermized him almost immediately afterwards, and the result was to convert the carcinoma into a fibroid mass, with a resulting complete œsophageal obstruction.

Mr. Wright (in reply) said that one felt justified in using the method in the effort to ease some of the trouble these patients endured. With regard to the time the relief lasted, he said that last April he diathermized in a case in which there was inability to swallow solids. The patient had some months of relief following the treatment. On re-application, the relief was not so long or so marked. He (Mr. Wright) finished by putting in Souttar's spiral metal tube, and that was still in place. The point of importance, and the whole difficulty, consisted in being sure that one was applying the diathermy to the stricture only.

Early Lupus of the Palate, treated by Diathermy.

By Dan McKenzie, M.D.

Man, 29. Circular patch on palate first discovered by him a year ago. Small piece removed in June. Suspicions of malignancy. Large and deep removal with the diathermy knife undertaken in July. Tissue showed lupus. Following the deep excision a perforation in palate appeared, bone exposed and destroyed. Cicatrization proceeded smoothly; perforation healed.

Case of Laryngeal Syncope in a Patient with Carcinoma of the Larynx.

By Sir James Dundas-Grant, K.B.E., M.D.

Man, 57.

History.—Carcinoma left half of larynx, concealing entrance (extensive glandular involvement). On two occasions, just when beginning to cough, he fell down momentarily unconscious, sustaining injuries to the face; one attack was quite recent, the other four weeks previously.

DISCUSSION.

Mr. W. M. Mollison asked whether Members had seen paralysis of the soft palate and of one side of the tongue in association with massive glands in the neck, as in this case. In his experience it was very rare.

Sir James Dundas-Grant (in reply) said it was possible that there might be a lesion involving the two nerves at the base of the skull. The patient began to cough, and then found he had fallen down on two occasions and struck his head. The syncope was not due to the floating in of a portion of the cancer. In his other cases, with this symptom there was arteriosclerosis, but in this case there was said to be no evidence of that condition, except some rise of blood-pressure. Still, there was obvious goutiness, and his impression was that in these cases the vessels of the brain did not adapt themselves as in the normal person, and hence it was much easier to produce that sudden anæmia of the brain which occurred when inspiration (with expansion of the thorax) was carried out with a closed larynx, as in the inspiration which formed the first stage of coughing.

Case illustrating the Advantage of Tracheotomy as a Curative Measure for Tuberculosis of the Larynx, in a Medical Man, aged 70.

By Sir StClair Thomson, M.D., F.R.C.S., and Walter Howarth, M.S.

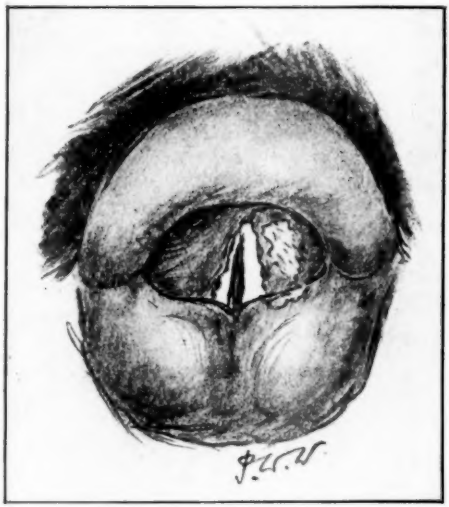
J. R. L. (F.R.C.S.Eng.). Spring of 1922, discomfort in throat, woolliness and change in voice. 1899, digital chancre, two years' mercurial treatment. Wassermann negative. Recent course of iodide and several treatments with salvarsan had not relieved him and he was apprehensive of malignant disease.

Vocal cords clear. Pale, almost uniform infiltration of the epiglottis, arytenoids and ary-epiglottic fold.

With general care (avoidance of tobacco, care of teeth, further inunctions) gained weight, but disease in larynx increased. Six months later epiglottis so infiltrated and overhanging that it resembled typical "turban-shape." Discomfort in swallowing. No evidence of pulmonary tuberculosis. Anti-luetic treatment abandoned.

A year after first observation, stridor on talking. Glottis narrowed by flabby,

lupoid infiltration which had changed epiglottis and aryepiglottic folds into sausage-like swellings, partly concealing cords. On the centre of left ventricular band, whitish, indolent ulceration (see fig.) General health good despite limited activity entailed by laryngeal stenosis. Diagnosis: lupoid tuberculosis. June 11, 1923, low tracheotomy, local anæsthetic (W. H.). The prompt result was striking. Within a month voice improved, good glottic space, but the conditions, though subsiding, were as recorded. Six months later 14 lb. heavier than when first seen; had resumed golf; no dysphagia; voice nearly clear. Tracheotomy tube abandoned on April 15, 1924, worn for ten months.



Lupoid tuberculosis producing stenosis of larynx and requiring tracheotomy. Cure. (Drawn by Dr. P. Watson-Williams from a sketch by Sir StClair Thomson.)

To-day voice good, respiration free, epiglottis returned to well-defined outline, but trifle of fibroid thickening on left side. Cords are white, broad, clean. A little general thickening of both arytenoids and ary-epiglottic folds—doubtless fibroid. Smokes, plays golf, enjoys excellent health.

DISCUSSION.

Dr. JOBSON HORNE said he took exception to the entitlement, "Case illustrating the advantage of Tracheotomy as a Curative Measure for Tuberculosis of the Larynx," on the ground that there was no evidence of tuberculosis. Given an error in diagnosis, it was possible, he said, to cure anything. The larynx that day did not present any clinical appearance of past tuberculosis that had healed. Sir StClair Thomson himself had regarded the appearances before tracheotomy as "typical of lupoid tuberculosis." The use of the term was evidence of uncertainty and it shook the diagnosis of tuberculosis. Moreover, the notes stated that no evidence of pulmonary tuberculosis could be found. With the disease in the larynx so extensive as had been described, if it had been due to tuberculosis one would have expected to have found some evidence of the disease in the lungs. On the other hand, there was a definite history of syphilis. Dr. Horne said he had no intention of opening up a discussion on the relationship of lupus to syphilis and tuberculosis, but he would remind them that in lupus there was a tendency through atrophy and otherwise for the larynx to recover itself apart from treatment; moreover, in syphilis as well as in lupus, tracheotomy, by placing the larynx at rest, helped to bring about a cure by Nature.

For the reasons stated, he considered the claim for tracheotomy as a curative measure for tuberculosis of the larynx had not been made out. He was prepared to say that in quite a small number of cases of tuberculosis of the larynx tracheotomy might be necessary to overcome obstruction and stenosis, but not as a curative measure. Speaking generally, in typical tuberculosis of the larynx, in which syphilis was not a factor, once the tracheotomy tube was in the end was not far distant.

Sir JAMES DUNDAS-GRANT asked what was the degree of stridor in the case. If there was stridor during phonation, there was probably some obstruction below the vocal cords, and he thought that a strong indication for tracheotomy; probably Members who saw the condition would have done it at once. Obviously the lungs were not affected to any considerable degree. The result, he considered, was brilliant.

As Dr. Jobson Horne said, there was the question of diagnosis, and he (the speaker) did not think Members were in a position to judge now as to whether the infiltration was tuberculous or syphilitic. He had seen cases of lupus of the larynx tracheotomized with great advantage, the patients living many years afterwards. He had been obliged to do tracheotomy in a case of tuberculosis of the larynx in which the vocal cords were fixed in such a position that there was inspiratory stridor. Soon after the operation the cords became mobile, but there was extensive disease of the lung, and the man ultimately died, though of asphyxia.

Sir WILLIAM MILLIGAN congratulated both the patient and his surgeons on the result. But was the condition tuberculosis? Was it not possibly a parasyphilitic condition? There was the history of infection of the finger with lues, and a negative Wassermann did not prove it was not specific.

The profession had been educated in too slavish a manner in the idea that tracheotomy should not be done for tuberculosis; he believed that to be a mistake; there were cases in which it was required, not only to relieve obstruction, but to secure physiological rest to the larynx. Dr. Jobson Horne said that if tracheotomy was carried out in a tuberculous patient, he seldom lived long afterwards, but he (the speaker) recalled a bad case of tuberculosis of the larynx in which the patient lived nine years after tracheotomy. The late Dr. Dreschfeld said that the nearer tuberculosis was to the surface of the body, the more dangerous it was. The man to whom he was referring had very limited tuberculosis at the base of one lung but his larynx was severely involved and the introduction of a tracheotomy tube gave great relief. The tube was removed a year later, and the patient lived eight more years. He (Sir William) had never since then worked slavishly on the idea that tracheotomy was to be avoided in tuberculosis of the larynx. The point deserved to be fully ventilated. He admitted the operation was most inadvisable when there was very active disease in the lung, as it acted as an irritant, and the coughing and discomfort were much increased.

Mr. J. R. L. (the patient) said he felt convinced that if tracheotomy had not been done he would not now have been alive. The stridor he suffered was very severe. Three or four days after it was done he ate well, and had no dysphagia nor stridor. He received instructions in regard to certain matters, and he carried them out. Silence was hard to observe, but he communicated his wishes and thoughts only in writing, even when playing golf, which he did twice a week. He had had his thumb infected in practice twenty-four years ago, and he was under Sir Jonathan Hutchinson and Mr. Jacobson. All he had was a hard sore and an enlarged supracondylar gland, and a sore throat. He took mercury for two and a half years, and Colonel Harrison gave him injections. He was an old athlete, and had led a very healthy life.

Sir STCLAIR THOMSON (in reply) said he was not recommending this procedure as a panacea. In his experience, tracheotomy was only indicated in about one case out of 200 of laryngeal tuberculosis. If it seemed suitable in certain cases, its advisability should be carefully considered. On the left ventricular band there was a grey ulcer, and a shallow ulcer on the laryngeal surface of the left ary-epiglottic fold. Clinically, the appearances were almost typical of tubercle (see fig.). Tubercle was of all degrees, from lupus to so-called "galloping consumption"; some patients were dead in three weeks, others who had had tuberculosis of the larynx had lived twelve and fifteen years.

The present case was brought forward because of the case Sir James Dundas-Grant showed at the last meeting, which he (the speaker) regarded as tubercle.[1] He (Sir StClair) said there was stridor, but there were no physical signs in the lung, nor a temperature, nor bacilli in the sputum. He strongly recommended a trial of tracheotomy in that case. The stridor on

[1] See *Proceedings*, 1924-25, xviii (Sect. Laryng.), p. 4.

talking to which he (Sir StClair) referred was that which occurred when there was a little extra exertion. Stridor on talking was an indication for tracheotomy, as also was stridor occurring in sleep.

There still existed the old prejudice against tracheotomy *qua* tracheotomy. He agreed there was very little call for it in tubercle. It was recommended forty years ago by Moritz Schmidt, before sanatoriums were established, and before the silence treatment was instituted. Now, silence produced nearly all that was formerly achieved by other means. But when there was stridor, tracheotomy should be done.

Microscopical Section of Laryngeal Tumour, from a young child.

By HERBERT TILLEY, F.R.C.S.

B., AGED 3, male.

History.—Crowing inspiration at night two to three months, during day for past fortnight. No distress on exertion. October 29 : direct endoscopy showed a pendulous growth on the upper and inner aspect of the right arytenoid. Removed by Paterson's forceps. Immediate disappearance of symptoms.

Examination.—Naked eye : translucent in appearance like small nasal polypus.

Microscopical Report (by H. G. BUTTERFIELD, M.D.) :—

"The material received for examination consisted of two portions of tissue, the larger of which measured approximately 9 mm. in length and was roughly fusiform in shape ; the general resemblance to the naked eye was that of a rather firm fibrous nasal

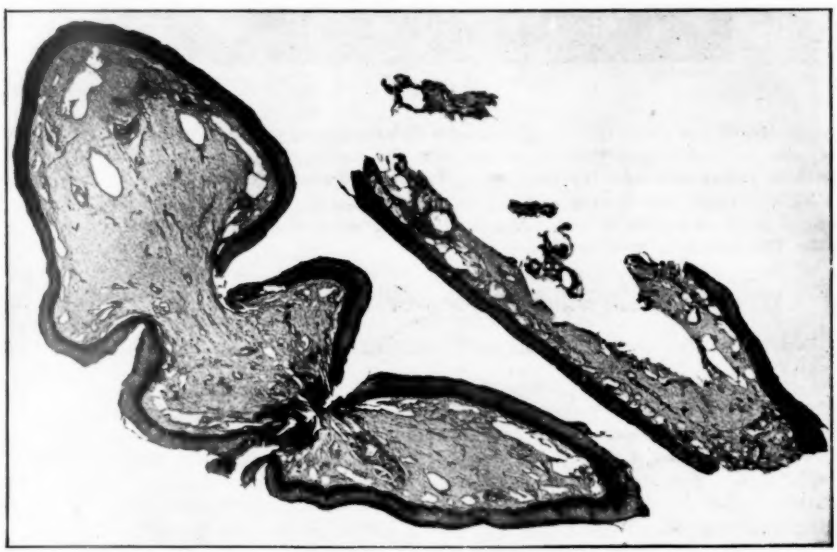


FIG. 1.—Angio-fibroma from the arytenoid region in a young child. (For description see text.) × 20.

polyp. Both pieces were imbedded in paraffin, and sections cut longitudinally (see fig. 1). The outer covering of the growth is an even layer of stratified squamous epithelium showing no papillæ and no signs of either proliferation or ulceration. The only break in its continuity was caused by the manipulation necessary for its removal. The main mass of the tumour is composed of a reticulum of fine fibrous tissue. Among this are seen numerous thin-walled

blood spaces, the majority of which occur towards the periphery and here and there appear to be the foci of ordinary round-celled infiltrations. The walls of the blood-spaces appear to be only one cell in thickness, but one or two larger vascular structures are to be seen which represent the arterial blood-supply. There is nothing in the appearances of the cells of this

FIG. 2.

connective tissue or of the vascular spaces which suggests malignancy (see fig. 2). There is not an undue proportion of nuclei, and the staining reactions are those of normal tissues. The character of the tumour may best be expressed by calling it an angiofibroma."

Mr. TILLEY said he believed the condition to be unique. He used the small direct speculum of Chevalier Jackson and a mere spot of blood marked the insertion of the growth after this had been removed.

Congenital Stridor or Laryngeal Web.

By PHILIP FRANKLIN, F.R.C.S.

FEMALE, at 7½ months, inspiratory stridor, fortnightly attacks of dyspnœa.

Direct examination without anæsthetic—no accurate observation made.

Stridor not urgent. At 1 year 11 months stridor much the same—child well developed, healthy, no cause for anxiety; unable to cry; no voice except occasional high-pitched note. Direct examination, general anæsthetic, webbing of anterior one-third of cords in appearance a dense white union, much like scar tissue and not a bluish membrane.

DISCUSSION.

Dr. DOUGLAS GUTHRIE said he had under treatment a similar case, in a baby of only 2 months. It was brought with the history that it had suffered from difficulty in swallowing ever since birth. The difficulty of examination by the direct method without an anæsthetic in a small infant was very great, largely in the matter of controlling it.

Dr. W. H. KELSON said it was easy to cut such webs, but not easy to prevent subsequent joining up. He had a case in a man who had suffered from epithelioma of the anterior commissure, which he (the speaker) removed. The anterior part of the vocal cords persisted

in joining up afterwards. He did not know whether it would be possible to get one cord covered with epithelium, by skin-grafting, or otherwise. Sir William Milligan told him he had a tube which he used successfully in such cases.

Mr. E. D. D. DAVIS suggested that this patient should be examined by suspension laryngoscopy or by Haslinger's directoscope and the web excised. Excision could be repeated if necessary, but if an intubation tube were to be left in the larynx ulceration and stenosis would be liable to occur. Children were more easily examined by suspension laryngoscopy than adults.

Mr. HAROLD BARWELL said he had had children patients of three and four years of age who had done very well with treatment by prolonged intubation.

Mr. J. F. O'MALLEY said he had had a case in which a toy-gun bullet went across the larynx in a child. Acting on advice, he made an incision, and he had an anxious time with it. After settling down the adhesion appeared again.

Sir JAMES DUNDAS-GRANT said it was important to foster the growth of the larynx; he therefore suggested breathing exercises, and encouraging the child to shout, in the hope that when the larynx enlarged as a whole, the web would not increase with it. There was enough mobile cord to produce a voice. He would avoid operative interference unless it were called for by difficulty in breathing.

Mr. FRANKLIN (in reply) said that he agreed with Mr. E. D. D. Davis that if anything was to be done, suspension laryngoscopy was the right method, though he did not agree with that gentleman regarding after-treatment by intubation. He had had considerable experience of intubation in children, even under a year old, and he did not observe any trauma or subsequent scarring after the use of an intubation tube. He would incise the web.

Case of Thyroglossal Cyst.

By F. J. CLEMINSON, M.Ch.

A. W., 28, married woman. Shown at February meeting, 1924. Cystic swelling in thyrohyoid region mainly to left of mid-line and very slight swelling of left ventricular band, of bluish colour, traceable outwards under left aryepiglottic fold.

March 29: Cystic tumour dissected out. Found to have no connexion with the tumour of larynx. Hyoid bone divided. Stalk running downwards divided and ligatured.

Section: Thyroglossal cyst.

Present Condition.—Left ventricular band bluish-purple swelling, somewhat larger than on last occasion when patient was shown.

Dr. IRWIN MOORE said he considered that this was a typical diffuse angioma or telangiectatic tumour.

Case of Papilloma of Larynx.

By F. J. CLEMINSON, M.Ch.

W. T., 72, male, retired police officer.

History.—Shown by Mr. O'Malley here on May 5, 1922. Had been hoarse some months. Irregular elevation inner part anterior half right vocal cord, spreading to anterior part left cord. Opinion then was that it might be malignant, and that thyro-fissure should be done.

June 20, 1922, Middlesex Hospital: Portion of growth removed by indirect method, simple papilloma.

Since then growth gradually increased in size, extends now over anterior two-thirds of right cord and anterior one-third of left cord, and is more prominent. No limitation in movement; pinkish colour, surface granular.

DISCUSSION.

Mr. A. J. WRIGHT said he considered the case malignant, and strongly advised removal.

Sir JAMES DUNDAS-GRANT said that if laryngo-fissure were done in this case, it should be carried out in accordance with Chevalier Jackson's latest recommendation, because if one went boldly through the thyroid cartilage, one would cut into the middle of the tumour. Jackson had shown that with a small saw the operator could cut down through the cartilage without opening through the soft parts, and when the cartilage had been separated, the soft parts could be detached, and one could judge as to how much could be removed. He was not sure this growth was malignant.

Mr. J. F. O'MALLEY said that two years ago his view was that the growth was carcinoma, and in this he was supported by some Members of the Section. The man's teeth were bad, and he advised their removal. The case then came under Mr. Cleminson, who obtained a report on a portion that it was papilloma, but the growth was now considerably larger, and he still suspected it was malignant.

Dr. IRWIN MOORE said he did not think the growth was attached to the cord; he believed it emanated from the sacculus laryngis; he took it to be a chronic hyperplasia of a fold of the sacculus, or else a fibroma. He thought thyro-fissure was indicated in this case.

Mr. CLEMINSON (in reply) said he considered it was on the border-line between a malignant and a simple papilloma; at present the cord was not infiltrated. If left for a year it might become definitely malignant; therefore it was safer to do thyro-fissure, and he would remember to do it as Sir James Dundas-Grant had suggested, so as not to wound the tumour by the initial incision.

Section of Laryngology.

President—Dr. A. Logan Turner.

Two Specimens with Sections of Salivary Glands removed from the Tonsil Bed, and One Section of Salivary Gland removed from the side of the Tongue adjoining.

By H. M. Wharry, F.R.C.S.

Specimen I.—Removed (H. M. W.) from left tonsil bed of a baby, aged 10 months, suffering from enlarged glands in neck. Left tonsil, which was only one quarter the size of the right, removed intact; tumour found beneath it, on section salivary gland.

Specimen II.—Removed by Mr. Julian Taylor from child aged 12. Tonsil removed intact, section made because of the abnormal appearance of one end. Abnormal part proved on section to be salivary gland, separated from the lymphatic portion only by a thin strip of fascia.

Specimen III.—Section of tumour removed by Mr. Julian Taylor from side of tongue adjoining tonsil bed in a child. Section shows a salivary gland.

The sections in each case are those of fully functioning salivary glands.

Discussion.—Mr. H. J. Banks-Davis asked whether these specimens were shown with the object of giving some explanation of the throat dryness occasionally seen after enucleation of tonsils. If in this kind of case the tonsil contained salivary gland, evidently entire removal would result in subsequent dryness.

Dr. Logan Turner (President) asked whether Mr. Wharry was able to satisfy himself that the submaxillary and sublingual glands were in their due place in this patient, or whether the glands removed with the tonsils were the only examples the patient had of this group of salivary glands. The salivary glands were evaginations from the buccal mucous membrane. In some animals, such as the cat, the sublingual gland was retro-lingual in position, hence an aberrant position in the human subject was possible.

Dr. Salisbury Sharpe asked whether the enucleation of these tonsils was accompanied by more hæmorrhage than was usual, as the salivary gland seemed to be supplied by the same vessels as the tonsils.

Mr. Herbert Tilley (replying for Mr. Wharry) said that the salivary gland tissue was encountered unexpectedly when the tonsil had been enucleated in the ordinary way. A fat-like substance was seen bulging into the lower part of the fossa, and he found this to be salivary gland structure. A similar experience happened to Mr. Julian Taylor; there was no abnormal hæmorrhage.

Extensive Tuberculosis of the Larynx healed by Sanatorium Treatment, Silence and the Galvano-cautery.

By Sir StClair Thomson, M.D., F.R.C.S.

A. H. P., aged 36, male, was admitted to Sanatorium (Midhurst) in December, 1922, with tuberculosis, Group III, i.e., two lobes affected each side, tubercle bacilli in sputum. Left half of epiglottis purplish, congested, infiltrated, ulcerating on laryngeal surface. Both vocal cords, in their whole extent, largely destroyed by mouse-nibbled ulceration and overlapped by hypertrophy of ventricular bands. (He had continued talking much in his business.) Some thickening of left ary-epiglottic fold.

Maintained strict silence for ten months. Treatment by galvano-cautery employed in 1923 in April, June and July.

At present gained 22 lb. in weight: steady temperature; no cough; arrested lungs. Left vocal cord quite normal; right cord intact and narrow. Much limited in its excursion except in abduction, owing to scarring. Scarring also prevents the right ary-epiglottic fold from coming forward and hence sinus pyriformis not so visible on that side, but cords meet well. Faint suspicion still of thickening in outer part of left ary-epiglottic fold, some compensatory hypertrophy of left ventricular band.

Lives in open-air hut: has resumed profession as a showman of sea-lions since Christmas.

Discussion.—Mr. HERBERT TILLEY congratulated Sir StClair Thomson on one more of his series of cases, which proved the necessity of silence treatment in this type of the disease. One case shown by himself (the speaker) at the Section was galvano-cauterized, and the patient afterwards went to Midhurst. She was still at work, and it was now practically impossible to detect which cord had been punctured. In her, the lung condition seemed to be localized and quiescent. It seemed that the œdematous mucous membrane in the present case should be punctured, as it would probably complete the cure.

Dr. W. JOBSON HORNE said that a point often overlooked in cases giving successful results was that there might also be syphilis. Often such a history could not be obtained, and the Wassermann was negative, but the possibility of the combination should always be remembered.

Sir STCLAIR THOMSON (in reply) said his object in bringing the case was to call attention to the fact that this was a Group III patient—i.e., one of those patients who, on the average, would be, in 60 per cent. of cases, dead in three years if the larynx were free of disease, and 80 per cent. dead if the larynx was invaded. This was a case of very extensive tubercle, and the view taken of the case was at first not a hopeful one. Among the causes of success he placed the man's own cheerful personality, which was quite an asset to the other patients at Midhurst. He was on ten months of silence, and the cautery was not started until he had been there four months. If these patients had tuberculosis in the lungs, the older they were the more favourable was their case; but an elderly case with tubercle in the larynx and no tubercle in the lungs was a bad case, whereas an elderly person who had tubercle of lung and developed disease in the larynx was a favourable case compared with younger people.

He carried out the local treatment under cocaine. Some sensitive people had a hypodermic injection of morphia half an hour beforehand, and a 10 per cent. spray was used. He did it by the indirect method, and started carefully to see how the patient tolerated it. If both cords were affected, he applied it to only one at a time at first, and if it was well borne, he attacked both at later sittings.

"Puncture" was a good word, as he used only a fine-pointed electrode, and this was passed as far as it would go without encountering definite resistance; it felt as if it were being passed through butter. When the cautery reached healthy tissue, it required definite force to push it further, and at this stage it was withdrawn. If the whole cord was diseased, he made two or three punctures. Most disease was found in the vocal cords and the inter-arytenoid, and when there, the case was more favourable than in other quarters of the larynx.

He had used the method for relief to dysphagia in cases in which he had no hope of effecting a cure; he made a deep puncture in each arytenoid, or two or three in the epiglottis, and in some cases it greatly relieved the pain.

With regard to scarring, he had had a few cases which had healed spontaneously, and then there was slight stenosis; but when marked stenosis did occur, much of it was due to the galvano-cautery.

Mr. Tilley's patient came back to Midhurst after fifteen years, with a recurrence of disease in the chest, but there was no return of disease in the larynx. Thanks to the cure of her laryngeal lesion, she had been able, all these years, to earn her living as a schoolmistress.

With regard to a combination of syphilis and tubercle, he was not sure what Dr. Horne suggested, because, according to the books, one disease generally made the other worse. If it was suggested that improvement resulted, he had not examined his statistics enough to enable

him to be sure on the point. His impression was that if syphilitic patients acquired tubercle, the laryngeal condition was slightly mitigated, and was more likely to be of the fibroid type.

Fibro-sarcoma of Nasal Pharynx Treated by X-rays and Radium.

By NORMAN PATTERSON, F.R.C.S.

BOY, aged 18 when first seen, December, 1917. Referred by Mr. Russell Howard. Swelling above and below zygoma, the naso-pharynx was blocked by a large tumour. Pain and epistaxis. Some operation previously performed.

December 14, 1917 : 50 mgm. radium inserted into external tumour twenty-four hours; subsequently X-rays applied. Radium and X-rays repeated. The tumour gradually disappeared, no recurrence.

Discussion.—Mr. HAROLD BARWELL asked why Mr. Patterson named this fibro-sarcoma rather than fibroma. These cases showed the quickly growing fusiform cells of young fibrous tissue; but they did not form metastases, nor did they occur as secondary deposits in the glands.

Mr. E. MUSGRAVE WOODMAN said that microscopically these tumours looked like vascular, rapidly-growing fibromata, and they did not form metastases nor involve the glands, but locally they were very malignant; they recurred many times and gradually spread and killed the patient unless they were cured. His only case of fatality in the upper jaw was a case of the kind in a child of 15. On his beginning to remove the tumour the bleeding was so severe that he had to stop. Radium was used, but a month later the patient was dead.

Mr. E. D. D. DAVIS said he thought a distinction should be drawn between fibromata occurring in the naso-pharynx, and the very malignant round-celled sarcomata which produced the so-called "frog-face." The radium application caused sarcomata to disappear like melting ice, but the patient died of secondary deposits in other organs. He had one distressing case in a boy aged 17, a lympho-sarcoma which disappeared quickly after the insertion of 50 mgm. of radium, and he was comparatively well; but in five weeks he returned looking very anæmic and ill, and in four months he died of diffuse sarcomatosis.

Mr. NORMAN PATTERSON (in reply) said that it was difficult to decide whether these growths were sarcomata, fibromata, or fibro-angeiomata—he had heard them called all three by eminent pathologists. Locally, they were malignant, and attained a great size, and that form of tumour eventually produced "frog-face." If this boy had been left alone he would have been dead years ago. He thought the term fibro-sarcoma was an appropriate one: the tumour responded to radium, and he did not think fibroma responded in the same way.

A Large Laryngeal Growth.

By NORMAN PATTERSON, F.R.C.S.

WOMAN, shown by Dr. Bond, in May, 1902.[1] Extract from *Proceedings*: "Husky voice, thirteen months, no cough, no difficulty nor pain in swallowing, no night sweats, temperature normal. One severe attack of stridor, no enlarged glands: on right side of larynx large red mass, firm on palpation, extending from below cord to level of epiglottis. Right arytenoid and ary-epiglottic fold merged in mass." In discussion general opinion favoured tubercle. One member suggested sarcoma. Dr. Bond would not commit himself. Eighteen years ago, Dr. Bond

[1] Bond (J. W.), "Case of large laryngeal growth in a woman." *Proc. Laryng. Soc., Lond.*, 1902, ix, p. 124.

performed tracheotomy as patient was in poor state of health. Tumour still present, exhibitor thinks it must have enlarged considerably. The patient will not consent to removal.

Discussion.—Dr. LOGAN TURNER (President) said he thought it might be a chondroma of the larynx. He did not investigate the feeling of resistance which it offered; this was an important diagnostic point.

Sir JAMES DUNDAS-GRANT said that if one saw this case for the first time now, without learning the history, one would say—as he did twenty-three years ago—that it looked like sarcoma. In view of the history, however, that idea could not be persisted in and the growth might possibly be an enchondroma.

Dr. W. H. KELSON said he thought it might be a lipoma with a good deal of fibrous tissue in it. Mysterious swellings of this description had been shown from time to time, and they usually turned out to be lipomata.

Mr. NORMAN PATTERSON (in reply) said he did palpate this tumour, and on the surface it was rather soft. He might be able to get permission to remove the tumour, and if so, he would show the case again.

Diffuse Osteomyelitis of Skull from Acute Frontal Sinus Suppuration.

By H. BELL TAWSE, F.R.C.S.

C. M., AGED 19, male.

June, 1923, severe pain bridge of nose and left orbit, high temperature, delirium, no nasal discharge.

July 9: Abscess left inner canthus burst. Few days later tender swelling over forehead, followed by one on top of head and another further back, both in middle line.

July 26: Went to seaside and returned August 8. During this time almost continuous headache and feeling of weight on top of head, and swellings increased.

September 3: First seen by exhibitor.

Nose shows marked septal deflection to left, middle turbinal not seen. No pus visible right or left. Discharging sinus left inner canthus. Marked thickening over frontal eminences—two swellings size of pigeon's egg in mid-line extending from this thickening nearly to occiput.

Skiagram showed pus in left frontal sinus, doubtfully in right. Numerous sequestra scattered from front to back of skull.

September 5: Swellings incised, glutinous pus, numerous sequestra removed, left frontal sinus opened and drained.

September 24: Resection of septum and amputation of anterior ends of both middle turbinals.

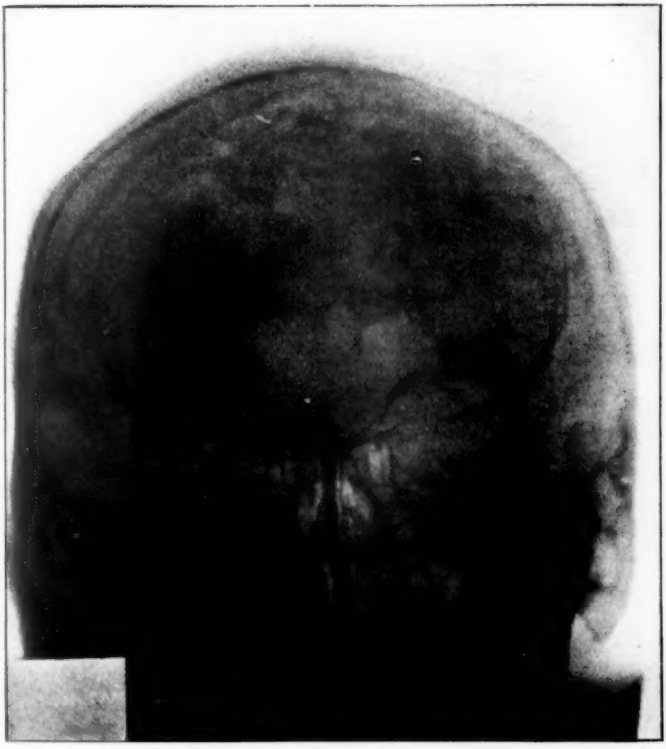
November 5: Boggy swelling in mid-line of forehead opened by extending original incision. Left frontal sinus again opened up and incision joined to vertical one. Intersinus septum necrosed, pus and polypi both sinuses, dura mater exposed by disease in posterior wall of left.

Howarth's operation left, intersinus septum removed, drainage tubes passed into both nasal cavities and secured, external drainage of both sinuses. Uninterrupted recovery.

Interesting features were the extensive osteomyelitis involving the whole frontal bone and much of the parietal, without apparently any rise of temperature, &c., after July 20.

Pathological Report: *Staphylococcus aureus.*

Discussion.—Mr. HERBERT TILLEY said he had often had the impression that if one saw a case of early frontal osteomyelitis after it had been started by disease one could cure it by operation. He had published in the *British Medical Journal*, 1917, a case in which there was most extensive disease. He made the incision in the hair-line from the parietal bone to the level of the zygoma and forward to the eyebrow so as to throw the flap towards the middle line. The patient was a lady in the prime of life, and the line of that incision could not now be seen. He had to take away the whole of the left half of the frontal bone; the dura mater was adherent to the cortex and portions of these came away with the diseased bone. When the limit of the inflamed bone was reached, the wound was lightly stitched up, and she recovered.



September 3, 1923.—Pus and sequestrum left frontal sinus; pus, right frontal sinus; osteomyelitis of frontal and parietal bones with numerous scattered sequestra.

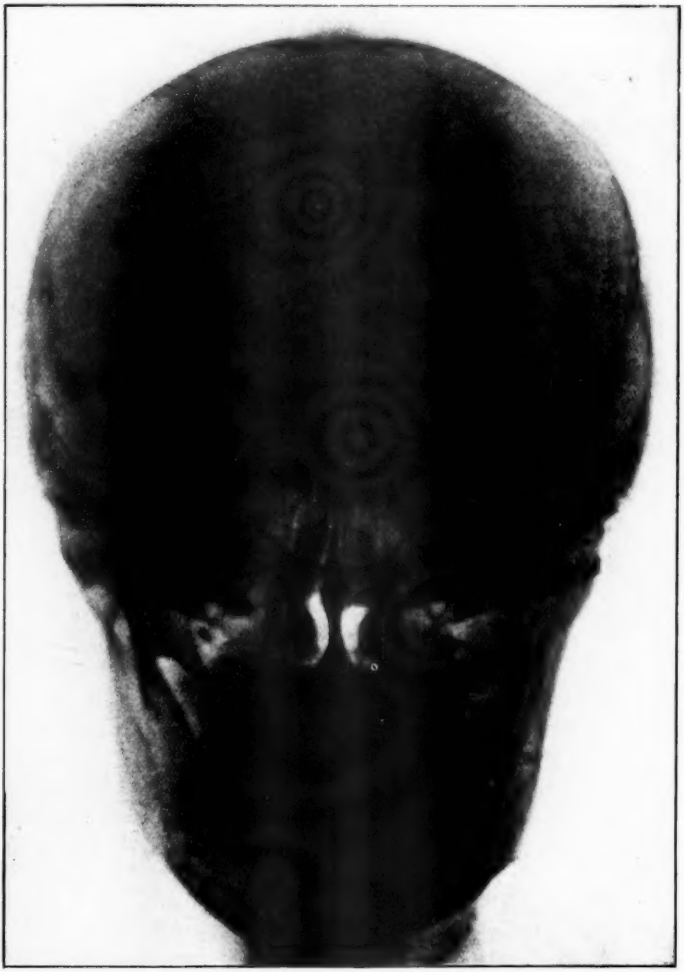
Osteomyelitis which started secondarily to operation he had rarely seen cured, except in one or two cases which Mr. Mollison showed. Mr. Tawse had obtained an excellent result in his patient.

Mr. NORMAN PATTERSON said he had had a case which was operated upon for disease of the frontal sinus at Shanghai. The patient came to England, and underwent a second operation, which was done by Mr. Lambert Lack. During Mr. Lack's absence he developed more trouble in both sinuses, and the speaker took him over. Extensive osteomyelitis occurred, and finally the whole frontal bone was taken away. The patient was now back at Shanghai, and was very busily engaged on important work. He was quite well.

Dr. LOGAN TURNER (President) said he did not consider the prognosis of these cases was as bad as Mr. Tilley implied; he (the speaker) had notes of nine recoveries from spontaneous,

and four from post-operative osteomyelitis, gathered from the literature and in answer to a questionnaire which he had sent out.

Mr. A. J. HUTCHISON said he hoped to be able to show a case of post-operative osteomyelitis which he hardly dared yet to claim as cured, though he trusted it was. It started with the extraction of a tooth, and both antra, both frontals, the ethmoids and both sphenoids



February 1, 1925.—Healing complete. Whole frontal bone has been affected, and is now enormously thickened.

were involved. The last of several operations was done two months ago, and since the scars healed the man had been apparently well.

Mr. TAWSE (in reply) said that, contrary to what the notes stated, the occipital bone was not involved, but the entire frontal bone was. He made a long incision through two big

swellings from the parieto-occipital suture down to the frontal region, opening up the left frontal sinus, through a separate incision, and he then put in tubes. He did nothing more for some weeks. He did no chiselling and no curetting. After all had become clean he opened it up again, and dealt with the right frontal sinus.

February 1, 1925.—Very slight deformity after double frontal sinus operation. The median vertical incision made to drain the abscesses and remove sequestra was utilized to deal with the right frontal sinus.

Specimens and Microscopic Slides of Polypus of Nasal Septum.

By H. Bell Tawse, F.R.C.S.

(1) Female, aged 29, epistaxis so serious during confinement that she was almost pulseless. Removal twenty-four hours later.

Pathological Report.—Hæmangeioma.

(2) Female, aged 34, free epistaxis in last fortnight of pregnancy; polypus blocking anterior choana.

Pathological Report.—Capillary angeioma.

(3) Female, aged 36, occasional epistaxis during last month of pregnancy; free bleeding during confinement, and to a less extent for three months after; polypus blocked anterior choana.

Pathological Report.—Sections show very vascular, fibrous and rather dense granulation tissue which has resulted from the superficial erosion of a simple muco-fibrous polypus. The remains of the epithelium, though disordered and inflamed, is typical in construction. All three polypi arose from the same area—the anterior quarter of the septum.

(4) Woman, aged 55. *Pathological Report.*—In the sections one area shows an irregular adenomatous condition, due apparently to a proliferation of mucous gland tissue. Another area shows an angeiomatous condition.

Rhinolith of Large Size.

By T. B. Jobson, M.D.

Miss H. B., aged 60, female.

History.—Obstruction and discharge of yellow matter from left nostril over thirty years. Of late years obstruction had been complete. Necessity of wiping her nose frequently had become a social disability. No pain or headache.

Examination.—Transillumination, dull on left. Large polypi blocking left nostril.

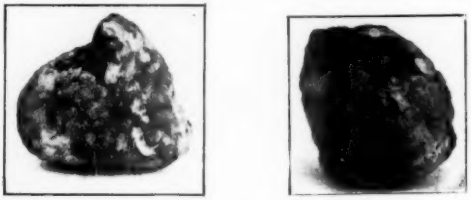
Treatment.—Caldwell-Luc operation. After removal of polypus a large rhinolith found. Antrum full of thick caseous pus. Rhinolith removed through nose with some difficulty, a few fragments breaking off in the process.

The main mass exhibited weighs 157 gr. On section it shows a light coloured friable core—the exact nature of which is unknown.

Chemical composition : calcium phosphate and carbonate.

Discussion.—Dr. WILLIAM HILL said he had had two cases of rhinoliths, which he had been obliged to crush. This case looked like rhinitis caseosa, and the association was fairly common. He did not know which appeared first. Caseous rhinitis of the antrum had been recorded many times when there were rhinoliths of any considerable size.

Dr. W. T. GARDINER said that four years ago he operated on a case which was thought to be one of a large sequestrum of the nasal septum with a defect in the hard palate. Wassermann reaction negative. Access was obtained through the antrum but the sequestrum



Rhinolith—actual size.

could not be removed. A fortnight later he tried partly through the antrum and partly through the hole in the palate. The sequestrum broke up and proved to be a rhinolith the nucleus of which was a bone trouser button. There had been a unilateral foul discharge for over forty years.

Dr. JOBSON HORNE said that in a case he had had the rhinolith had formed around cotton wool, and there was a good deal of rhinitis caseosa, which he considered was secondary to the rhinolith.

Mr. J. ALDINGTON GIBB said that whilst he was a house surgeon a spur in a lad's nose was spokeshaved, not treated by septal enucleation. Six months after, there was rhinitis caseosa, caused by the spur becoming wedged in the nose. Its removal brought about a rapid cure.

Mr. G. WILKINSON mentioned a case in a child in which he found the nucleus was a piece of rolled-up newspaper, which had become impregnated with calcium salts. It closely resembled, at first sight, an elongated sequestrum of bone.

Dr. JOBSON (in reply) said that he had had the rhinolith bisected. It showed a central core the nature of which he did not know. The antrum was full of caseous material—such as was found in rhinitis caseosa.

Larynx Removed, for Malignant Disease, under Local Anæsthesia.

By W. S. SYME, M.D.

LOCAL anæsthesia employed because of debilitated state of patient, man aged 50. Anæsthesia, induced by infiltration of 1 per cent. novocaine solution, was altogether satisfactory. The man, however, died of heart failure the following day.

Atrophic Rhinitis treated by the Implantation of a Piece of Rib Cartilage.

By W. M. MOLLISON, M.Ch.

MISS D., adult. For years has suffered from atrophic rhinitis with large crusts which have caused much discomfort by falling into naso-pharynx.

On December 18 last, the tissues on floor of nose lifted up through incision under upper lip, and a piece of rib-cartilage inserted along floor of nose.

Since this there have been no crusts formed and the patient finds great relief.

Discussion.—Sir JAMES DUNDAS-GRANT said the nasal cavity was still large enough to be compared with that in ozœna. It seemed as if the cartilaginous graft had some dynamic effect. Sometimes the injection of a very small quantity of solid paraffin produced a marvellous improvement; even though it was too little to make any appreciable difference in the size of the cavity. At present the mucous membrane looked as if it could scarcely have been the seat of atrophic rhinitis.

Mr. PHILIP FRANKLIN said that, in our present state of knowledge, caution was necessary in ascribing cures to any particular treatment whether operative or palliative. Some cases underwent spontaneous cure, and others reacted favourably to various kinds of treatment.

Mr. E. MUSGRAVE WOODMAN congratulated Mr. Mollison on his result. He (the speaker) said he had inserted cartilage into the nose in ozœna. In two cases improvement resulted, but the cartilage sloughed and came away through a hole in the septal wall. He was careful not to perforate the muco-periosteum. There was much transmigration of sepsis through the naso-septal wall. The method in this case was better.

Dr. W. H. KELSON said this raised a large question. Anyone reading these notes might think there was a suggestion that reducing the size of the cavity had cured the atrophic rhinitis. He asked whether Mr. Mollison meant that. He (the speaker) did not think that atrophic rhinitis with its very peculiar odour could be cured by merely reducing the size of the nasal cavity. He had never seen such a case.

Dr. W. S. SYME said a larger piece of cartilage would have reduced the lumen of the nose still further. He still treated cases by displacement of the antro-nasal wall, and the results in the cases he reported to the Section last June had so far been maintained. It was necessary to wait two or three years to see whether there was any recurrence of symptoms. In some of the cases so treated there was considerable retraction for a time, there was no further crusting formation, and the sense of smell had returned to some. The appearance of the nose suggested that there was at least an œdematous condition which kept the mucous membrane moist. The operation could be done under local anæsthesia, and was not difficult.

Mr. J. ALDINGTON GIBB asked whether the narrowing of channel was supposed to bring about the destruction of the causal micro-organisms in the disease. If this was not effected the case could not be said to be cured.

Carcinoma of Left Vocal Cord, One Year after Operation.

By W. H. JEWELL, O.B.E., M.D.

W. D., MALE, aged 61, previously shown February, 1924, when general opinion was in favour of epithelioma: opinion also expressed that the infiltration was subglottic.

Laryngo-fissure performed February, 1924. Larynx and skin united by sutures at time of operation and tracheotomy tube left in for twenty-four hours. Larynx and rings of trachea ossified.

Pathological Report.—Squamous-celled carcinoma.

Discussion.—Mr. G. W. DAWSON said he remembered the case and the growth was an extensive one. He had seen one or two others even more extensive that had done perfectly well on the conservative operation as opposed to the major operation of laryngectomy.

Mr. JEWELL (in reply) said that the tracheotomy tube was left in twenty-four hours, and there was no trouble. He did not think there was any immediate need to leave the tube in, but as he was not going to see the patient for a week, thought it would be safer to leave it in. The cartilages of larynx and trachea were ossified. One or two spicules came through, but there was no trouble. No resection of lateral plate of larynx. Patient made an uninterrupted recovery, taking liquids the evening of the day of operation and semi-solids the next day. Highest temperature 99° F. Patient said his voice was better than before the operation, and at times he had a normal voice. Respiration was free and tranquil, and there was no shortness of breath. On laryngoscopic examination there was no evidence of recurrence.

Nasal Growth: Sarcoma of Ethmoid: Removal by Lateral Rhinotomy.

By IRWIN MOORE, M.Ch.

FEMALE, aged 19, previously shown November 7, 1924, "Case of Nasal Growth" (*vide Proc. Roy. Soc. Med.*, 1925, xviii, Sect. Laryng., p. 1).

Moure's lateral rhinotomy, November 20, fully exposed growth. Fibrous hardness and smooth surface of anterior portion confirmed, remainder found to be soft, cystic, and easily broken down. Originated from right ethmoid cells, which had been destroyed. Sphenoidal sinus also involved. The right antro-nasal wall partly absorbed by pressure of growth, which occupied the greater portion of the maxillary antrum. Cartilaginous portion of septum, and greater portion of vomer also absorbed. Soft parts stretched across the left nasal cavity, adhering firmly to the antro-nasal wall, while the site of the left nasal cavity was occupied by growth.

Microscopically, round-cell sarcoma (A. N. Leathem).

Recovery marred by severe outbreak of erysipelas—fourteen days after operation which attacked side of face involved by growth, but did not break down wound, nor interfere with operation area.

January 15 a tube of radium—34 mcs. strength, with 2 mm. lead screening—placed in nasal cavity twenty-four hours.

Naso-pharyngeal Growth.

By F. BRAYSHAW GILHESPY, L.R.C.P., M.R.C.S.

M. H., AGED 21. Seen early 1923, post-nasal space filled by very vascular growth, causing complete nasal obstruction. Malignant endothelioma. Radium 65 mgm. inserted with rapid disappearance of neoplasm. Seen one year ago with slight recurrence in same position. Radium inserted—35 mgm. twelve hours—again six months ago. X-ray photograph taken January 20, 1925, shows antra clear, ethmoids dim: nasal space appears filled up.

At present, growth seen in nose and roof of naso-pharynx.

Mr. E. MUSGRAVE WOODMAN said Mr. Gilhespy showed this case to him five months ago, with a small growth at the back of the nose, and he asked him to try further radium before submitting the case to operation, because of (1) the high mortality which took place when dealing with malignant growth at the back of the pharynx; contrasting with the results from removal of the upper jaw; (2) to get at it he did not think mere retraction of the palate was sufficient. At present there was a big growth at the back of the pharynx, and it had extended along the floor of the nose on the left side, and had not reached the antrum. He thought the best method of approach was by a combination of upper-jaw and post-pharyngeal operation, removing the upper jaw partially on the left side, leaving the orbital plate, and removing the hard palate on that side and leaving the soft palate. There need not be much loss of blood.

Sarcoma of Nasal Septum treated by Radium.

By PHILIP FRANKLIN, F.R.C.S.

PATIENT, male, aged 51. Case of Ulcer of the Nasal Septum (*vide Proc. Roy. Soc. Med.*, 1924, xvii, Sect. Laryng., p. 50).

Examination.—January, 1925. Destruction of septum extending from just beyond vestibule to within half an inch of posterior edge, and from roof to floor of nose. Examination under anæsthetic one week later; large piece of growth removed. Microscopical report—mixed cell sarcoma with a delicate fibrous stroma.

15 mgm. radium applied for twelve hours to roof, floor and posterior part of septum. Result satisfactory except for a portion in roof. Another application of radium will be given later.

Section of Laryngology.

President—Dr. A. Logan Turner.

Hoarseness Persisting for Ten Years.

By A. L. Macleod, M.B.

W. J. H., aged 68, male.

History.—Shown May 4, 1917. Hoarseness with general thickening of all structures of the larynx. Wassermann negative. Sputum negative to three examinations.

Thickening more marked, especially on the left side. Right arytenoid fixed. Some stridor but no distress.

Discussion.—Sir StClair Thomson said he saw this patient eight years ago, and that he was showing the sketch he made of the condition. In 1917 there was a history of hoarseness for two years, so the man had now been hoarse ten years, and, except for stridor, he was still walking about in apparently normal health. At that date all the air passages except the larynx were normal, and the note stated that he spoke easily, but with a husky voice—a "choky" voice, indicating that there was something subglottic. At that time both cords were moving freely, there was a superficial ulcerating deposit on the anterior two-thirds of the right cord, and an earlier similar condition on the anterior third of the left cord. He then told Dr. Macleod that if he could exclude tubercle it was probably malignant. Two Wassermann tests were negative; repeated tubercle investigations were also negative, and the man disappeared from view. To-day the right side of the larynx was fixed by a growth which was infiltrating the whole ventricular band and passing into the subglottic region; but there were no enlarged glands. It seemed to him (the speaker) to be malignant and very suitable for complete laryngectomy. If it should prove to be epithelioma, the slowness of growth was of interest as showing that one need not be in a hurry to interfere. Dr. Harman Smith published a case in which the patient had been husky thirteen years before a section was removed and reported to be epithelioma. In that case laryngectomy was done.

Both Morell Mackenzie and Semon were mistaken in saying that malignant disease affected chiefly the posterior part of the glottis. On the whole, malignant disease favoured the anterior end of the glottis.

Dr. Jobson Horne said the case reminded him of one he brought before the Laryngological Society of London in conjunction with the late Professor A. A. Kanthack in 1895.[1] Post-mortem histological examination of the larynx led them to describe the condition as pachydermia diffusa.

If tracheotomy became necessary it would be also beneficial in putting the larynx at rest.

Sir James Dundas-Grant asked when the last test for tuberculosis had been made; he did not think it was yet excluded. A modified form of it might produce such an appearance.

Postscript, April 22, 1925.—No examination for tuberculosis made since eight years ago. Patient died suddenly ten days after meeting. Larynx was removed for examination, and report will be given.

Laryngeal Stenosis of 18 years' standing treated by a Method of Intubation.

By H. Bedford Russell, F.R.C.S.

August, 1924, general anæsthesia, glottis just admitted a large probe. By means of probe a loop of silk was drawn through the stricture, and a No. 2 red rubber catheter pulled up into it.

[1] See *Proc. Laryng. Soc. of London*, 1895, ii, pp. 82, 83.

Larger and larger sizes gradually employed until the present one was tolerated.

Patient now breathes freely through larynx, and makes sounds faintly resembling voice (possibly by vibrating the ventricular bands). Arytenoids do not move, but a structure in the position of right vocal cord does so to some extent.

Advice sought as to when the tracheotomy tube may safely be discarded (if at all), and as to methods of vocal re-education.

Discussion.—Dr. LOGAN TURNER (President) suggested that it would be a danger to remove the tracheotomy tube in this case. The patient might wear a corked tube by day, and remove the plug at night. It was well to bear in mind the surgical axiom, "once a stricture always a tendency to a stricture." Cases of lye stricture in the œsophagus required dilatation many years after the initial treatment.

Sir STCLAIR THOMSON showed two pictures, remarking that Mr. Bedford Russell had evidently evolved a method which had already been carried out in America. Dr. Iglauer, of Cincinnati, had the same idea as Mr. Russell; he doubled the tube and tied it, so that it made a sort of folded pneumatic tyre. This was available when the stenosis was not complete and a tracheotomy was present. The Americans had given up the Schrötter tubes; they had no great success with laryngostomy, but they were successful with the method now described. He (Sir StClair) had been corresponding with Dr. Iglauer, who was more inclined to recommend the soft tube than the intubation tube, as dangers arose with the latter. Dr. Lynah, whose work he (the speaker) had seen in America, had had fifty cases of post-diphtheritic stenosis, thirty-two of which he cured; in eight the patients died from other causes; ten were still under treatment when he (Sir StClair) was there. Dr. Lynah considered no case cured until the patient had been two years without a tube; it took anything from three months to five years to restore a lumen.

Mr. BEDFORD RUSSELL (in reply) said that he thought he had come to the limit of dilatation of the larynx by the ordinary methods. He would certainly use a double tube. In 1910 thyrotomy was performed on this patient four times in six months for papillomata; a high tracheotomy had been done. In 1921 Mr. Harmer changed it to a low tracheotomy as a first step, since there was active ulceration in the larynx at that time. An anæsthetic was given her with a view of doing thyrotomy and removing scar-tissue, and treating the condition by the method of Moure, of Bordeaux. But that was a big procedure, and, with no certainty of result, he felt it unjustifiable; he therefore gradually used elastic dilatation. It was under a promise that the patient should have some voice that she allowed this to be done. She could make a definite vibration. It was, he thought, partly a question of educating the speech-centres in the cortex.

Specimen of a large Mixed Tumour of the Parotid Enucleated from the Soft Palate.

By E. D. D. DAVIS, F.R.C.S.

PATIENT, male, aged 67, sent with diagnosis of sarcoma of tonsil. Patient's friends noticed change in voice for three months but tumour not observed until seen by his doctor. Twenty years previously a mixed tumour was removed from the left parotid and the tumour capsule burst during its removal.

Examination.—A hard globular tumour (bluish lobe seen in specimen) occupied whole of left half of soft palate and fauces pushing the tonsil downwards and inwards. A nodular hard tumour could also be felt in left parotid. Lymphatic glands normal.

Treatment.—Intratracheal ether given and large crucial incision made in soft palate over tumour. It was then found that tumour could be shelled out by the fingers like a prostate.

Uninterrupted recovery. Now more than twelve months since operation, no recurrence, and scar on palate difficult to find. Section shown.

Mr. E. D. D. DAVIS said that Sir Harold Stiles showed, at Edinburgh, a very similar case, and about a month later this patient was sent to him with the diagnosis of sarcoma, but, having seen Sir H. Stiles' case, a provisional diagnosis of mixed parotid tumour was made, which proved to be correct, and the tumour shelled out easily. The patient had done extremely well.

Immobility of Vocal Cord.

By M. VLASTO, F.R.C.S.

PAINTER, aged 56. Hoarseness eighteen months. First under observation eight months ago. Laryngeal structures congested; left false cord swollen so that left true cord not seen. Marked pyorrhœa; teeth removed. Wassermann negative; chest normal. No dysphagia. Shortly after the first examination some of the laryngeal swelling subsided and it was then apparent that the left vocal cord was fixed.

The condition has not altered for the last six months.

Opinions are invited as to the diagnosis.

Discussion.—Mr. HERBERT TILLEY referred to a paper which he had written for the *Journal of Laryngology and Otology*, June, 1918, and in which he had followed up the after-histories of twenty-three cases of paralysis of the vocal cords, which had been seen in private practice during twenty years. There was no reply from ten cases, seven had completely regained movement of the cord, three patients died and three remained in the same condition as when first examined. The fatal cases were due to aneurysm of the aorta, or to malignant disease of the gullet or in the mediastina. The seven recoveries were probably due to inflammatory ankylosis of the crico-arytenoid joint. One of the patients was a young and healthy married woman who had pains in the joints which were diagnosed as gout. These symptoms, as well as the paralysis of the left vocal cord, were cured by the Droitwich treatment.

In view of such cases one must revise the teaching we received in our student days and not regard a motionless vocal cord as necessarily an indication of serious disease.

Sir WILLIAM MILLIGAN said he believed that a considerable number of these cases were functional, though this was not usually recognized when only one cord was affected. He had seen several cases which were thought to be due to organic disease, but having seen them two or three years later, he found that they had cleared up without treatment, and that they almost certainly were functional. The possibility of a unilateral paralysis of a cord being functional should not be overlooked.

Sir STCLAIR THOMSON said he had a suspicion that it was possible for there to be functional paresis of one cord. He had watched cases in regard to which he could not make up his mind as to whether a cord had complete mobility or not. He had had eleven cases of paralysis of vocal cord in tuberculous subjects with nothing wrong in their larynx. Five out of those eleven absolutely recovered the movement, and of the remaining cases one or two were lost sight of, and two or three had become permanent after the lapse of several years. There was the possibility of pleuritic pressure, which might subside and so permit the cord to move again. Half of his cases were right-sided.

Mr. E. D. D. DAVIS said he had seen two cases of undoubted apical pulmonary tuberculosis with right recurrent laryngeal paralysis, which recovered. He once did a dissection at a post-mortem examination on a patient who had right recurrent laryngeal paralysis, and the cause was a stretching of the vagus nerve, due to pressure of the enlarged mediastinal glands.

Sir JAMES DUNDAS-GRANT said he had shown a case in which paralysis of the left vocal cord was due to dilatation of the aorta, and the patient completely recovered. He had described two cases of paralysis of the left cord which he considered due to alcoholism, and which disappeared when the patients amended their habits.

Mr. VLASTO (in reply) said that when he and Mr. Banks-Davis first saw the case they thought that it was malignant. As time went on and the patient instead of getting worse, anything improved, they thought that the diagnosis should be revised. With Mr. Tilley's paper on fixation of the vocal cord in mind he (Mr. Vlasto) had thought that the Section

would take the view that there might be fixation of the vocal cord as a result of arthritic changes in the crico-arytenoid joint. The suggestion that fixation of the cord might be functional could not possibly apply to the present case. In support of Sir James's remarks he stated that since his own bibulous patient had forsworn liquor, his laryngeal condition had tended to show improvement.

Tuberculosis along with Syphilis of the Larynx.

By E. D. D. Davis, F.R.C.S.

A woman, aged 26, was sent to hospital for tracheotomy to relieve dyspnœa and stridor. She had been hoarse for eight weeks.

All the structures of the larynx were œdematous, including the epiglottis. The vocal cords were irregular and covered with granulation tissue. The glottis was very much stenosed. Wassermann reaction positive. Definite evidence of pulmonary tuberculosis present. Salvarsan injections were given with marked improvement, the dyspnœa and stridor have disappeared, but the larynx still shows marked signs of disease which resembles lupus.[1]

1 Comp., "Extensive Tuberculosis of Larynx," shown by Sir StClair Thomson, February 6, 1925. See *Proceedings*, 1925, xviii (Sect. Laryng.), p. 15.

Section of Laryngology.

President—Dr. A. Logan Turner.

DISCUSSION ON PAROXYSMAL RHINORRHŒA.

Dr. J. Freeman.

My business this afternoon is to give a general or rather a pathological survey of paroxysmal rhinorrhœa; and neither to deal with its history, nor, since I am not a laryngologist, to give the laryngological view of it.

However, I thought it best to begin by finding out what the laryngologists themselves have said about paroxysmal rhinorrhœa, and on looking up the records of discussions I was delighted to find that not only was hay fever taken as the typical example of this illness, but that speakers seemed to spend two-thirds of their time discussing it. Which suited me very well.

Let us then start with a rough definition of paroxysmal rhinorrhœa as *a symptom-complex where the pathological mechanism runs parallel with that of hay fever.*

This afternoon I hope to give you that pathological mechanism, so let us examine at once the essentials of our type—hay fever.

(1) The first point to be emphasized is that whereas the pollen of the grasses is quite innocuous to the normal person, it invariably causes distress whenever it comes into contact with the living tissues of hay-fever subjects. This is perhaps the most striking of all immunological phenomena. I have frequently given myself, with impunity, at least twenty times what to some hay-fever subjects would be a fatal dose of pollen. To a rabbit I have given 20,000 times this human "lethal dose." In fact the stuff is completely non-toxic to the normal animal. But in the hay-fever subject whenever grass pollen arrives on his mucous membrane in the form of atmospheric dust, then paroxysmal rhinorrhœa is produced. Therefore we can develop our original definition and say that paroxysmal rhinorrhœa is *the symptom-complex produced whenever a foreign protein comes into contact with the nasal mucous membrane of a person who is sensitive to that protein.*

(2) Such foreign proteins are legion. They include any animal or vegetable substance in the atmospheric dust: dermal scales from animals, all the pollens of plants, all the spores of all the fungi and other lowly organized vegetables; also vegetable substances such as wheaten flour, sawdust, &c. Many of these are almost as easily detected as the grass pollen: others, such as the spores of fungi, are rarely, if ever, identified as the causal agent.

(3) Now the inspired air is not the only route by which foreign proteins arrive at the nasal mucous membrane and so produce paroxysmal rhinorrhœa. The irritant may arrive through the blood, e.g., from a bacterial toxin, or from some food substance absorbed from the alimentary track.

That the foreign proteins can arrive both by the air and by the blood has been a commonly accepted fact for years, but to give you a laboratory demonstration on this point I have, out of two perfectly normal men, constructed two artificial paroxysmal rhinorrhœas, one where the protein arrives out of the inspired air, the other by way of the blood.

(*a*) In the first I sensitized (with the aid of Mr. Waggett) the mucous membrane of my colleague Dr. Hare with the serum of a hay-fever subject; the result was that when he sniffed up a little grass pollen to-day he at once began to sneeze, and if he is in the room at this moment you will find his pockets stuffed with some half-dozen thoroughly saturated pocket handkerchiefs. Thus an artificial rhinorrhœa was successfully produced by a pollen irritant from the air.

(*b*) In my second experiment the middle turbinate of my own nose was sensitized with the serum of a person who becomes ill whenever she eats eggs, with the result

that after drinking an egg-flip this afternoon my nose has been itching and uncomfortable, though I rather spoiled the experiment by having too much cocaine in the nose, the effect of which had not completely passed off before I drank the egg-flip. In this second case the irritant got from the mucous membrane of the stomach up to the mucous membrane of the nose by the blood-route.

It is therefore clear that paroxysmal rhinorrhœa will occur whenever a foreign protein irritant arrives, whether by way of the blood-stream or by way of the inspired air, at the mucous membrane of the nose of a person who is sensitive to that particular protein.

Now to revert to our type.

In laryngological practice hay fever is more or less a laryngological disease, but pathologically it most certainly is nothing of the sort.

(1) Even in the naturally occurring disease the eye of the sufferer, according to my statistics, gives more trouble than the nose; a few patients complain more of the skin of the face than of nose or eye symptoms together, and others are troubled by the general upset and complain of neurasthenia and so on rather than of any local nose symptoms.

(2) Further, when one brings grass pollen artificially into contact with the tissues of a hay-fever subject in a site other than the mucous membrane of the nose, it becomes clear that the rhinorrhœa of hay-fever is merely a botanical accident. For example, if pollen is placed in any part of the cutis vera of a hay-fever patient, a very marked urticarial wheal surrounded by a crimson flush will develop in from fifteen to twenty minutes and the patient will suffer irritation at the site of contact. Dunbar placed pollen in the rectum of a hay-fever subject and produced pruritus ani; I have put pollen into the food of another subject, and produced nausea, giddiness, vomiting and diarrhœa, though it is easy to show that the normal person can eat quantities of pollen without any ill-effect whatever.

Again, if the pollen is injected into the vein of a hay-fever subject, and is given, so to speak, the entire body to choose from in the matter of symptom production, giant urticaria, migraine-headache, angio-neurotic œdema, perhaps asthma, will appear, and possibly some rhinorrhœa: but it is quite obvious that this last is neither the earliest nor the most striking symptom to be produced.

(3) A still more convincing experiment can be performed to show that the trouble is a constitutional one, that it exists in fact in the very blood of the patients. If a small amount of serum from a hay-fever case is injected into the skin of a normal man, the area of skin injected will become grass-pollen sensitive, and will remain sensitive for one or two weeks. This may be demonstrated by bringing pollen into contact with the small sensitized area, or else by injecting pollen into the blood-stream of the experimental person, when it will produce no symptom whatever on all the rest of the body, but at the site of the injection a swelling comparable with angio-neurotic œdema will be produced.

(4) Parallel experiments can be performed with any of the other proteins capable of producing paroxysmal rhinorrhœa, so that I think I have shown you in detail why I claim that it is not a laryngological disease, but merely a local manifestation of a general sensitiveness.

I will now give you a list of other manifestations of protein idiopathy. The list is admittedly incomplete but will be of use in *two* directions.

	Paroxysmal rhinitis ("nasal asthma").	F.	Food sickness (vomiting, diarrhœa).
HF.	Hay fever.	E.	Epilepsy.
H.	Horse asthma (cat asthma, &c.).	M.	Migraine.
A.	Asthma.		Paroxysmal arthritis.
U.	Urticaria.		Paroxysmal colitis.
	Eczema.		Paroxysmal nephritis.
	Angio-neurotic œdema.	I.	Insect fever.

(1) The presence of any of these other protein idiopathies will make the occurrence of paroxysmal rhinorrhœa the more likely, for they are sister-symptoms.

(2) Also any generalizations that may be arrived at regarding, let us say, angio-neurotic œdema or asthma, will be applicable to our subject this afternoon.

I have no time to-day to offer definite proof that these other protein idiopathies *are* sister-symptoms, but the fact is nowadays fairly generally recognized.[1] Before leaving the list, however, I should like to say that while some of the items (e.g., paroxysmal colitis and nephritis) are new-comers, there is no reason to believe that the list is complete.

To turn to the pathological mechanism of these protein idiopathies, and of paroxysmal rhinorrhœa in particular, it would be bad philosophy to speak of the protein irritant as *the* cause. One must rather think of a causal chain in which the protein irritant is merely one of the links.

I give here a very incomplete list of such causal links: (i) Diathesis (heredity); (ii) protein irritant; (iii) nervous factors; (iv) trauma (regional factor).

(i) Of the first of these four, diathesis, we know little or nothing. The marked heredity incidence is very noticeable, but a case of hay fever has been known to show itself in a patient for the first time at the age of 74, he having numerous hay-fever blood relations and having been absolutely immune up to that point. I give

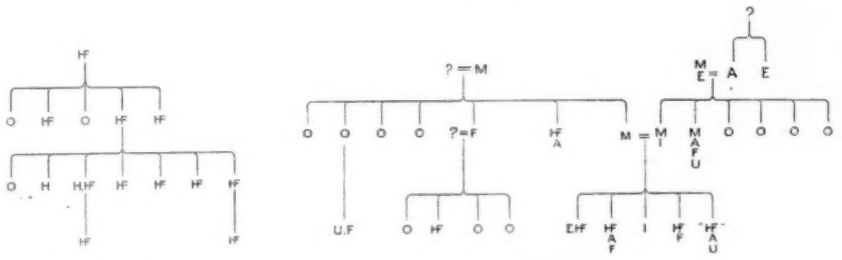


CHART I. CHART II.

The above two charts show the heredity incidence of the protein idiopathies. The first is almost entirely concerned with hay fever; the second one shows six cases of hay fever and a large mixture of other protein idiopathies. The meanings of the various letters and symbols are given in the left hand margin of the list of protein idiopathies given above. The symbol "O" is put in to signify that the person was as far as could be ascertained normal in relation to foreign proteins, i.e., possessed no protein idiopathies.

here a couple of heredity charts chosen at random, one of them to illustrate the enormous incidence of hay fever in one family; the other to illustrate how a family may inherit not one particular protein idiopathy only, but an apparently indiscriminate mixture of them. (The symbols in these charts denoting the various protein idiopathies are explained in the left-hand margin of the list of the protein idiopathies given above, see p. 30.)

(ii) The protein factor: about this I have already said enough; it is the complement of the diathesis factor.

(iii) The third, or nervous, factor becomes very obvious when one collates observations on a large number of these protein idiopathies, paroxysmal rhinorrhœa included. The only conclusion concerning hay fever given in the edition of Osler's "Medicine," which I used for my reading, is the statement that a woman got an attack of hay fever by smelling an artificial rose, and that, therefore, "the whole disease is a neurosis." When I started working at hay fever fifteen or sixteen years ago nearly every patient that came to me had been told by his or her doctor that it was "all nerves"; and, indeed, anyone of us can demonstrate that nerves have

[1] *Proc. Roy. Soc. Med.*, 1920, xiii, pp. 129-148.

something to do with it by producing an attack of hay fever in the sensitive subject (though this can be done in the pollen season only), by mere suggestion or word-painting of the particular object or incident to which he attributes his symptoms. Over and over again one hears of hay fever, or any of the other protein idiopathies, being produced by some emotional disturbance—bad news, excitement, danger, pity, eroticism, emotional music, and so on; and apparently the attacks may be terminated in the same way. Yet, while it is perfectly true that the more "jumpy" and "nervy" the subjects are, the more likely they are to have attacks of some protein idiopathy, anyone who has examined large numbers of these cases will, I think, agree that taking them all in all they are not more mentally unstable than, let us say, any chance collection of medical men. And if that is disputed I can say this: I have shown that paroxysmal rhinorrhœa can be artificially produced in a perfectly normal man by the transference of serum from a hay-fever patient. Supposing that hay fever is "all nerves," is it then suggested that the nervous mechanism of the patient must be transferred with the serum to the normal man, before the reaction at the sensitized area can be brought about? I think not. Even here nerves do come into play, but they are the nerves of a normal man.

(iv) Trauma is my fourth link in the chain; there must be something to account for symptoms arising in one particular place. In hay fever the atmospheric method of arrival is no doubt the chief localizing cause; but in angio-neurotic œdema, for example, it is very noticeable that slight injuries to the skin may determine the exact site of the swelling, and, again, in epilepsy injury to the head may predispose the sufferer to an epileptic fit in exchange for, let us say, eczema or asthma, or paroxysmal rhinorrhœa.

Such are the bare outlines of the pathology of paroxysmal rhinorrhœa. With diagnosis and treatment I have here no time to deal, but I should like to make just one point about each. People used to say, and some still say lightheartedly, "identify the foreign protein and deal with that." But this is not so easy. There are not hundreds but thousands of these proteins, and a patient may be subject to many of them, so that you may well identify dozens by experiment without coming across the one which is causing havoc. Again, in the artificial skin-reaction method, many of the largest protein groups (for instance, bacteria, spores from the moulds) give next to no reaction compared with food proteins, or still more the pollens and the dermal scales from animals. When, however, the protein cause can be identified this is of course well worth doing.

As for treatment, I will only say here that every specialist will deal with the link in the causal chain which concerns his own province; I, as immunologist, would wish to deal with the foreign proteins by avoidance or de-sensitization, and that still seems to me by far the most helpful line of treatment. The psychologist or neurologist would of course want to work on the nerve factor, not, I think, so hopefully, and the laryngologist in dealing with paroxysmal rhinorrhœa would quite properly wish to treat whatever abnormal local conditions he might find.

Dr. A. Brown Kelly.

I assume that the title of our discussion—paroxysmal rhinorrhœa—was chosen in order to avoid the use of the term vasomotor rhinitis. Objection has been taken in the past to the word rhinitis in this connexion, as it was supposed to imply inflammation, but according to a note recently published in the *British Medical Journal* the suffix *itis*, strictly speaking, signifies disease and not inflammation, so that vasomotor rhinitis means vasomotor disease of the nose. It is therefore correct to apply that designation to the group of nasal affections characterized by pronounced vasomotor manifestations and including nasal toxic idiopathies or nasal allergy, vasomotor nasal neuroses and certain disorders of metabolism. In this paper I shall

deal with the symptoms and signs of vasomotor rhinitis and with some aspects of the vasomotor nasal neuroses.

SYMPTOMS.

Vasomotor disturbance in the nose manifests itself by paroxysmal sneezing, profuse hydrorrhœa and excessive swelling of the inferior turbinates. A typical vasomotor attack—which, for convenience, may be termed a nasal crisis—sets in suddenly with frequent and violent sneezing. This is accompanied by abundant watery discharge which may saturate a dozen or more large handkerchiefs in an hour. Usually at a later stage the nose becomes completely blocked by turgescence of the erectile tissue. The symptoms persist for a period varying from a few minutes to several hours, and pass off as suddenly as they came. When violent or long-continued they may leave the patient prostrate.

A nasal crisis such as outlined differs from acute rhinitis or true cold in the head in that it is not due to infection and is not infectious; it is not ushered in with irritation in the naso-pharynx, shivering, malaise and rise of temperature; it rarely lasts longer than a day; and it is not followed by a discharge of muco-pus.

Sometimes the two conditions—acute coryza and a nasal crisis—are associated and produce what is popularly known as a "streaming cold." The attacks are at long intervals—perhaps twice yearly; the immediate cause may be exposure; the symptoms are severe and render the patient unfit to do anything but go to bed and attend to his nose; the attack usually lasts two days and is followed by a muco-purulent stage.

We rarely have an opportunity of seeing our patients during a nasal crisis and are thus dependent upon them for a description of their ailment. Generally this is summed up in the statement that they suffer from "catarrh" or "colds," and it is only by questioning them closely that we elucidate what is really wrong. When the three leading signs—sneezing, rhinorrhœa and obstruction—are pronounced and co-exist, there is no difficulty in recognizing this affection, but when they occur singly or in association with other nasal diseases which mask them, the vasomotor element is readily overlooked. It is, therefore, desirable to be acquainted with the special characters which each of these signs acquires when due to vasomotor irritability.

All three are marked by suddenness of onset and cessation, and by unusual intensity while present. Consequently the qualification "paroxysmal" is equally applicable to each.

Inquiry should be made as to the frequency and cause of the sneezing. If it is found to come in violent paroxysms, which continue for half an hour at a time or recur at short intervals, and is excited by slight causes which are not apparent, as when it sets in while the patient is resting and reading or asleep in bed, then we may assume with a fair degree of certainty that it is a manifestation of a vasomotor neurosis.

As to the rhinorrhœa, it is more difficult to get an accurate and relevant account which enables us to distinguish it from the rhinorrhœa accompanying rhinitis or certain disturbances of nasal respiration. The chief points to be determined, besides the presence of the paroxysmal characters already mentioned, are the duration, amount and characters of the discharge, and the absence of a subsequent muco-purulent stage.

Swelling of the nasal erectile tissue is the third and, it should be noted, occasionally the only sign of a vasomotor disturbance. The swelling takes place suddenly, causes complete blocking, and, after a varying interval, suddenly subsides; and yet, the patient does not feel that he has a cold in the head. When symptoms such as these exist, and especially if within a short period they have developed to a pronounced degree in one who hitherto has had no trouble with his nose, they are probably vasomotor in nature.

Examination of the Nose.

The intranasal appearances in a case of nasal vasomotor neurosis are often normal between the attacks of sneezing, rhinorrhœa and obstruction. During the examination one must keep in view the patient's nervous tension or state of mind which causes contraction of the nasal erectile tissues and cessation of the hypersecretion. The patient may remark upon the temporary improvement and express regret that the examination is being made while the nose is at its best. In such cases, however, it is not uncommon on probing to find hypersensitiveness of the nasal mucosa. This sign should suggest the presence of a neurosis.

On the other hand, if the examination is made in an old-established case while pronounced vasomotor swelling of the inferior turbinates is present, several peculiar features may be observed. The swelling involves the inferior turbinate in its whole depth, whereas in rhinitis the lower part is mainly or alone enlarged; the mucous membrane is pale, œdematous and slightly nodular; the œdematous tissue may be very firm so that it does not yield under the probe as does the congested inferior turbinate, and it does not readily subside under the action of cocaine. If adrenalin is also applied, it may or may not cause contraction, but several hours later some of the patients suffer from violent sneezing and rhinorrhœa, or what they term "a fearful cold." This reaction is occasionally so severe that a membranous exudate forms between the inferior turbinate and septum.

Vasomotor Disturbance secondary to Intranasal Disease.

Various nasal diseases, e.g., turbinal congestion and hypertrophy, polypus, and septal deviation may be associated with vasomotor instability and may mask its manifestations. In some cases the vasomotor symptoms are excited or maintained by the gross nasal disease and pass off when it is corrected. A striking example of this connexion and one which is not generally known is the *Aspergillus*. This fungus occasionally gains a foothold in the nose, especially in the posterior part of the middle meatus, where it gives rise to the formation of a mass of extremely tough greenish mucus which is found to be largely composed of the mycelia of *Aspergillus*. This collection produces mechanically or by its toxins a very severe unilateral vasomotor reaction which persists until the fungus has been removed.

In cases of deviation of the septum it is generally found that when out of doors the free side discharges much more than the obstructed one. This is probably due to two causes: First, the temperature in the wide fossa is lower than in the narrow fossa and in consequence more of the moisture in the expired air is condensed within it; and secondly, the larger volume of inspired air in the wide side excites a corresponding increase of secretion. When nasal hyperæsthesia is superadded to deviation of the septum, hypersecretion sets in on the wide side with very slight alterations in temperature such as are experienced in passing from one room to another, so that individuals thus affected, even while moving about the house, are constantly blowing and sniffing, and the tendency to do so passes off only after the septal deviation has been rectified.

Intranasal Disease secondary to Vasomotor Disturbance.

In another group of cases the vasomotor disturbance is the fundamental affection, and the turbinal enlargements and polypi are secondary. If we ask patients with polypi as to whether the pronounced nasal obstruction due to the growths was preceded or followed by paroxysmal sneezing and rhinorrhœa, it will be found that in the majority of cases the sneezing followed the obstruction but that in a considerable proportion the vasomotor symptoms were present for years before marked or constant obstruction was experienced. In some of the patients belonging to the latter class the sneezing ceases after the nose has become filled by polypi, and sets

in again after their removal, showing that the air current is more of an irritant than the pressure of the polypi.

I have patients suffering from intractable vasomotor disturbance in the nose who have attended me at intervals over a period of many years. In several of these I have noted a succession of changes in the nose attributable to chronic vaso-dilatation. These patients complained first of symptoms due to hypersensitiveness and turbinal swelling; later, turbinal hypertrophies developed, and still later, polypi. The recurrence of the polypi was largely influenced by the state of the vasomotor mechanism for the time being. When stable, the nose was quiescent and no fresh growths appeared. Then came a period of instability with frequent nasal crises, and rapid recurrence took place. The radical cure of polypi is thus difficult or impossible as long as a vasomotor disturbance exists.

Nasal polypi often run in families. In some of these one can trace an inherited nasal vasomotor neurosis which is probably the underlying cause of the polypi.

It is therefore of importance to be on the outlook for such associations in order that the vasomotor instability when present may be detected and treated as well as the more obvious nasal disease. If this precaution is neglected before operating various difficulties are likely to be encountered. Thus, in spite of the free use of cocaine the tissues remain swollen and imperfectly anæsthetized; on an attempt being made to snare large posterior ends they collapse and are strangely elusive; polypi shrink surprisingly and the bleeding is unusually profuse. After resection the septum may swell and œdema may persist, so that a permanent, boggy condition of the anterior and upper part results. Such was present in several patients shown at meetings of this Section.[1] In cases with a vasomotor complication, although the enlarged inferior turbinates have been reduced to a normal size, they may still swell so as to cause discomfort, and the sneezing and rhinorrhœa may continue. If the surgeon has not yet recognized the presence of a vasomotor disturbance he is likely to consider further operation or operations necessary until in despair he may even have recourse to turbinectomy—the least scientific of nasal procedures. I have seen patients who had derived no benefit to their symptoms from resection of the septum and removal of the greater part of both inferior and middle turbinates, improve quickly under constitutional treatment.

To a train of circumstances such as outlined, galvano-cauterization owes its undeservedly bad reputation in some quarters. If the important part played by the vasomotor element is not appreciated the nose may be cauterized over and over again in order to make the patient comfortable, but usually with indifferent success. The probability that errors of this kind are not uncommon suggests itself in explanation of the hesitancy shown by some patients to have their noses cauterized. Apparently they have learned of cases in which the treatment had to be continued long or repeated often. This prejudice is unfortunate, for the cautery is a valuable method of dealing with chronic congestion of the inferior turbinate, but when this is complicated by a vasomotor disturbance, a dozen applications instead of one, or at the most two, which should suffice, may fail to establish the requisite patency.

Nasal Hypersensitiveness.

Hypersensitiveness of the nasal mucous membrane is the local condition which in most cases predisposes to the sneezing, rhinorrhœa and turbinal swelling in vasomotor rhinitis. These manifestations are due to vaso-dilatation, and in this connexion it is interesting to remember that the sensory nerves may convey not only afferent impulses to the central nervous system but also vasodilator impulses in the opposite direction—the antidromic impulses of Bayliss. In a recent paper on the "Trigeminal System," Ivy Mackenzie[2] states that: "There is not the slightest doubt that a large proportion of the so-called vasomotor reactions, so well known to

[1] *Proc. Roy. Soc. Med.*, 1923, xvi (Sect. Laryng.), p. 27.

[2] *Trans. Ophthal. Soc.*, 1924, xliv, p. 149.

rhinologists, are in effect the result of central antidromic stimulation of 'sensory' nerves to the mucous membranes of the parts involved."

Abundant evidence is available of the presence of hyperæsthesia from the ease with which mild stimuli, e.g., dust and slight differences in temperature, excite violent symptoms of vasomotor disturbance. Conclusive proof of the presence or absence for the time being of hypersensitiveness is obtainable by testing the mucous membrane.

For this purpose a long thin silver probe is used with which the parts are gently stroked. The regions which should be tested are: the tubercle of the septum, and outer wall of the nose quite anteriorly, corresponding to the distribution of the anterior ethmoidal nerve, the septum opposite the posterior third of the middle turbinate which is supplied by the naso-palatine, and the anterior end of the inferior turbinate which often is particularly sensitive. Upon these parts being gently stroked no complaint is made of discomfort when sensation is normal. If heightened, signs of irritation are produced of which it is convenient to distinguish a slight, medium and pronounced degree.

In recent years I have tested, in the manner mentioned, the nasal sensitiveness in many of my patients with vasomotor disturbance. From the results obtained the following conclusions are drawn: (1) Nasal hyperæsthesia was present on one occasion or more in a large proportion of the cases tested. (2) Very pronounced nasal hyperæsthesia may exist without vasomotor symptoms: such patients are usually hysterical or of a highly nervous temperament. (3) The hyperæsthesia may vary from day to day, and at different times of the day. This is in accordance with the patient's feelings; he is aware of flitting variations, and notices particularly that the hypersensitiveness and his symptoms are worse when he is fatigued or worried. (4) The sensitiveness on the two sides of the nose often differs. The patient may state that the sneezing is due to irritation on a certain side. In deviation of the septum the wide fossa is usually the more sensitive. (5) The sensitiveness at different parts of the same nasal fossa often differs; posteriorly it may be normal, anteriorly it may be heightened, and vice versa. The spheno-palatine ganglion, therefore, is not a decisive factor. (6) The skin of the nose, which is supplied by the external branch of the anterior ethmoidal nerve, may also be hyperæsthetic. (7) As the sneezing and rhinorrhœa gradually abate under treatment the hyperæsthesia, as a rule, also diminishes; in some persons, however, it persists after the symptoms have passed off.

With the object of reducing or abolishing nasal hyperæsthesia I have made frequent use of alcohol injections into the regions traversed by the two principal nerve-trunks. A few drops were injected into the septum opposite the posterior third of the middle turbinate and another small amount into the septal tubercle. At a subsequent visit the other side of the septum was treated similarly. Some of my early cases were strikingly benefited, so that I was encouraged to continue the method. I still employ it when hyperæsthesia is a pronounced feature and I do not doubt that in some cases the injection acts as a nerve block. In others, however, both success and failure are more likely attributable to the mental effect produced in the patient. This procedure in my hands has lost some of its previous efficacy probably in consequence of the qualified opinion I have come to hold of its value. My experience supports Dr. T. A. Ross's[1] statement that "this phenomenon of early cures in the history of a treatment with gradually diminishing results is common to many kinds of therapy."

Causes of Nasal Hyperæsthesia.

The detection of the cause of the hyperæsthesia in each case is of first importance, as it indicates the line of treatment to be adopted.

Heredity is an important predisposing cause of both general and local hyper-

[1] "The Common Neuroses," London, 1923.

sensitivity. Patients with nasal vasomotor neuroses are often found to be highly strung and descended from a nervous stock, so that their general hypersensitivity renders them easily affected by strains and stresses where the individual with a normal nervous organization escapes. Evidence of local inheritance is also not infrequently forthcoming. Thus, several of my patients have given a history of nasal hyperæsthesia in three generations; others have stated that some of their relatives suffered from asthma or nasal polypi.

Nerve exhaustion of emotional origin is, in my experience, one of the commonest causes of nasal hyperæsthesia. It is always important to inquire as to whether the onset of the vasomotor symptoms was preceded by a period of mental or physical tension. From the history obtained in many cases it would appear that ideal conditions for the development of a nasal vasomotor neurosis are prolonged anxiety or apprehension and a lowered state of the general health. Typical examples are the following: The mother who tends day and night her delicate only child; the daughter who nurses a dying parent; the business man with financial worries; the schoolgirl overworking for an examination; and the young woman with her love affairs and approaching marriage. In some of the worst cases no cause is admitted, and only long afterwards proceedings in the divorce, bankruptcy, or criminal court, or death, bring to light the skeleton in the cupboard.

If we approach the question from the other side and inquire as to the manner in which nerve exhaustion produced by violent or prolonged emotion manifests itself, the psychologist replies—by hypersensitiveness chiefly. Maurice Craig[1] has emphasized this relation and has stated that the hypersensitivity is observed on the physical or the psychical side or on both. It is precisely this condition that affects the nose. In addition, the patient often confesses to general irritability, apprehension and disinclination and unfitness for mental exertion.

Toxæmias may lead to vasomotor disturbances by causing nerve exhaustion and its accompanying hypersensitiveness. Those of intestinal origin are probably commonest and of most importance, but it is our duty as rhinologists to search for focal sepsis in connexion with the accessory sinuses and tonsils. I have seen on several occasions the nasal crises cease after a suppurating antrum had been drained. In such cases the disturbance may be due sometimes to protein sensitization.

Sexual excesses and irregularities cannot fail to be frequent causes of the nerve exhaustion in the affection under consideration. Reliable information as to this association is best obtained through the family physician.

I have dealt with only one of the chief causes of vasomotor rhinitis, but to form a correct estimate of a case the other possible causes should be investigated.

Treatment.

In nasal vasomotor neuroses constitutional treatment is of first importance. Even if intranasal structural defects have to be remedied a short preliminary course of a nervine tonic is useful, especially if hyperæsthesia is pronounced. Quinine, arsenic, strychnine, phosphorus and valerian exercise a strikingly beneficial influence in many cases of paroxysmal sneezing, rhinorrhœa and obstruction.

Lastly, we should not be discouraged if at first our treatment is unsuccessful. We know little of what happens in the patient's home and still less of what is passing through the patient's mind. As long as the conditions of life are such as to cause considerable mental disquietude, vasomotor symptoms are likely to persist. We should also remember that this is a functional disease, which, as a rule, passes off sooner or later, but is liable to recur.

Dr. James Adam.

Paroxysmal rhinorrhœa connotes two things—catarrh and spasm. These are common, together or successively, to the whole respiratory tract, and, as the causes

[1] "Nerve Exhaustion," London, 1922.

are similar if not the same throughout, it would be short-sighted to limit consideration to the nose. In general, they are due to toxæmia largely produced by dietetic and hygienic errors.

Laryngitis stridula and laryngismus—distinguished from each other without any ætiological reason—describe these conditions as regards the larynx: laryngismus is asthma of the larynx. The recurrent bronchitis of children ends, unless prevented, in asthma; these are the catarrhal and asthmatic equivalents in the bronchi and bronchioles. Acute pulmonary œdema, the paroxysmal bronchial flux of Gee, is a fulminant combination of catarrh and spasm similarly produced.

My remarks are based mainly on analysis of 850 cases of asthma. These show that 42 per cent. begin in the first decade, usually as what is called recurrent bronchitis, afebrile, and rightly recognized by Gee as asthma, which it typically becomes after a few years. They are often preceded by attacks of laryngismus. Manifest dietetic and hygienic errors were found in 92 per cent. of these cases of the first decade and at least in 77 per cent. of cases beginning before the age of 50. Of the 42 per cent. arising in the first decade over 21 per cent. gave a history of eczema, which is catarrh of the skin produced by the same toxæmia, and curable in the same way as asthma, and in the same way as, at least, some forms of rhinorrhœa. Cases of rhinorrhœa, laryngismus, urticaria, epilepsy, goitre and Raynaud's symptoms all turn up as more or less frequent complications of asthma—all toxæmic.

The cause of asthma, as I stated twenty-five years ago, is a toxæmia resulting from carbohydrate excess, especially in respect of what are called milk foods; interfering with proper metabolism of the more complex protein molecule. The eosinophilia, typical of asthma, laryngismus, &c., is the chemotactic reaction of the polymorphs to the acidosis so produced.

In support of this toxæmic view I can give here only summary statements.

(1) *Therapeutic Results.*—About 75 per cent. are cured by attention to diet and hygiene—mainly outdoor exercise,—a weekly mercurial and other simple drug treatment, always provided that sources of sepsis—nasal, oral, colonic, &c.—are attended to, and that sources of nasal irritation are removed. If treated in the first decade nearly 100 per cent. should be thus cured. The same treatment succeeds with laryngismus, eczema, and, in its early stages, epilepsy.

(2) *Periodicity.*—Most cases of asthma are worse at night, some occur only at night. Only in a minority of cases is this due to supersensitiveness to feathers—call it anaphylaxis if you like. Week-end periodicity, that is asthma on Sunday or Monday, brought on by the big feed and the big rest of the week-end, occurred in 24 per cent. of the total—men, women and children. Week-end periodicity occurs also in many cases of epilepsy and other affections.

(3) A score of men subject to asthma for the best part of a lifetime, got rid of their asthma under the strenuous conditions and not too generous diet of army life, and suffered from it again after their return to the softer conditions of civilized life.

(4) Through Dr. Grenfell I learn that the Eskimos of Labrador do not suffer from asthma; they have no butcher's cart or grocer's van, they have to hunt and fish for what they eat.

(5) Broken-windedness, veterinarians say, can be produced in any colt by bad feeding; wheeziness in pet dogs and canaries, and in animals in the Zoological Gardens, arises in the same way. The chapters on broken-windedness in horses and asthma in dogs, in Law's "Veterinary Medicine," might have been written from a study of human asthma. I have recorded cases of broken-windedness in horses, and asthma in canaries and dogs, cured or improved by attention to the feeds.

(6) Analyses made during the last three years show that in asthma the proportion of urea to non-protein N in the case of the blood, to total N in the case of the urine, is much less than normal; in the latter case nearly 20 per cent. less on

an average. This 20 per cent. N which should have changed into urea is out for mischief, whether it be as histamine, guanidine, or albumose, or some other of the two trillion odd billion million compounds which Abderhalden has calculated the twenty amino-acids constituent of protein may form. At any rate the analysis showed that during asthma, ammonia was in much excess in the urine, this clearly pointing to acidosis.

(7) Epilepsy sometimes complicates asthma. Cunéo, in his recent researches on epilepsy, makes the same point as I have made for asthma, namely, that digestion of the starches interferes with protein metabolism, thus producing albumosæmia, a toxic condition of the blood which causes epilepsy. Twelve years ago I had records of cures of about a dozen cases of epilepsy—most, but not all, in their early stages—on the same lines as I have mentioned for asthma, and without bromides.

Anaphylaxis may occur in asthma, idiosyncrasies may be found in 40 per cent., but they are not all anaphylaxis, as they are loosely called. Anaphylaxis is not the most important thing in most cases, and sometimes not even in those in which it is thought to be all-important. Take what Professor Levi calls the classical case of Widal:—

A sheep dealer, aged 47, had been in daily contact with sheep from the age of 12; then began to have typical attacks of asthma with increasing frequency whenever he went amongst sheep, each attack preceded by the symptoms characteristic of anaphylactic shock. This case has, in Levi's words, "all the precision of a laboratory experiment." But the precision did not arise till after the 90,000 exposures to sheep when he was between the ages of 12 and 47, and not a word is said about the man's nose, teeth, food, habits—all vitally important and all carefully disregarded.

Someone a few years ago mentioned the case of a groom with horse asthma, who had no asthma during the war, although his job in the army was connected with horses. The hygiene of the war, not protein shock, cured his anaphylaxis by curing his toxæmia.

To bring all this into relation with rhinorrhœa, I will quote brief records of just two cases.

A man, aged 29, sensitive to horses, suffered from rhinorrhœa and sometimes asthma, set up by exposure to any dust—to that of a railway smoking-carriage, to room-cleaning, fog, hay-cutting, long-haired dogs, horses, roses, or sweet-peas. He got rid of his trouble after resection of his septum, and after care being taken about his diet and ways of life. He has remained well for years.

Last year his mother came complaining of rhinorrhœa and cough. Like a specialist, I concentrated on her nose: it needed operation. But the cough and breathlessness persisted. Then I found that her blood-pressure was 180, and I obtained the history that her doctor had X-rayed her and said she had abdominal aneurysm. She lived under a cloud and avoided exertion. I put her on treatment similar to that employed in her son's case. I reduced the blood-pressure permanently, the cough went, the abdominal aneurysm story became a myth, and she is now a happy, vigorous woman, with blood-pressure permanently down to 120.

I have cured at least half a dozen cases of paroxysmal rhinorrhœa, some complicated with asthma or hay fever, by simply stopping milk. In other cases this failed and sometimes the treatment mentioned for asthma succeeded. But the typical case of rhinorrhœa shades, I think, into an atypical case with bloody turbinals, excessive discharge of mucus, and often with eczema. Local treatment is not to be despised, but the radical treatment is again diet and hygiene, especially vigorous open-air exercise. In at least 60 per cent. of my total asthmatics there was a nasal condition asking for rectification.

Haseltine, of Chicago, has come to the same conclusion as I have; and in a communication sent me two weeks ago he writes:—

"Asthma is an abnormal bodily state, manifested by various symptoms such as urticarias, eczemas, eosinophilia, cyclic vomiting, protein or non-protein sensitization, disturbed calcium or endocrine-metabolism, hyperesthetic rhinitis, angioneurotic edema, acidemia, mucous-membrane hyperesthesia (hay-fever), to which may or may not be added bronchospasm.

"This last symptom, when it does occur, so dominates the clinical picture that both the lay and the medical mind are somewhat hypnotized. It is now for the first time recognized as only the climax to a rather definite sequence of abnormal manifestations. It occurs only when general toxicity reaches a certain stage and when ethmoid irritation becomes sufficient to incite the spasm.

"The asthmatic patient is in a constant state of delicate balance between absorption and elimination. Anything that helps to throw the balance the right way will relieve him; anything throwing it the wrong way will increase his distress. The asthmatic patient has an abnormal ethmoid. Anything that lessens ethmoid pathology will relieve him; anything that increases ethmoid pathology will increase his distress."

La Forge holds the same view.

Percepied said much the same thing in 1912:—

"A periodic spasmodic coryza, hay catarrh, asthma, are only various expressions of the widespread affections causing vasomotor spasm, and secretory disturbance of the aërial tree. The name changes according to the anatomical area involved or according to the exciting cause; but it is an error to consider such manifestations as a separate entity."

On hay fever I need say only two things: First, if anaphylaxis is more applicable to any one human disorder than to another it is to hay fever. Chloral will sometimes do for hay fever what it does for anaphylaxis—stop it; and is a peculiarly suitable drug for hay fever, which lasts only six weeks a year. Secondly, hay fever is not quite the same thing as asthma, for only 7 per cent. of asthmatics suffer from it and only 7 per cent. of hay-fever patients have asthma.

The main point I wish to make is that anaphylaxis, so far as it occurs in man, is often a graft on a preceding toxæmic condition, the result of an unwholesome régime, and that it is simpler, more radical and more efficient to deal with this and get the patient into a condition under which he can stand up to, be insensitive to, any ordinary protein, than merely to hunt for special proteins and act solely against them. The aim should be to de-toxicate the patient by attending to all the emunctories and make him observe a healthy life. In this way most rhinorrhœas will disappear with or without operation, according as the disease is treated late or early.

Dr. Millais Culpin

said the psychologist rarely saw these cases primarily for the conditions which had been described in this discussion, but he was accustomed to seeing the conditions in persons who came for other reasons. He, therefore, gave consideration to such conditions as rhinorrhœa, Raynaud's fingers, or colitis, as forming part of one big picture. He was disposed to criticize Dr. Freeman's contention that a larger number of these sensitized persons were not nervous, but were stolid, steady-going folk. The psychologist could not accept that generalization, because many people with nervous symptoms succeeded in hiding them, and only by making a skilled examination could one be certain whether the patient was or was not nervous.

He recalled the case of a lady who suffered from asthma if she was in the presence of sheep, or attempted to ride a bicycle, or if her husband, a medical man, was away at a confinement. A full examination of that patient linked up those three peculiarities under one psychological cause.

To the psychologist this particular group of disorders was very interesting. If the psychologist could explain why a serum injected produced certain reactions in one person and not in others, he would also be able to explain many other happenings. His own view was that these conditions were parallel to phobias; what would produce in one person asthma or hay fever would cause a phobia in the other. One patient was laid up three weeks after contact with a cat, which produced an absolute panic. He thanked Dr. Freeman for his references to the psychological side of the subject.

Sir James Dundas-Grant

referred members to an article he wrote in *The Practitioner* in 1913 on nasal disease in relation to asthma. His numbers of cures were much the same as those shown in Dr. Tod's tables. In a typical set, 18·2 per cent. were cured, 45 per cent. improved, no improvement resulting in 20 per cent. In his work on this subject he had been much influenced by the experiments of Dixon and Brodie. They found that the dilator nerves for the bronchial muscles were derived from the sympathetic, though they passed through the trunk of the vagus. This accounted for the beneficial effect of adrenalin. Experimenting on decerebrate rabbits, they found that when they could not get a reflex contraction of the bronchial muscle from stimulation of any other sensory nerve, they got it from the nasal mucous membrane, and particularly from the upper and back part of the septum. In patients with asthma one could often find a small swelling of the middle turbinate body which came into contact with the upper and posterior part of the septum, especially if there were variations of pressure. A nose might be full of polypi without this causing asthma, but a small mobile polypus might cause it. In regard to vasomotor rhinitis, he would add to what previous speakers had said, that there was sometimes a tendency to urticaria and chilblains, and to bruising readily on slight provocation, conditions indicating a deficiency of calcium in the blood. Hence the importance in these cases of giving calcium and sometimes magnesia.

Mr. Frank Coke

said that he agreed with Dr. Freeman that those patients suffering from paroxysmal sneezing, hay fever and asthma were in no way more neurotic than normal people.

With regard to judging the effects of nasal operations, naturally one did not see the successes of the nasal surgeons; only patients in cases of failure came for further advice. He had found that cauterization of the septum and turbinals was a procedure of considerable value. Very successful results often followed the removal of septic tonsils, in curing asthma; especially was this so in children.

It was a question whether too much attention was not being paid to the nose itself and too little to the system as a whole. Every case of hay fever gave the dermal reaction on every portion of the skin, showing that the whole body was affected, and contra-indicating the presence of a neurosis in these cases. He had recently seen a doctor who overdosed himself with pollaccine, given hypodermically; this was at once followed by an attack of hay fever, asthma, and a profuse urticarial rash.

Lumière had recently shown that anaphylaxis was due to a colloidal change taking place in the blood, in the nature of precipitation and flocculation of the particles. If that were so the condition of the blood was of more importance than the local nasal troubles. McDonagh suggested that the condensation of these colloidal particles led to this flocculation. If an ordinary case of rhinitis were treated, on his suggestion, by manganese butyrate a cure would probably result.

In all these nasal conditions a larger view should be taken, and every effort should be made to trace the cause of the complaint by means of the dermal reactions, and the full examination accorded to cases of asthma with such successful results.

Mr. C. A. Parker

said that one cause of paroxysmal sneezing and rhinorrhœa which was not commonly recognized was mahogany dust. At High Wycombe there were many cabinet-makers, and many of them when they were sand-papering the mahogany suffered from profuse rhinorrhœa. The onset in that case suggested a purely mechanical cause. He understood the same happened to workers in some other kinds of wood.

Mr. W. Stuart-Low

said he did not consider that local treatment at this discussion had been sufficiently enlarged upon. In the acute stage, namely, the stage at which patients most frequently appealed to the specialist, when there was often such a profuse watery discharge that as many as a dozen handkerchiefs in the course of half an hour became saturated with this serous fluid—what he had found most effective was a solution of cocaine of a strength of 10 per cent. applied quite freely in the nose. The interior of the nose should be swabbed with this solution, which he found more advantageous if used as warm as could be comfortably borne. This was the best means of getting rid of the excessive vascularity and hyperæsthesia of the mucous lining. Having accomplished this the best applications to act as a protective against cold air and dust, which were very aggravating to so sensitive a surface, he found to be those of an oily nature, such as plain paroleine, applied by means of an anterior nasal atomizer.

In most cases of rhinorrhœa there was an anatomical abnormality in the interior of the nose causing a disturbance in its functions in the way of nasal obstruction and bringing about local congestion; and in some cases he had found high up in the ethmoid region an œdematous spot which, if punctured, bled freely: this indicated a collection of purulent material in the underlying cells, from which pus discharged and caused such irritation as to precipitate an attack of rhinorrhœa.

The only remedy in such cases consisted in the ablation—more or less—of the middle turbinal. Sometimes the interior of the nose was so sensitive that perfectly pure non-bacterial dust would induce a severe attack, as in the case of a lady patient of his who was always affected by the dust from the Penmaenmawr slate-quarries.

Where a deviation of the septum existed it was essential that a complete resection should be carried out, and, if the ethmoid septum were out of place, it was imperative that it should be thoroughly removed.

This was well illustrated by a case he had seen and operated upon, in which the lower half of the septum had been resected at a former operation by an expert operator, but the upper part left. The rhinorrhœa was not remedied until a complete resection had been done, because this mechanical displacement kept up a chronic congestion in the upper region of the nose.

Very frequently the inferior turbinal is the offending body, it being firmly enlarged and pressing on the septum, causing obstruction and congestion—such a turbinal is not susceptible to the galvano-cautery puncture. The best procedure for such a condition was the performance of the operation introduced by himself (Mr. Stuart-Low), namely, submucous turbinectomy, which consisted in ablating, more or less, the bony framework of the turbinal, preserving the soft structures, and so effectively removing the mechanical obstruction in the inferior meatus.

Dr. Logan Turner (President)

said he felt somewhat disappointed that none of the speakers had discussed the endocrine aspect of the subject, as work had been done in that connexion.

He agreed with Dr. Freeman that we were not dealing with a nasal disease, and that it was necessary to look further than the nasal cavities for the cause in many of the cases. Dr. Brown Kelly had rightly emphasized that it was not necessary that there should be sneezing, discharge and obstruction in every case; only one or two of the three symptoms might be present.

Dr. Brown Kelly (in reply)

said complaint had been made that the causes and treatment had not been referred to; but it was mainly with the causes that all the openers had dealt, and when the cause in a particular case was determined, the line of treatment became evident.

If he had had time he would have referred to what might be termed false

sensitizations. In these cases sneezing was excited by flour, fine dust from hay, flowers, &c. No specific protein sensitization existed, however, only temporary hyperæsthesia, which made the patient susceptible to various mechanical irritants.

It had been suggested that cases of vasomotor rhinitis could best be investigated by team work. He (the speaker) had had several of his patients very thoroughly overhauled by physicians, radiologists and other specialists without their being able to elucidate the presence of any abnormal condition beyond a neurosis.

With regard to the endocrines it was perhaps as well that this subject had not been introduced into the discussion as there was still so much speculation attached to it.

Dr. JAMES ADAM (in reply)

said he thought members would find the questions which had not been answered in this discussion were very fully answered in the articles which had appeared in the *American Journal of Clinical Medicine*, by Dr. Haseltine and La Forge; a symposium on the subject was to appear in the June number of *American Medicine*.

With regard to endocrine disturbance, this was due to the toxæmia, which affected the endocrine glands in the same way as the skin and the vagus. Doses of any dust frequently repeated would start asthma in a toxæmic subject.

He had been very pleased to hear Mr. Coke's reference to the work of Lumière, who had been working at the nature of anaphylaxis for years; it was most valuable work. To asthmatic patients he (the speaker) gave a mercurial once a week for years, as it helped one to deal with the toxæmia. Golf was a most effective medicine for these people: it was preferable to injections.

Section of Laryngology.

President—Dr. A. Logan Turner.

Case of Epithelioma of Right Vocal Cord—Removed by Right Lateral Thyrotomy—Recurrence on Left Vocal Cord—Removed by Left Lateral Thyrotomy.

By C. A. S. Ridout, M.S.

Patient, a man, aged 60, first seen August, 1922, complaining of hoarseness of long standing. On examination, a tumour the size of a split pea was seen attached to under-surface of right vocal cord near anterior commissure. Pathological examination of small portion removed showed epithelioma.

Operation, October, 1922.—Right lateral thyrotomy without tracheotomy, large window removed from right ala of thyroid cartilage. Right vocal cord and whole of mucosa lining the ala removed as far as processus vocalis of right arytænoid, also small portion of anterior third of left vocal cord.

Recurrence, May, 1924.—Small nodule size of a pin's head middle third of left vocal cord—removed for examination with forceps—proved to be epithelioma.

June, 1924.—Left lateral thyrotomy performed just as previously, except that greater part of left arytænoid was also removed.

Present condition.—On right side is seen a re-formed band of fibrous tissue in position of original vocal cord, movable by the action of the right arytænoid. On left side no movement owing to removal of left arytænoid, the resultant scar being crescentic in shape, concavity towards middle line. Patient can talk in a loud whisper and is in good health.

Discussion.—Sir StClair Thomson said the Section was indebted to Mr. Ridout for bringing this case forward; the cases in which laryngo-fissure was done were still so few that nearly every case should be reported, not only the successes, but also the failures, which were not published so frequently as they might be. Except for his own statistics, and those of Mr. Fraser, he (the speaker) did not know that anyone had published full records since the time of Semon and Butlin. Successful cases were shown from time to time.

In the present case there was a curious history of recurrence, of interest to others as well as to laryngologists. This patient had a recurrence on the opposite cord eighteen months after the first growth appeared. Everyone would agree that if a growth appeared within a year after laryngo-fissure had been done, the case was not one of recurrence, but of incomplete removal, not necessarily due to a fault of the surgeon, but perhaps because the case was unsuitable. After the lapse of a year, reappearance of disease indicated a fresh growth. He (the speaker) had had a case in which a re-growth took place seven years afterwards on the opposite cord. That record had, however, been beaten by a case of Mr. Tilley's, in which, if he remembered correctly, the recurrence took place thirteen years after removal. He (Sir StClair) had had a case in which he had removed the vocal cord on one side, and there was a re-growth of malignant disease in the lingual tonsil but on the other side three years afterwards. He had also had a case of malignant disease of the right ary-epiglottic fold which was removed by lateral pharyngotomy by Mr. Trotter. That case was shown to the Section as a brilliant success; but four or five years afterwards the patient had recurrence of malignant disease on the side of the pharynx opposite. These cases could not all be, as had been suggested, implantations at the time of the operation. To students of cancer it was interesting to observe that certain patients not only showed a tendency to re-growth of cancer, but seemed to possess a local predisposition to it. This point appeared to be exemplified in Mr. Ridout's case.

He wished to ask Mr. Ridout two questions: (1) What were his reasons for avoiding tracheotomy? (2) Why did he operate through what was known as "a window resection"?

Mr. Ridout might say he did so because he was satisfied that he was able to dispense with tracheotomy, and could operate through a window resection. But he (Sir StClair) thought the procedure violated some of the principles of surgery, one of which was that the operator should get as complete a view as possible of any malignant growth he was about to remove, and another of which was, that everything in surgery should be done with the utmost safety to the patient. Avoidance of a scar could not have been the reason, as this patient was elderly; besides, a scar was present. He (the speaker) held that if a tracheotomy was not done in these cases they ran a very serious risk. Any surgeon who had to operate in a number of cases would realize—in some cases too late—the risk incurred through omitting tracheotomy. There was a risk at the time of operation. Whatever might be seen, by indirect or direct laryngoscopy, one could not tell the extent of an intralaryngeal growth until it had been exposed; it usually ran much deeper than was thought. He (the speaker) and others had been surprised to find that a growth was largely subglottic. The growth in this particular case might have been found at the operation descending into the subglottic area, and even inside the cricoid cartilage, which, in some cases, he (the speaker) had had to divide. That could not be done through a window resection.

Secondly, when tracheotomy was done, one could make certain that not a drop of blood would go down into the air passages during the operation. There were not many, except the seniors, who knew the early history of this operation. He (Sir StClair) adopted it as it had been learned from Butlin and Semon, who at that time always did a tracheotomy because a Hahn's tube was used. They did not sew up all the neck wound, but left an opening, and those who came after followed that tradition. But while the operation had been made more safe it had subsequently been simplified. He (the speaker) used to leave out the tracheotomy tube, and tried sewing up completely, but, after trying both plans in doing over sixty cases, he came to the conclusion that tracheotomy was not only a great safeguard at the time, but also a great protection for the first twenty-four hours. He had been called to one case in which the patient was struggling for breath, and was afraid to cough; he (Sir StClair) quickly replaced the tracheotomy tube, whereupon the patient coughed up a large clot of blood. He (the speaker) had had about three cases of post-operative hæmorrhage, and if he had not had a tracheotomy tube in, it would have been impossible to check the hæmorrhage.

He recommended everyone to read again Mr. Wilfred Trotter's lectures before the College of Surgeons. In one passage the lecturer said: "It is extremely important that the growth" (that is, any malignant growth) "in this neighbourhood should be centrally placed in the part removed. Any excess in one direction is an unnecessary mutilation, whereas any diminution is a risk. In order that such an excision should be carried out deliberately and systematically, it is absolutely essential that the surgeon should obtain free and untroubled access. Provision of free access is, in fact, perhaps the most important part of the operation." He (Sir StClair) did not think that Mr. Ridout's method fulfilled that requirement. Mr. Trotter continued: "Free exposure is the first necessity. The second is that it should be exposed on its mucous surface." Having this in mind he (Sir StClair) thought it should be seriously considered whether tracheotomy and the splitting of the larynx did not constitute an absolute necessity in approaching all these cases of laryngo-fissure or partial laryngectomy.

Mr. H. TILLEY said that in the case mentioned by Sir StClair Thomson in which there was a re-growth thirteen years after the primary operation, the patient had been seen in consultation with Sir Felix Semon. He (the speaker) had removed a large growth from the right vocal cord, and thirteen years afterwards he was called to see the patient and found him in an almost dying condition from asphyxiation; he died the same night. The specimen showed a healthy scar in the previously operated region and an isolated new growth on the opposite vocal cord. He (the speaker) had had another case, which was operated on seventeen years previously, and he saw the man twelve hours before his death. A growth had appeared on the other cord after seventeen years of good health. The patient was under treatment for malignant disease of the prostate gland. He (Mr. Tilley) thought these were fresh and independent growths. Were they instances in which an immunity produced by the original growth had been lost?

Sir WILLIAM MILLIGAN asked whether the term "recurrence" was justifiable under the special circumstances mentioned by Sir StClair Thomson; he himself doubted it. In the present case he thought there was a new growth, not a recurrence. He raised the question whether this was not a contact infection, whether an initial stage of cancer formation was not set up on the opposite cord as a result of the contact. Cases of "contact cancer" (*cancer*

à deux) in the larynx had been recorded; such cases of contact cancer were common in the vulva. He (Sir William) congratulated Mr. Ridout on his case, but called attention to what he considered to be a commencing recurrence, as there seemed to be changes going on just in front of the right vocal process.

The question of tracheotomy was a very important one, and should be thrashed out by the Section. He agreed with Sir StClair as to the enormous advantage of doing tracheotomy beforehand, i.e., some days before the actual operation, as that gave the patient time to adapt himself to the respiratory changes. One of the great dangers from anæsthesia in these cases was œdema of the lungs.

Mr. RIDOUT (in reply) said that he was glad of the criticism, especially from Members of long standing. He agreed that he had been somewhat rash in omitting to do a tracheotomy, but the patient was very placid, and his (the speaker's) desire had been to avoid wounds in the trachea if possible. With regard to hæmorrhage: after the operation the patient was inverted, and he (Mr. Ridout) had waited several hours to see whether there was oozing before allowing the recumbent posture to be taken.

As to "access," if he had followed the ordinary laryngo-fissure route he would have cut into the growth, which was impinging on the anterior end of the left cord, and he thought that by doing a lateral window operation he would be able to get a good view and avoid cutting the growth. After removing a large portion, to make a window by submucous resection on the right side, and after injecting cocaine into the larynx to prevent coughing, he opened the mucous membrane well below and behind, and so obtained a good view. He found that the growth was close up to the anterior commissure, and he removed some of the cartilage of that commissure, also a little piece of the cord on the opposite side, with which the growth was in contact. The pathologist had reported that in that small portion of the cord there were karyokinetic changes. After the operation the patient had not failed in his progress; he was seen once a month. In May last year a tiny nodule appeared on the middle of the cord and that might have been a direct contagion from the original growth, but he (Mr. Ridout) thought it was too far back for that to be the case. It was probably a second growth. As the patient had done so well after the first operation he (the speaker) had been tempted to do a similar operation on the other side, and the patient had stood it well. He (Mr. Ridout) did not think there was yet any recurrence, but would keep the patient under observation.

Two Cases Illustrating the Comparative Failure of Ventriculo-cordectomy for the Relief of Double Abductor Paralysis.

By WALTER HOWARTH, F.R.C.S.

IN both cases the paralysis followed operations on the thyroid gland. In the elder woman a tracheotomy tube had been worn for ten years. In the younger woman not at all. Both cases have been operated upon twice by suspension laryngoscopy. In each case the immediate result was excellent, but as the new fibrous cord formed, as it inevitably must, the air-way became progressively diminished. A second operation involving a more drastic removal of tissue was followed by the same result.

The procedure is a simple one, but it is singularly ineffective, and would not appear to justify the claims that have been made for it.

Discussion.—Mr. E. D. D. DAVIS said that Sir James Berry had performed laryngo-fissure and eviscerated the larynx, dissecting away the soft tissues and vocal cords. In one case he had carried out this operation twice, but the tracheotomy tube had to be replaced a few months after each operation.

Mr. E. MUSGRAVE WOODMAN said that his experience of the operation was that the immediate result was pretty and successful, but his patient had coughed regularly all the time, and a fortnight after the operation she had had increasing dyspnœa; the cord had come down again and the tube had to be re-inserted.

Mr. RIDOUT said he believed Mr. Hobday had done a similar operation with considerable success on the horse, removing the whole mucous membrane of the left side of the larynx.

If one were to do a window operation on the thyroid cartilage and remove the whole contents on that side, one would probably get a contraction outwards, which would give a permanent airway. It would be better for the patient to have a whisper and no tracheotomy tube, than to be condemned to a tracheotomy tube for the remainder of his life.

Mr. E. D. D. DAVIS said Mr. Vlasto and himself had seen Mr. Hobday do this operation on the larynx of a horse; it was an easy and practical operation in the horse owing to the large larynx and the size and position of the ventricle, but it could not be applied to the human larynx. Dissections of the human larynx clearly showed that the operation could not be carried out successfully in man.

Mr. M. VLASTO said that in performing the operation of cordopexy operators were, perhaps, forgetting the law of cicatrization at an angle. Panas had shown that if two raw surfaces were placed in apposition, the healing of those surfaces always started from the angles. The recognition of this law found many applications in general surgery, as for example in the operation for separating the fingers in syndactylism. In their own specialty, he (Mr. Vlasto) might instance the operation for freeing the soft palate in cases in which over-cicatrization had followed the removal of tonsils. It was useless to try to relieve tension by linear incisions or cauterizations through the soft palate, because the raw edges would surely come together again. If, however, a perforation were made on either side of the base of the uvula with the point of the cautery, and the perforation allowed to heal, incisions made into the perforation would heal without reunion of the edges, and the tension would be relieved. With this fact in mind, he (the speaker) suggested that the operation of cordectomy should be restricted to the lateral structures of the constricted area and that the anterior and posterior commissures should be left with the mucous membrane intact.

Mr. HOWARTH (in reply) said that he had not removed the vocal process. He had undertaken these operations because Chevalier Jackson had published such glowing accounts of his operation in the American journals that he (the speaker) had thought it worth trying. He did not propose to discuss cordopexy, or any other modification, as in these cases he was simply concerned with Jackson's operation. He thought it must be regarded as a bad operation; he did not think a permanently satisfactory result could be achieved by it, though the immediate result was magnificent. In spite of various devices he had failed to prevent the onset of cicatrization. It should be remembered that each of these cases had been operated on twice and yet each showed a fibrous cord almost indistinguishable from the real cord. It would seem that one could be misled by a glowing account of an operation that was unsupported by histories of cases which had been followed up subsequently.

A Set of Modified Jackson's Tubes and Instruments for Peroral Endoscopy.

By WALTER HOWARTH, F.R.C.S.

HAVING felt that the American models of Jackson's well-known tubes and instruments were by no means mechanically perfect, I considered that the time had now arrived when it was advisable to devise some tubes and forceps which embodied all the advantages of Jackson's models but which abolished the dirt-holding grooves and crevices, the unnecessary nuts and other features which make these instruments so difficult to clean and keep in proper order.

In the present tube and spatula models it will be seen that the light-carrying tube no longer projects as an excrescence, but is incorporated in the main tube, whilst the collar at the end is also done away with and the sharp angle between the main tube and the aspirating tube is blocked up.

Handles and mounts are blackened to prevent undue reflection of light; the tubes are heavily gilt in order to obviate the possibility of nickel flaking off from contact with the patients' teeth, &c. Each tube and light carrier is clearly marked with its length and diameter. All the cords are interchangeable, as are the new English 2·5-volt lamps, which will be found to be more durable than those generally used.

The handle of the forceps for removal of foreign bodies has been re-designed and now takes a simpler form. On closure of the forceps the barrel of the tube moves up to the tip, and there is no "kick."

Two forceps handles are all that are required, as the forceps are provided with stainless steel centre rods on which the whole range of tips devised by Jackson, Brünings, Tucker and others, can be screwed. These tips, which are jet-black, show up plainly on the end of the forceps.

The swab carriers and hooks all have blackened ends.

In the standard set that has been constructed for me, two modified Mosher inflating œsophagoscopes replace two modified Jackson models, as I find the wider tubes more useful for foreign body work.

A pair of large optical plain glass spectacles for protection of the surgeon's eyes, aspirator, bellows and bougies are included in the set, which is put up in a strong mahogany case.

Discussion.—Dr. D. A. CROW (Brighton) said that Chevalier Jackson would certainly never claim finality in the design of his instruments, but since he had given so much care to the subject any suggested modification should be thoroughly examined before it was pronounced to be an improvement. He (the speaker) did not regard the blackened forcep tip as an advantage. Those who had been instructed by Jackson would remember the importance attached by that authority to the light reflex as an indication of the depth to which the forceps had gone. The interchangeability of the forcep-tips was a questionable advantage. One might require half a dozen forceps for a given case and the time occupied in making the change might be of vital moment in the operation. One could not afford to be economical in such work.

Mr. E. D. D. DAVIS said that he considered it an advantage to have the barrel of the forceps sliding over the point as in Mr. Howarth's modification.

Mr. HERBERT TILLEY spoke highly of Mosher's œsophagoscope with ovoid lumen, because it gave an excellent field for observation, and a tube containing radium or its emanations could easily be inserted into the growth. In these respects it compared favourably with Jackson's largest-sized tube. He (the speaker) was having a full-sized circular tube made with the distal light flush with the lumen of the tube, so that there was nothing to obstruct the free removal of gauze plugs or to hinder the insertion of full-size Souttar's tubes. The latter could not be passed through a Mosher's tube.

Dr. W. HILL said he agreed that one could not do work of any kind with a very small tube. Only the small bougies of Jackson would go through a Mosher tube. It was difficult to get a full-sized funnel down through any tube unless it was at least of 18 mm. diameter and circular. Jackson's tube did very well for examination, but for operative procedures the patient must be put under a general anæsthetic, and large, circular tubes must be used. Dr. Paterson, of Cardiff, had used a bicycle tyre—a tube 20 mm. in diameter—for the extraction of a mutton bone from the gullet. Since he (the speaker) had used large tubes, he had found the work comparatively simple. In making comparisons, however, Members must remember that Chevalier Jackson was a mechanical genius.

Sir WILLIAM MILLIGAN said that this was the most beautiful set of instruments he had ever seen. The idea of the blackened ends was an excellent one; it was a distinct advance. If circular tubes were substituted for oval ones, and were made larger, that would be a further improvement. Generally speaking, he (the speaker) agreed that the larger the tubes used, the better.

Mr. WALTER HOWARTH (in reply) said that it was not intended that these instruments should replace Hill's, or Brünings', or other tubes; they merely represented an attempt to secure a set of distally-lighted tubes for those who liked them, which would be useful on all occasions. He did not think the light reflex was of paramount value; a light reflex could be obtained from the point just before the tip. He agreed with Sir William Milligan that a rounder œsophagoscope would be better, and some were already being made larger and rounder in section.

Osteoma of Ethmoid.

By Walter Howarth, F.R.C.S.

The patient complains of nasal obstruction of three months' duration. A portion of the tumour presents into the nose, but the radiogram shows that the main mass is in the ethmoid. An external operation will be necessary for its removal.

Postscript.—A very large osteoma, involving practically the whole of the ethmoid, was removed by the external route.

Acute Pulmonary Infection following Operation on the Maxillary Antrum.

By Dan McKenzie, M.D.

Patient, a male, aged 33, underwent double nasal antrostomy February 13, 1923. Six days later pleurisy on the right side set in, followed by general bronchopneumonia, with, later on, several attacks of hæmoptysis. Temperature rose and continued elevated with daily remissions for about eight weeks (chart shown), when patient was removed first to the Victoria Park Hospital, and afterwards to the Hospital for Consumption, Brompton. Here X-ray examination suggested an abscess in the lung, but after several exploratory operations an empyema (? encysted) was discovered and drained by Mr. Roberts, with complete recovery. (The sputum was free from tubercle bacillus throughout.)

Several cases of acute pulmonary infection of various kinds, two of them fatal, have come under my observation, and in every one of them it was the maxillary antrum which had been the site of operation. The infection does not seem to attend one variety of operation more than another, as it occurs after the radical as well as after the intranasal operation. The route by which infection reaches the lungs is not quite obvious. The inspiration of septic material during general anæsthesia may perhaps be responsible, and, for that reason, it is a wise precaution to have the cavity washed out immediately before operation. If this should prove to be the most usual route, the argument in favour of operating under a local anæsthetic would be strengthened. It is difficult, however, to exclude the possibility of infection through a venous channel.

These cases are fortunately uncommon. The most usual septic sequela of operation on the nose is acute tonsillitis, and this may be terminated by a 25 c.c. dose of anti-streptococcus serum.

Discussion.—Mr. G. H. Wilkinson said that on account of the danger of infecting the lungs he would suggest intratracheal intubation in these cases. Nine years ago he (the speaker) had had a similar case, which ended fatally; he had then decided that intubation should be done in every case of throat operation in his clinic, and that practice had been followed ever since. This procedure prevented the entry of fragments of bone or mucous membrane into the lungs, and the time taken in passing an intubation tube was more than saved by the ease of the anæsthesia. He (the speaker) now employed the same method even for mastoid operations.

Dr. P. Watson-Williams said he doubted whether infection did take place during the anæsthesia, and he did not think it was only then that there was danger of pulmonary complication. It was a rare complication following operation on the maxillary antrum, but was one which could not always be avoided—for instance, it might be due to infection entering through venous or lymphatic vessels in the periosteum, &c. Infection might take place through the inhalation of septic particles, but this possibility was almost as great after as during anæsthesia.

Dr. JOBSON HORNE said that in order to prevent the inspiration of septic matter into the lungs during an operation upon the maxillary antrum, or during any intranasal operation, it had been his (the speaker's) invariable practice to pack the post-nasal cavity with a marine sponge tied round with a silk ligature passing out through the mouth. This caused no inconvenience to anybody, it saved much trouble during the operation, and it minimized the possibility of inspiratory infection.

With regard to the choice of operation on the antrum, the results of nasal antrostomy that had come under his (Dr. Jobson Horne's) notice had not been sufficiently satisfactory to lead him to adopt the nasal in place of the combined oral and nasal operation which had given him satisfactory results.

Dr. LOGAN TURNER (President) said that the possibility of embolic infection could not be excluded. All these cases were not inhalation infections. Further, the complication was not limited to those cases in which the patients had general anæsthesia, as it had arisen in operations on the maxillary antrum after local anæsthesia had been given.

Dr. DAN MCKENZIE (in reply) said that these accidents were disconcerting, because the post-operative illness was then out of all proportion to the pre-operative conditions. In doing the external operation one was faced by the possibility of infecting the soft tissues; and osteomyelitis might occur as a sequel. In America, tonsillectomy was done under local anæsthesia, and there had been many cases of acute pulmonary infection following that operation, in which there was no possibility of blood reaching the bronchi. The question needed to be thoroughly ventilated.

Case of Lympho-Sarcoma of the Pharynx and Naso-pharynx.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

(Previously shown at the meeting of March 6, 1925.)

PATIENT, a man, aged 38, with immense infiltration of glands of both sides of the neck, which had developed with considerable rapidity about five months ago. When shown at the March meeting the naso-pharynx was encroached upon by a hard, vascular, non-ulcerated growth in the soft palate, while in the naso-pharynx itself there was another smooth, firm growth of about the size of a large grape, growing from the left side wall. Breathing through the nose was absolutely impossible. A slight diminution of the glandular swelling had taken place under exposure to X-rays, and it was proposed to introduce radium into the growth in the naso-pharynx.

On March 11 I introduced two radium needles through the left nasal fossa into the naso-pharyngeal growth; they were left in position for twenty-four hours and breathing began to be possible through the nose; when I saw him again at the end of a week nasal breathing was quite easy. On April 1 I introduced three radium needles into the palatal growth and the diminution in a week was most marked, while at the same time the glandular mass on the left side of the neck shrank with greatly increased rapidity.

The change in the internal appearance is shown in the drawings exhibited.

A microscopical examination of a gland which was removed was reported as showing lympho-sarcoma.

Discussion.—Sir WILLIAM MILLIGAN said he must congratulate the exhibitor on the result, but he (the speaker) did not think Sir James had proceeded far enough; the treatment was incomplete. Unless a block dissection of the glands was done, recurrence was almost certain. He asked what had been the total dose and the total exposure in this case.

Sir JAMES DUNDAS-GRANT (in reply to Sir William Milligan) said that in the first instance three tubes of 25 mg. each had been introduced, and that they were kept in for forty-eight hours; on the second occasion three tubes, each of 5 mg., kept in for twenty-four hours.

Sir WILLIAM MILLIGAN said that this was a fair total dose for the nature of the growth. In such a case a combination of X-rays and radium was excellent; there seemed to be some physical affinity between the two. In the present case he (Sir William) thought that the nose

was not quite clear, and that some intranasal treatment was needed. In such instances, attention should always be given to excision of the glands. Team work was necessary, i.e., work in co-operation with a general surgeon and a radiologist, who should decide by means of the iono-micrometer the most suitable dose for the given case. Hitherto dosage had been too empirical and haphazard. The tendency to-day was not to use tube emanations of radium, but the element itself, and to leave it *in situ* for much longer periods, such as a fortnight or three weeks. Tube emanation was practically useless after three days. In 3·75 days 50 per cent. of its radio-activity had vanished.

Dr. ANDREW WYLIE said that he had shown at a former meeting of the Section a case in which the patient had contracted erysipelas and had been cured. It might be worth while to experiment on such cases with the germs of erysipelas.

Sir JAMES DUNDAS-GRANT (in reply), said that to deal with these glands at present would be very difficult, as they were so firmly fixed and so extensive. X-ray treatment was being continued. An extraordinarily rapid improvement had taken place after radium had been introduced. There was still some thickening at the back of the nose, and perhaps the growth in the palate had not quite disappeared. He thought the latest idea in radium treatment was to introduce a weak dose of the element and leave it *in situ* for a long time, and that this gave good results, even in epithelioma of the tongue. He would report again on the case.

Specimen and Lantern Slides illustrating an attempted Ingestion of a 10-in. Table Knife during Puerperal Insanity.

By E. MUSGRAVE WOODMAN, M.S.

PATIENT, a woman, aged 34, was suffering from slight puerperal insanity. In an attempt to commit suicide she swallowed a large poultry knife. She said nothing about the condition for fourteen days, but at the end of that time she complained of her throat and was admitted to hospital.

X-rays showed a large knife apparently in the thoracic œsophagus.

Œsophagoscopy under local anæsthesia showed a slit on the posterior wall of the post-cricoid region of the pharynx. A tube was passed through this into the posterior mediastinum, which was gangrenous and full of food débris.

A lateral œsophagotomy was rapidly performed under an anæsthetic and the handle of the knife felt through the opening into the right pleural cavity; the knife was extracted between two fingers.

She died two days later from double pneumonia.

Two Cases of Anatomically Irregular Sphenoidal Sinusitis with Defective Vision, illustrating the use of the Suction Syringe in localizing Focal Infection.

By P. WATSON-WILLIAMS, M.D.

IN these two cases there was undoubted infection of the sphenoidal sinuses and posterior ethmoidal cells, with well-marked toxic ocular defects, but, in both, the irregular development of the sphenoidal sinuses afforded technical difficulties in localizing the precise source of infection.

In the first, a large over-developed left sphenoidal sinus, encroaching on the opposite side, was infected, while the small under-developed right sphenoid was not seriously involved.

In the second case the small ill-developed right sinus was infected, while the large over-developed left sphenoidal sinus was not infected. The detection and drainage of the small tucked-away right sinus gravely influenced the patient's serious visual defect as well as his general well-being.

(Illustrated by the epidiascope.)[1]

[1] The full account of the cases, together with visual colour field charts, will be reported in *The Lancet*.

Discussion.—Mr. G. L. GIMBLETT discussed these cases from the ophthalmic point of view. The ophthalmic surgeon, in cases of defective vision charted the fields of vision, found a contracted field, and set out to discover the reason for the defect. The first person whose assistance he sought was the rhinologist, and when the rhinologist reported that there was nothing wrong with the sinuses the ophthalmic surgeon was handicapped, and time was lost. There were two quite separate methods. One was for charting the peripheral field, by McHardy's method, the other was for charting the central field. One could not measure the size of the blind spot accurately with McHardy's perimeter. For this a Bjerrum's screen or Bishop Harman's scotometer was necessary. In the case of sphenoidal sinus infection both eyes showed contraction of the nasal visual field, but on one side more than the other. When, as in these cases, the nasal side was especially reduced, the ophthalmic surgeon always thought of glaucoma as the explanation. It was very important to chart the fields for colours, for while the field for white might be full, the field for red or blue might show almost hemianopia. He (the speaker) had learned much from these two cases.

Mr. E. D. D. DAVIS asked whether Mr. Gimblett had seen many cases of retrobulbar neuritis arising from nasal sinus suppuration.

Mr. GIMBLETT (in reply) said that he had already found an unpleasantly large number of cases in which sinus infection had been discovered when the visual fields were contracted. The infection had not been limited to the sphenoid; the ethmoidal cells, both anterior and posterior, had also been infected.

Dr. P. WATSON-WILLIAMS (in reply) said that Mr. Gimblett's remarks directed attention to the need for great care on the part of the ophthalmic surgeon when examining the cases referred to by the rhinologist. Not being content with examining the fields merely for white he must examine them for colour, enlarged blind spots and scotoma. Moreover, every possible source of infection must be explored by the rhinologist, and every cavity, whether infected or not, must be investigated and the contents bacteriologically examined before one could locate infection—or exclude a nasal source of infection—in such cases.

Nævus of the Epiglottis and Tongue.

By HAROLD KISCH, F.R.C.S.

PATIENT, female, aged 39, married. History of repeated and severe hæmoptyses since the war. Nævoid condition of epiglottis and vallecula. Suggestions as to treatment invited.

I saw this patient two months ago; a few days previously she had had a large hæmorrhage. The veins on the right side were enormously dilated in comparison with those on the left, and on that day they spread on to the epiglottis. The condition has now largely subsided, but the veins on the right are still larger than those on the left.

Case of Thyroglossal Cyst.

By A. TWEEDIE, F.R.C.S.

MRS. N., aged 31, alleged to have undergone an operation on the throat seventeen years ago. There is a healed T-shaped scar on the middle line in the hyo-thyroid area, corresponding probably to the removal of a thyroglossal cyst. Apparently no normal thyroid gland is present, but in the neighbourhood of the foramen cæcum on the dorsum of the tongue there is a firm, globular swelling, diagnosed as a "misplacement" of this gland.

Patient applied for treatment, February, 1925, complaining of trivial discomfort in her throat of some six weeks' duration.

The case is shown for advice as regards procedure, the suggestion being that no treatment at present is indicated.

(Members are referred to an article on the subject in the *British Journal of Surgery*, January, 1925, by Mr. Hamilton Bailey.)

Sir JAMES DUNDAS-GRANT said that the swelling did not seem to be causing the patient much discomfort, and there appeared to be no urgent need for interference. If, however, the thickness of speech was an annoyance, he (Sir James) would suggest the removal of a portion of the swelling by transfixing it with a needle from before backwards, and then applying the galvano-cautery snare. He (the speaker) had done this on one occasion and in that case there had been no post-operative trouble whatever. If the growth were removed entirely the patient would probably suffer from myxœdema all her life.

Pharyngeal Growth in a Man aged 70.

By GEORGE BADGEROW, C.M.G., F.R.C.S.Ed.

WHEN first seen by me at Golden Square, the patient complained of an ordinary sore throat with discomfort in the right side when swallowing. The laryngoscope showed a growth attached to the epiglottic pharyngeal fold.

Mr. Wilfred Trotter confirmed the diagnosis of epithelioma. Lateral pharyngotomy was performed by Mr. Trotter. The patient made a good recovery.

Nasal Tumour, Probably of Tuberculous Origin.

By N. S. CARRUTHERS, F.R.C.S.

R. N., FEMALE, aged 13. Always been a healthy child. About eighteen months ago it was noticed that the right side of her nose was becoming deformed. The condition gradually grew worse. No pain, no epistaxis, no nasal discharge. Wassermann negative.

Present Condition.—There is a semi-solid mass on the right side of the nose, which appears to be expanding the junction of the frontal process of the maxillary bone and the nasal bone. The nostril is obstructed by a bluish mass covered with normal mucous membrane. Post-nasal space and choana are normal. Trans-illumination shows right antrum to be dull. X-ray shows septum pushed to opposite side, right antrum opaque, but no bony change.

When first seen by exhibitor twelve days previously, the swelling appeared universally solid.

There is a large submental gland and there is some thickening of the *left* side of the lower jaw.

Postscript.—An operation was subsequently performed. The "tumour" was very vascular, the nasal process of the maxilla (together with the inner wall of the antrum) was necrotic and was removed with a sharp spoon. There was no pus. The submental gland was removed and showed scattered foci of suppuration. Microscopically, both the primary growth and the gland suggest tubercle.

Epithelioma of Right Vocal Cord.

By J. ALDINGTON GIBB, M.B.

PATIENT, a male, aged 54, labourer. Admitted January 3, 1925.

History.—Hoarseness for some months. Lately, some pain low down in throat.

Diagnosis.—Epithelioma of right vocal cord.

January 5, 1925: Thyrotomy with tracheotomy. Right vocal cord with half of right ala removed.

January 9: Tube removed; breathes freely and swallows; throat well.

January 19: Sudden collapse and difficulty in breathing; much pus in larynx; tube replaced; strychnine given hypodermically.

January 23: Much improved.

Present condition.—Considerable narrowing of larynx due to adhesions. Wears a tube.

Remarks.—The operation was delayed until patient had recovered from parotid abscess. It is probable that the infection of his larynx was due to his low immunity. He was also an alcoholic. The question is whether dilatation of the larynx is necessary. There has been some improvement in air-space and this may increase still further.

Disease of Anterior Ethmoidal Cells, Causing Optic Neuritis.

By J. ALDINGTON GIBB, M.B.

PATIENT, male, aged 22; a gardener; admitted to casual department from eye department suffering from receding optic neuritis (left), severe supra-orbital headache, and occasional occipital pain. Giddy and sleepy at times.

I was requested by my colleague Mr. Potts to examine the patient's nasal sinuses with the object of discovering the cause of the optic neuritis.

On examination.—Mucous membrane of nares dry: scanty thick pus down in middle space descending from under surface of middle turbinal. No tenderness over frontal sinus. In postnasal space pus was seen winding round the Eustachian cartilage. Transillumination: Maxillary antrum shadow. Frontal sinus *nil.*

Diagnosis.—Disease of anterior ethmoidal cells.

Ophthalmological Report.—September 27, 1924: Right vision = $\frac{6}{5}$ (part); left vision = $\frac{1}{36}$. Slight hyperæmia of left disc; physiological pit filled in; lamina not visible.

October 12: Left vision = $\frac{6}{60}$; operation under chloroform and gas; anterior ethmoidal cells opened on right and left sides; anterior end of middle turbinal punched off; maxillary antrum left opened; pus present. Forty-eight hours after, left vision = $\frac{6}{36}$.

October 27: Left vision = $\frac{6}{18}$; fundus satisfactory; very little discharge from nose.

November 29: Left vision = $\frac{6}{12}$; no active optic neuritis; discharged.

Remarks.—I have had four of these cases of anterior ethmoid disease causing optic neuritis. I never remove the whole of the middle turbinal. In two I have completely exenterated the cells and have felt the instrument to be in contact with the soft tissues of the orbit.

Soft Fibromata of Larynx followed by Malignant Disease.

By W. H. JEWELL, O.B.E., M.D.

PATIENT, H. H., male, aged 61. This case was shown at a meeting of the Section about four years ago as one of soft fibromata of the larynx, and those who examined the patient on that occasion agreed with the diagnosis. He gave a history of five months' hoarseness and shortness of breath.

On examination, soft fibromata could be seen nearly filling the larynx, obscuring the vocal cords and leaving a central airway about the size of a small quill. A few days after the meeting, during a violent fit of coughing, the growths were expelled. Some of these were said to be the size of a hazel-nut. Similar pieces had been coughed out on previous occasions. When I saw him a day or two later his voice was natural, the vocal cords were active and clean, his respiration free, and I did not at that time (March, 1921) discover any evidence of malignant disease. He returned to his heavy work—carrying sacks of malt from the holds of barges up two ladders to the store—without the least inconvenience.

Three days ago he returned to hospital complaining of hoarseness of one year's standing and of shortness of breath.

The left vocal cord is fixed and infiltrated, and there is also a ridge of infiltration above the anterior commissure, but nowhere in the larynx can any growth be seen resembling the soft fibromata previously observed. No glands palpable. Wassermann negative, January, 1921.

Postscript.—The condition now appears to be malignant, and this was confirmed by those who examined the larynx at the Meeting, May 1, 1925.

Specimen of Larynx and Œsophagus, from a Case of Paralysis of Left Recurrent Laryngeal Nerve and Tuberculous Ulcers of the Œsophagus, all caused by Caseous Tuberculous Glands.

Shown by HERBERT TILLEY, F.R.C.S.

THE specimen was removed from an adult female who was under the care of Professor T. R. Elliott, at University College Hospital. He has kindly supplied the following report and history.

PATHOLOGICAL REPORT AND CLINICAL HISTORY BY T. R. ELLIOTT, M.D., F.R.S.

The left recurrent laryngeal nerve is stretched over a black strip immediately above the level where it was caught by a caseous lymphatic gland.

The larynx is not diseased.

In the œsophagus are three ulcers. The uppermost is shallow, but the middle one extends by a deep track into a mass of breaking-down caseous glands at the bifurcation of the trachea. A white rod has been passed down the track, which did not actually open into the bronchus.

There was a shallow tuberculous ulcer in the stomach, also caused by inward spread from an adjacent caseous gland.

Clinical History.—Woman, aged 68, who died in a state of weakness and emaciation, with caseous mesenteric glands and extensive tuberculous ulceration of the bowels. Probably this was the starting point of infection.

The lungs showed only recent miliary tubercles. There was no sputum, but tubercle bacilli were found in the fæces.

The patient's voice became hoarse during an attack of "influenza" eighteen months before death and never recovered. Her bodily weakness dated from the same illness. The larynx was examined by Mr. Herbert Tilley ten days before death,

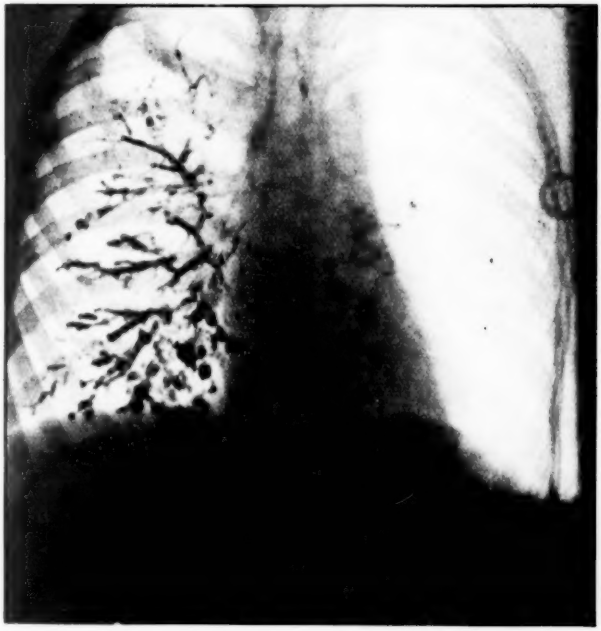
and "the left vocal cord was found to be paralysed and motionless in the middle line, owing to paralysis of the abductor muscles."

The patient mentioned a little pain in the throat: she said that there was no difficulty in swallowing solids but that "fluid went the wrong way and *caused coughing*" (n.b., track alongside trachea). No dysphagia was noticed in hospital during the fortnight that she survived, and the œsophagus was not examined.

X-ray Films illustrating Lung-mapping with Lipiodol in a Case of Bronchiectasis.

By WALTER HOWARTH, F.R.C.S.

IN this instance the patient was anæsthetized, and 5 c.c. of lipiodol were injected into each main bronchus through a bronchoscope. This method is more certain and exact than that of intratracheal injection. In most cases an anæsthetic is not necessary.



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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Neurology.[1]

President—Dr. E. Farquhar Buzzard.

A Case of Syringo-bulbo-myelia with Unilateral Nerve-deafness and Immobility of the opposite Vocal Cord.

(By permission of Dr. Worster-Drought.)

Shown by Sir James Dundas-Grant, K.B.E., M.D.

Patient, a man aged 46, referred to the Throat and Ear Department of the West End Hospital for Nervous Diseases, in 1922, with right-sided deafness and a change in the character of the voice. The deafness, as proved by the tuning-fork tests, was of the "nerve" type, but the relative loss of hearing for the highest-pitched tones (Galton's whistle) was not so marked as in the more typical cochlear diseases, such as syphilis. Bárány's caloric test (carried out by means of cold air) showed loss of reaction (nystagmus, past-pointing and falling), and this was also the case with the galvanic (Babinsky) reaction, showing that the disease was situated more deeply than the internal ear—probably in the acoustic nucleus. The reactions on the left side were normal to both tests. There was complete paralysis of the left vocal cord, while the palate was so paretic on both sides that hemiplegia of the velum, though probably present, could not be distinguished.

Dr. Worster-Drought has stated that the onset of the condition occurred, during 1917, with numbness of the fingers, while about a year later weakness and wasting of the hands appeared. Results of physical examination: Pupils equal and reacting to light; fundi normal; tongue deviates to the left on protrusion. Bilateral atrophy and weakness of the intrinsic muscles of the hands; arm muscles generally atrophic with occasional fibrillary tremors. Abdominal reflexes absent; knee and ankle-jerks exaggerated; plantar reflexes extensor. An area of anæsthesia to pain and thermal stimuli extends upwards from the sixth thoracic segment, involving both arms, reaching posteriorly to above the occiput and anteriorly to the level of the lower lip.

Sir James Dundas-Grant commended to the neurologist the apparatus devised by himself for the application of cold air in the ear for carrying out the labyrinthine tests, as free from the inconvenience and danger attending the use of cold water for the purpose. He demonstrated its use in the case under consideration.

Two Cases for Diagnosis.

By Philip Cloake, M.D.

Case I.—H. D., aged 69, a pensioned cooper.

Thirty years ago noticed right leg was smaller than left. *Fifteen* years ago left hand noticed to be weak; does not know what movements were affected. About this time suffered much from vomiting and colic, and had frequent morphia injections. Had been working as a cooper and painting casks: used a quantity of white lead. Right hand noticed to be wasting some years ago—does not remember when. Considerable increase in weakness of legs in last six months. Never any pain. All toes amputated for gangrene seven years ago. Began as painless discharging ulcer

[1] Clinical Meeting held at the West End Hospital for Nervous Diseases, Welbeck Street, W.1, December 11, 1924.

of left hallux, which was ultimately amputated. One month later other toes turned black and were amputated.

Family History.—Mother died, aged 79, ? cause. Father died, aged 54; legs deformed, difficulty in walking. Three sisters alive, one is the patient whose case is recorded below. Two sisters died, one of cancer, one of "fever."

Examination.—Memory impaired. Well preserved physically. Early cataract of right eye. Fundi (?). Pupils normal. Transverse fissuring and fibrillation of tongue. Tremor of left side of upper lip on voluntary movement, otherwise cranial nerves normal. Arms: Wasting of thenar, hypothenar and interosseous muscles of hand, still progressing. Slight wasting of back of left forearm. Right grip fair, left grip weak. Weakness not severe as compared with wasting. No thickening of ulnar nerve. Slight fibrillation of muscles of forearm. Ulnar side and little fingers of both hands show slightly diminished sense of pin-prick. Posture and vibration senses normal in arms. No ataxy of arms with eyes open, slight with eyes closed. Arm tendon reflexes all absent. Coarse tremor of hands. Abdomen: Sense of pin-prick normal. Reflexes: Just obtained on both sides. Slight difficulty in sitting up, great difficulty in standing up and sitting down. Lower limbs: Movements at hip weak, at knee fair though weakened. Marked weakness at ankles, especially dorsi-flexion, kicks his toes into stones and falls. Slight inversion right foot. Fibrillation lower part of thigh muscles. Legs moderately wasted, especially anterior and external groups. No definite wasting of thighs. Legs not ataxic. Pin-prick and postural sensibility normal, except dulling to pin-prick over ends of feet. Soles tender, vibration sense impaired, right more than left. Knee-jerks and ankle-jerks absent. Plantars not obtained, legs drawn up. Sphincters normal. Feet blue and cold. Œdema at night. Hands cold, numbed and weak in cold weather. *A remarkable symptom is profuse sweating of the face and forehead shortly after drinking or eating anything hot.* Wassermann reaction negative; cerebro-spinal fluid colourless, 1 cell per c.mm.; globulin negative; albumin 0·025 per cent.; Wassermann reaction negative; Lange's gold curve ten O. Power of limbs improved considerably under massage and electrical treatment.

Case II.—This case was not shown. Mrs. A. W., aged 64, married. Sister of H. D. For five years has had difficulty in walking. Right foot weaker since an injury three years ago. Cannot get up and down stairs without holding banister. Hands go white and get numbed. Feet numbed and tingling, worse in cold weather. No marked sweating.

Examination.—Argyll-Robertson pupils, otherwise cranial nerves normal. Arms: Slight weakness at wrists. Grips much weakened, right more than left. Considerable wasting of interossei and adductors of thumb, right more than left. Slight wasting right thenar and hypothenar eminences. Postural sense slightly impaired in right hand. Pin-prick normal. No ataxy of arms. Hands red and cold. Abdomen: Sense of pin-prick normal. Reflexes: Right absent, left very sluggish. Lower limbs: No marked wasting. Slight flattening of peroneal region more on right side. Hip and knee movements fair. Weakness at ankles, dorsiflexion especially and right side greater than left. Sense of pin-prick normal in legs. Postural sense very defective right foot, less defective left foot. Vibration sense slightly impaired in both legs. No ataxy of legs. Knee-jerks and ankle-jerks absent. Plantars could not be elicited. Feet cold. No sphincter trouble.

A Case of Dyspituitarism following Encephalitis Lethargica.

By C. Worster-Drought, M.D.

J. M., aged 14. In December, 1921, he developed severe headache and giddiness and was admitted to hospital suffering from encephalitis lethargica. He remained "unconscious" for thirty-three days and was in hospital for three months. Since

his convalescence he has gradually increased in bulk and has been most difficult to manage, his disposition having completely altered. There is no family history of significance.

His present symptoms include excessive adiposity, vague pains in the limbs and body, frequent vertical headaches, restlessness and irritability, lack of mental concentration and frequency of micturition.

Physical Signs.—He exhibits pronounced general obesity with broad hips and well-developed breasts, smooth, soft skin and early pubic hair. The testicles and external genitalia are normal. His height is 5 ft. 3½ in., and weight 13 stone. Muscular power relatively weak. Circumference measurements of limbs: upper arms, 12 in.; forearm, 10 in.; thigh, 21½ in.; and calf, 16 in. His temperature is invariably subnormal, never reaching above 98° F.; the heart, lungs and abdominal viscera show nothing abnormal, the pulse-rate averages 72, and the blood-pressure is 110 mm. Hg systolic and 75 mm. diastolic. The urine shows no abnormality. Central nervous system: the pupils are equal and react normally; visual fields and optic discs are also normal. The abdominal reflexes are brisk and equal, knee- and ankle-jerks normal and the plantar reflexes flexor. There is no disorder of sensation or co-ordination. The cerebro-spinal fluid contains only five lymphocytes per c.mm., with a normal protein content and negative Wassermann and colloidal gold reactions. The blood shows 5,750,000 red cells per c.mm. with a general reduction in size of the corpuscles and moderate anisocytosis. The hæmoglobin is 86 per cent. and the colour index 0·75. The white cells show both an absolute and a relative lymphocytosis, viz., 10,600 cells per c.mm., with 47 polymorphonuclears, 50 small lymphocytes, 0·5 large mononuclear and 2 eosinophils per cent. X-ray examination reveals no abnormality of the sella turcica. Sugar tolerance is increased, no glycosuria following the ingestion of 300 grammes of dextrose. The resting blood-sugar is 0·11 per cent. The blood-sugar curve—following the oral administration of fifty grammes of glucose—is abnormal in that (1) the apex is not reached for one and a half hours (as compared with half an hour in the normal subject), (2) the altitude is abnormal, reaching only 0·14 per cent. (normal 0·17 per cent.), and (3) the blood-sugar does not regain the normal level within two hours. Mentally, the boy is bright and intelligent, but extremely difficult to control. He is wayward and irritable, lacks discrimination between right and wrong, and loses his temper upon very slight provocation. A headache renders him violently hysterical. For any offence he expresses great sorrow and repentance; in spite of protestations to the contrary, the offence is repeated. It has been impossible to retain him in hospital owing to his not being amenable to the slightest discipline. His relatives state that his disposition was very good preceding his illness and that he never gave any trouble whatever.

Dr. WORSTER-DROUGHT said that the general adiposity, normal genitalia, relative lymphocytosis, increased sugar tolerance and blood-sugar curve suggested a dyspituitarism of the Dercum type rather than of the Fröhlich type. Both types had been ascribed to decreased function on the part of the posterior lobe of the pituitary gland; had the condition developed at an earlier age in this patient, the dyspituitarism would probably have been indentical with the Fröhlich form. Since Camus and Roussy had shown that typical dyspituitary syndromes could be produced in dogs by comparatively slight injury to the base of the brain, above but not involving the pituitary gland, it was interesting to speculate how far the condition in this case might be due to involvement by the encephalitic process of the nuclei of the tuber cinereum, rather than of the posterior lobe of the pituitary gland itself.

Congenital Bilateral Athetosis.

By C. WORSTER-DROUGHT, M.D.

E. B., AGED 11, presents generalized, involuntary and irregular movements which have existed since birth.

She is the eighth of ten children. One sister, now aged 26, is quite normal, but

the other eight children all died with jaundice between the ages of three days and four weeks ; of these latter, only one was premature. The patient was born at full term, labour being normal. She walked at 5½ years but has never talked.

The generalized movements are irregular, jerky and wriggling, and are most pronounced in the face, neck and upper limbs. The face is constantly grimacing and the tongue and soft palate are also affected. The movements are increased by emotion but tend to subside on purposeful movements, e.g., she can raise with one hand and drink a glassful of water without any being spilt. The lower limbs are only slightly affected. She is able to walk without assistance in a somewhat ataxic fashion, the gait being punctuated at frequent intervals with asymmetrical jerky movements of the legs. In walking, she tends to deviate either to the right or left from a straight line. There is no spasticity, muscle tone being normal ; the knee and ankle-jerks are brisk and equal. The abdominal reflexes are present and equal, plantar reflexes flexor, and sphincters normal. The Wassermann reaction is negative both in blood and cerebro-spinal fluid, the latter being normal in all respects.

She has never been to school and can neither read, write, nor dress herself, but in her habits she is quite clean. Although unable to articulate, she can phonate, the only recognizable word being "more." From time to time, she emits a sound resembling "ah wah!" She understands most things said to her and when her attention is obtained, she will carry out correctly simple commands such as "Eva, get out of bed!" or "Strike a match!" &c. If the command does not interest her very much, it has to be repeated several times before she will obey, e.g., "Touch your nose!" "Clap your hands!" &c. If asked to imitate movements, she does so quickly and correctly.

Dr. WORSTER-DROUGHT said that the congenital athetosis was termed "congenital, bilateral athetosis," owing to the lack of a more suitable name. Although grimacing was present the movements of the limbs were more choreic than athetoid. The condition was that particular type of so-called "cerebral diplegia," in which the agenesis affected the corpus striatum, in all probability the putamen and caudate nucleus particularly.

An Unusual Case of Myopathy.

By C. WORSTER-DROUGHT, M.D.

J. U., MALE, aged 48, complains of "wasting and weakness of the thigh muscles and weakness of the back." The condition developed in 1917, when he began to complain of some pain and weakness in both legs from the thighs to the feet. Since that time the thigh muscles have gradually atrophied. Weakness of the back has been noticed since 1919. There is no family history nor previous illness of significance.

Physical Signs.—The pupils are equal and react normally. There is some temporal and malar hollowing, but all cranial nerves are otherwise normal. There is some lordosis and slight scoliosis to the right in the lower dorsal region. The erector spinæ muscles are weak and slightly atrophied. The muscles of the upper limbs show no abnormality and all arm reflexes are equal on the two sides. There is pronounced atrophy of both thighs. Circumference measurements : Thighs, right 13 in., left 13½ in., as compared with right calf 15½ in. and left calf 15 in. Voluntary movement is present, though weak, in the atrophied muscles ; they are not unduly atonic, nor has any fibrillation been observed. All other lower limb muscles are normal. The knee-jerks are very sluggish, the left being slightly greater than the right ; the left quadriceps reacts to direct percussion, but not the right. The ankle-jerks are brisk, the left being very slightly greater than the right. The abdominal reflexes are normal and equal and the plantars flexor. There is no abnormality of sensation, but patient is slightly unsteady in the Romberg position.

To faradism, all the thigh muscles react fairly well and in proportion to the amount of muscle present. There are no joint lesions in the hips or knees. The heart and lungs are normal, pulse 86, arteries slightly thickened and systolic blood-pressure 140 mm. The retinal vessels show arterio-sclerotic changes. Some varicose veins are present in the lower limbs. X-ray examination shows some atrophic decalcification at the lower end of both femurs, but no arthritic lesions in hips, knees, or spine. The blood yields a negative Wassermann reaction and the cerebro-spinal fluid is normal with negative Wassermann reaction.

Dr. WORSTER-DROUGHT said that the distribution of the muscular atrophy was somewhat unusual. A so-called "reflex muscular atrophy" secondary to arthritis of the hip or knee-joints was excluded by the clinical and X-ray examination. The distribution, the absence of fibrillary tremors, the retention of the knee-jerks, the fairly good voluntary movement and the faradic reactions were against a diagnosis of progressive muscular atrophy. Also, in spite of the history of some pain, the perfect retention of sensation and the electrical reactions rendered a post-neuritic atrophy unlikely. The condition, therefore, appeared to be a primary muscular dystrophy of unusual type—what might be termed a "proximal myopathy."

Case for Diagnosis.

By H. C. BECCLE, L.R.C.P.Lond., M.R.C.S.Eng.

E. H., AGED 27, under the care of Dr. Worster-Drought. The patient complains of involuntary starts, difficulty in speaking, defective memory, occasional diplopia and disturbed sleep. The onset of the condition has been gradual since 1918 but during the last twelve months has become considerably worse.

Physical Examination.—The pupils are equal and react normally; the optic discs are normal. Horizontal nystagmus to the right. Speech hesitant, jerky and explosive. Other cranial nerves normal. Right arm reflexes slightly greater than left. Slight intention tremor, more pronounced on right than left; co-ordination of arms otherwise good. Abdominal reflexes not elicited. Knee-jerks very sluggish, right greater than left; right ankle-jerk absent, left sluggish. The feet show a tendency to pes cavus and both plantar reflexes are extensor. Sensation is normal. He exhibits occasional and involuntary "starts" consisting of extension of the upper part of the back and elevation of the shoulders. He is slightly ataxic and shows positive Romberg's sign. Mentally, there is considerable confusion and regression. His wife states that he has an inclination to wander and "lose himself," but that he is tractable and easily managed. He relies unduly on his wife and most questions asked him are referred to her.

Cases of Epilepsy, General Paralysis and Disseminated Sclerosis, to illustrate Speech Defects.

By E. W. SCRIPTURE, M.D.

THE speech of patients with nervous diseases is registered on a revolving drum. The records are analysed and measured under a microscope. The following law has been established: Each nervous disease that affects the speech mechanism gives a curve of speech that is pathognomonic for that disease. No two diseases give the same speech curves unless they involve exactly the same nervous mechanism. No disease changes its speech curve except as it progresses or regresses. This is the fundamental law of speech neurology.

Inscriptions of idiopathic epileptics at all times reveal an abnormality that has

been found in no other disease and in no normal condition. This is a stiffness of the speech melody without monotony. The peculiarity can be detected otherwise only by the trained ear. It cannot be imitated. Many cases diagnosed as epileptic have been found to be hysterical, and vice versa. Study of the character of the inscriptions leads to the conclusion that idiopathic epilepsy is a congenital form of character, the fundamental feature of which is a diminished adaptability to the whole environment. The researches do not furnish information as to whether the defect is due to abnormality in the psyche, the nervous mechanism, metabolism or the hormone balance. The peculiar stiffness of the epileptic speech melody is so marked that the ear can be trained to detect it. Fifteen years of experience have shown that this stiffness of melody without monotony is the most reliable—perhaps the only reliable—diagnostic sign of the disease.

The trained ear can detect another characteristic always present in the epileptic voice—namely a peculiar hollowness of tone. It is not yet known just what produces this, whether it is special to epilepsy or what it means. Observations with the laryngo-stroboscope may solve the problem.

General paralysis registers in its very earliest stages—even when no speech defect can be detected by the ear and when the diagnosis is doubtful—a specific speech defect known as asaphia. In normal speech the same sounds are repeated not exactly alike but with the degree of precision learned and maintained as proper by the individual. For example, the intervals during which the lips are closed in repeating *pa-pa-pa* do not vary normally from their average by more than 5 per cent. to 15 per cent. In paralytics they vary from 25 per cent. upward to 100 per cent., 200 per cent., and more, without limit, as the disease progresses. During treatment by malaria the figure gradually sinks till, if the cure is complete, it becomes normal again. This percentage of asaphia varies inversely with the person's degree of development of the sense of conformity, or the social sense. In a paralytic it measures his loss of the social sense.

In the inscriptions of disseminated sclerosis there always appear (except in the few flaccid cases) two or three irregular waves in vowel records. Such waves have never been found in any other disease. They are the result of crico-thyroid and thyro-arytenoid ataxia. The phenomenon is so minute in early cases that nothing can be detected by the ear. The diagnosis, however, is made automatically the moment the inscription is examined with a magnifier. The quite ordinary mistake of diagnosing this disease in its early stages as hysteria is instantly corrected by a speech inscription.

The speech abnormalities have been found to result from a disturbed interaction of three biological factors: (1) An impulse to speak as the person has been taught, (2) a force that hinders the result, and (3) an effort to correct the defect. For example, the impulse to precision in speech, eusaphia, is opposed by antisaphia, with an attempt at correction by anasaphia. Asaphia beyond 15 per cent. in the case described above is due to weakness of the first $\frac{\text{or}}{\text{and}}$ the third or increased strength of the second. Every other speech phenomenon is the result of the same three factors. Ataxia is the result of the increased antitaxia or weakened eutaxia $\frac{\text{or}}{\text{and}}$ anataxia. The hypertonic speech of spastic conditions is the result of pathologically increased tonus (hypertonia) or weakened eutonia $\frac{\text{or}}{\text{and}}$ anatonia. It is thus possible as the result of an analysis to express a speech condition as an equation. For example, all the speech signs of disseminated sclerosis are the result of eutaxia + antitaxia + anataxia + eutonia + hypertonia + anatonia. This formula, moreover, sums up the entire symptomatology of disseminated sclerosis and may be regarded as the equation of the disease. With such precise analyses available it is quite out of place to use such misleading and largely erroneous terms as "scanning speech," "staccato speech," &c.

Case of Syringomyelia (Morvan's Type).

By LEWIS YEALLAND, M.D.

G. S., BARMAN, aged 49. There is nothing significant in his personal or his family history. Thirteen years ago, painless whitlows occurred successively, over a period of two years, in the little, ring, index fingers and thumb of the right hand. Two months ago he was admitted complaining of dragging and stiffness of the lower limbs and of "always getting sores on the right hand," which were painless but difficult to heal. On examination he presents a spastic paraplegia, not severe enough to prevent his walking without support; scoliosis; and enlarged, scarred, deformed and discoloured right hand. There are lateral nystagmoid movements and weakness of the right side of the face. There is slight wasting of the dorsal interossei of both sides, of the right upper limb and more severe wasting of the right shoulder girdle. Sensations to pin-prick, heat and cold are lost, on the right side, over the face, neck, upper limb and thorax as far down as the sixth rib. Sensation to cotton wool is retained over this area. The reflexes are pyramidal in type, including double extensor responses. The sphincters are unaffected. The cerebro-spinal fluid contains a total protein of 0·08 per cent., and no increase in cells.

Case for Diagnosis.

Shown by MACDONALD CRITCHLEY, M.D. (for Dr. F. M. R. WALSHE).

G. A., AGED 16, male, admitted for weakness of legs.

Patient has never been able to run or walk like other boys; finds difficulty in going up and downstairs.

Family History.—Nil ad rem.

Neurological Examination.—Internal strabismus, right eye. Nystagmus on lateral ocular deviation—fine and rapid to the left, slower and coarser to the right. Motor power is weak in upper extremities, grips feeble. Muscular development well marked, the deltoids in particular having a peculiar nobbly or knotted appearance. Well marked synkinesia between the right and left hands. Muscular development poor in legs; double pes cavus with hyperextension of great toe. Plantar stimulation frequently evokes an extensor response, at other times flexor. *Gait.*—Awkward and shambling; definite genu valgum with turning-in of toes.

Patient's mental development is well below the average.

A Case of Post-encephalitic Respiratory Disorder.

Shown by MACDONALD CRITCHLEY, M.D. (for Dr. ALDREN TURNER).

H. B., AGED 16, male pupil-teacher, was admitted to the National Hospital for peculiar, frequent attacks of breath-holding.

Patient had an attack of acute epidemic encephalitis in April, 1924; recovery was never complete, a definite inversion of the sleep rhythm having persisted since this illness. In August, 1924, patient developed a persistent short hacking cough without expectoration. Three weeks later, he began to fall down in what were thought to be faints. These turns recurred at very frequent intervals and were present on admission.

Present state.—Patient's turns consist of recurring spells of inspiratory apnœa going into cyanosis and mental confusion, followed by forced expiration with a short period of apnœa. Breath then returns in deep, noisy gasps which persist throughout

the inter-paroxysmal periods. During the period of apnœa the radial pulse becomes obliterated and the heart sounds are inaudible; a varying degree of mental confusion follows and there may even be a momentary loss of consciousness. The breath-holding is only partly under control, the patient being able voluntarily to inhibit the attacks for a few minutes only. The attacks cease during sleep, and become less marked under hyoscine injections or when the attention is distracted.

Neurological Examination.—(1) An inconstant, horizontal, rapid nystagmus on lateral ocular deviation, which is fairly well sustained. (2) Convergence is ill sustained when the fixation point is nearer than four inches to the patient. (3) Marked euphoria with some carelessness in habits and defective sphincter control. (4) Inversion of the sleep rhythm.

Case of Chorea with no Improvement after Eighteen Months' Treatment.

By W. R. Reynell, M.D.

E. A., aged 47, machinist; choreiform movements of face and limbs, especially right side, since December, 1922. Chorea at age of 12, lasted one year; relapse at age of 18, slight symptoms; second relapse at age of 21, recovered in four weeks; third relapse six months later, from which he has not recovered. Has been six months in hospital during last two years. Sister of patient suffered from chorea and died at age of 11 from heart disease. Father died of phthisis and heart disease.

On examination, weakness of right side, exaggerated tendon reflexes, slight irregular pyrexia at intervals; slight enlargement of left heart, pre-systolic murmur and thrill.

No improvement has taken place during the last two years in spite of treatment by rest, salicylates, arsenic, chloretone, hyoscine, bromides, &c.

Case of Facial Hemiatrophy.

Shown by Henry J. Macbride, M.B. (for Dr. S. A. Kinnier Wilson).

A. D., aged 20. Facial hemiatrophy began at the age of 4, with a "whitish spot" on the right forehead. Fits commenced at the age of 5, and during them she was convulsed generally, and unconscious; and when they ceased there was weakness down the left side. She has had other attacks without loss of consciousness; these were accompanied by headache and pain down the left side of the body, with drawing up of the left side of the mouth, left arm and leg.

Reflexes normal and equal right and left, except the abdominals, which were weaker on the left side. No objective sensory disturbance made out.

Case of Paraplegia in Scoliosis.

By James Collier, M.D.

P. C., female, aged 16. Between the age of 3 and 4 her back commenced to become curved. This curvature gradually became worse, but with the aid of a supporting jacket she has been able to walk comparatively well. Eighteen months ago her legs began to drag, the right preceding the left. Ten months later flexor spasms commenced. Six months ago bladder control became defective. No pain nor tight feeling have occurred.

She exhibits very marked scoliosis with severe rotation of the bodies of the mid-dorsal vertebræ. The abdominal muscles are weak; the legs are spastic with brisk knee and ankle-jerks and extensor plantar responses, upper abdominals just obtainable. Diminution to pin-prick up to seventh and eighth dorsal segments; vibration sense lowered in legs. No tenderness of vertebræ.

X-rays show no evidence of abscess formation nor active disease of the vertebral bodies; marked rotation well demonstrated.

Dr. COLLIER said that there seemed to be little doubt that in this case the scoliosis had resulted from poliomyelitis of the thoracic cord, of which the remarkable weakness of the abdominal muscles was a remaining sign. He submitted that the recent paraplegia was the result of pressure upon the spinal cord from the rotation and deformity of the vertebræ. He (Dr. Collier) had seen four other cases in which severe scoliosis resulting from poliomyelitis had been followed years later by paraplegia from a local lesion of the spinal cord. In the present case the cerebro-spinal fluid showed Froin's syndrome, and he suggested that, if this patient did not soon show signs of improvement with rest and extension of the spine, decompression at the site of the lesion was indicated.

Case of Muscular Dystrophy with Pyramidal Signs.

By REDVERS IRONSIDE, M.B. (for Dr. GEORGE RIDDOCH).

A. L., FEMALE, needlewoman, aged 43. Onset at age of 33 years; difficulty in climbing hills and going upstairs. She has occasionally fallen in the street: unable to pick herself up without help. After over-exertion patient complains of pain in the lumbar and gluteal muscles. She has suffered from "gastritis" since 1910. Patient is an only child; no paternal or maternal relations have suffered from weakness of the limbs or paralysis.

The optic discs, pupils, and cranial nerves are normal. Slight difficulty in blowing out the cheeks. No sensory defect. There is wasting of the pectorals, supra- and infra-spinati, and trapezii, and the latissimus dorsi contracts feebly on the two sides. The erector spinæ is flattened on the right side. There is wasting of both lower limbs, more in the right than in the left leg. The right vastus extensor is hypertrophied, both gluteal groups are wasted and the hamstrings are wasted and flattened on the left side. Left hamstring-jerk absent. Both plantar responses extensor, upper abdominal reflexes elicited. The cerebro-spinal fluid is normal (protein 0·015 per cent.) and the Wassermann reaction is negative. All muscles of shoulder-girdles, back, buttocks and thighs show partial R.D.

Myopathy of Unusual Distribution.

By REDVERS IRONSIDE, M.B. (for Dr. GRAINGER STEWART).

G. H., FEMALE, aged 27, one of twins. The twin brother is unaffected, as are also three older brothers and two older sisters. The patient at school was never quite able to keep up with the other children in running-games, but not until three years ago was real difficulty noticed in walking and rising from the sitting position. If the patient walks far, she has dull aching sacral pains which do not shoot down the lower limbs. The weakness was felt in the left leg two years before it was apparent in the right.

Points in physical examination: (1) Transverse "quadrangular" smile, and curious soft voice; (2) relatively slight muscular wasting, but great weakness in the ileo-psoas, hamstrings, adductors, gastrocnemii (not soleus) and anterior tibial group; (3) absent ankle-jerks; (4) electrical reactions in gastrocnemii approximating

R.D. The gait is waddling. There is no hypertrophy of muscles; no sensory defect. The cerebro-spinal fluid is normal and the Wassermann reaction negative.

Pathological Specimens.

Shown by W. E. Carnegie Dickson, M.D.

(I) Myxo-chondroma of Base of Skull.

Florence M., aged 32 (a case of Dr. Macnamara's), had complained of the following symptoms: Vertical headache, occasional vomiting, defective vision, difficulty with speech, numbness of right side of face. The onset of the condition had occurred about two years previously and the symptoms were steadily becoming worse.

Physical examination showed: Speech slow and slurred: pupils—right greater than left, the right being irregular and reacting neither to light nor for accommodation: lateral nystagmus, with diminished upward movement and convergence of right eye. Both optic discs were blurred at the nasal sides and the veins swollen; anæsthesia of right side of face involving all divisions of fifth nerve: weakness of right masseter and temporal muscles; right side of palate inactive and right vocal cord immobile. Tongue deviated to right on protrusion, with right-sided hemiatrophy. Head held tilted upwards and turned towards right side, while the trunk tended to sway towards the right. Right arm weaker than left, with some inco-ordination. She walked fairly well but with a tendency to sway towards the right. Knee-jerks sluggish and ankle-jerks not elicited. Plantar reflexes flexor, and abdominal reflexes absent. A mass was seen on the right side behind the palate and some glandular swellings were palpable on the right side of the neck. Death occurred in November, 1924.

Post-mortem.—In the base of the skull, and projecting into its cavity, was a large, irregularly rounded, or pear-shaped mass, $2\frac{1}{2}$ in. in transverse, and 2 to $2\frac{1}{2}$ in. in antero-posterior, diameter. It was situated mainly in the region of the sphenoid, expanding and replacing the bone, so that the pituitary gland was displaced and appeared partially embedded on the surface of the tumour, as if "floating" upon it. The prominent growth of the tumour had occurred mainly towards the right, and it extended more than half-way across the middle fossa, and also backwards and to the right, between the foramen magnum and the petrous temporal bone, the apex of which was surrounded by and embedded in the tumour. It had thus encroached extensively upon the cranial cavity, pushing aside and compressing, especially, the right temporo-sphenoidal lobe, the right side of the pons and the middle peduncle, and under surface of the cerebellum, the upper part of the medulla being also considerably distorted. The tumour had spread extensively in the substance of the bones of the base of the skull, especially in the body of the sphenoid, the basi-sphenoid and basi-occipital, and neighbouring parts. It had also spread downwards into and among the tissues on the right side of the neck, being found as a large pseudo-cystic tumour below the right mastoid, and behind the ascending ramus of the lower jaw, bulging into the naso-pharynx, and pushing the right tonsil and soft palate downwards and to the left. The contents of this were semi-translucent, very pale, slightly bluish milk-white material of about the consistence of table-jelly.

Microscopically.—The structure appears to be that of an ordinary *myxo-chondroma*, and not that of a chordoma, the mucoid degeneration affecting both cartilage-cells and the fibrous-tissue stroma surrounding the islets of degenerated hyaline cartilage. The large vacuolated cells characteristic of chordoma are not present.

(II) Hæmangioma of the Mid-brain.

Hetty P., aged 25, had complained of unsteadiness, "dropping things and difficulty in speaking" for two years. She was admitted under the care of Dr. Harry Campbell, on August 7, 1924, exhibiting slurred and indistinct speech, expressionless face, some limitation in outward movement of both eyes, slight left-sided facial weakness, and occasional antero-posterior tremor of head. Pupils, optic discs and other cranial nerves were normal. There was a constant tremor of the left hand, with dysmetria. The gait was slightly ataxic, but the reflexes were not abnormal. Death occurred on August 31, 1924.

Post-mortem.—The brain, on mesial vertical section, showed, in the region of the mid-brain, just below the lamina and corpora quadrigemina, a dark red *vascular tumour*, $1\frac{3}{4}$ in. in antero-posterior, and about 1 in. or a little more in vertical, diameter, situated in and expanding the crura cerebri and region around the aqueduct of Sylvius, into the upper part of which a horse-hair could be passed for about $\frac{1}{2}$ in., the canal being embedded in the tumour. Anteriorly, the mass extended to within $\frac{1}{4}$ in. of the posterior wall of the third ventricle; whilst, posteriorly, it distorted the upper part of the fourth ventricle, and projected towards the cerebellum, and pushed it backwards. Laterally, it reached the outer surface of the right crus, but, on the left side, it did not pass so far beyond the middle line.

The tumour, to the naked eye, consisted of closely aggregated, rounded or irregular vascular spaces, filled with blood, the largest about $\frac{1}{8}$ in. in diameter. Their arrangement was more or less compact, but isolated spaces could be seen around the periphery in the cerebral substance. Some of the spaces show the lighter tint of thrombosis, varying from brick-red to pale white of decolorized thrombus. There was moderate distension of the third and both lateral ventricles, and marked distortion of the anterior part of the roof of the fourth ventricle.

Microscopically.—The structure was that of a *cavernous angioma*, lined by much-flattened cells, presumably endothelial, the framework being composed for the most part of fully-formed dense fibrous tissue. The smallest spaces at the margins had the appearance of dilated capillaries, or capillary-venous spaces, composed of a single layer of flattened endothelial cells, with little or no supporting tissue around them. Various stages of thrombosis, recent to old-standing, were present in many of the spaces, and there were also considerable aggregations of polymorphs in many of them. Hæmorrhage had occurred from some of the spaces, with the presence of numerous phagocytic cells containing blood-pigment.

(III) Neuro-fibroma of Auditory Nerve.

Florence G., aged 37, was shown clinically before this Section in December, 1923, by Dr. Worster-Drought, as a case of early tumour of the acoustic nerve.[1] Subsequently, the symptoms and signs gradually increased in intensity, and papill-œdema developed in May, 1924. An attempt was made to remove the tumour by operation performed in two stages, but the patient died shortly after the second operation.

Post-mortem.—On opening the skull and removing the brain, a tumour about the size of a large walnut was found, lying at the right cerebello-pontine angle, causing considerable flattening and some indentation of the pons, and adhering externally to the petrous temporal bone, around the opening of the internal auditory meatus. Posteriorly, the tumour was broken up and mixed with blood-clot in the operation-area. The fifth cranial nerve was stretched antero-posteriorly over the upper aspect of the tumour. The trochlear nerve lay internally, but did not appear to be compressed; whilst the sixth nerve lay internally below, in the angle between the tumour and the pons, and has probably also escaped compression. The upper part of the medulla was slightly pushed towards the left.

[1] *Proceedings*, 1923-24, xvii (Sect. Neurol.), p. 28.

Microscopically.—Sections from the posterior part of the tumour, removed at operation, showed it to be a *neuro-fibroma*, composed of irregular whorls and bundles of fibroblasts in some parts, fully formed fibrous tissue in others, the vessels varying from a single layer of endothelium with a narrow zone of supporting connective tissue, up to considerably thickened small arterioles and capillaries, many of the vessels showing various stages of hyaline degeneration. Around some of the vessels, there was a moderate amount of small-celled infiltration, and, in their lumina, some excess of polymorphs.

Section of Neurology.

President—Dr. E. Farquhar Buzzard.

The Syphilitic Forms of Progressive Muscular Atrophy.

By J. P. Martin, M.D.

Introduction.

Apart from the myopathic types, syringo-myelia and the infantile form of Werdnig and Hoffman, most of the cases of progressive muscular wasting that we see to-day fall into a large group characterized clinically by certain very peculiar features, and in particular by the phenomenon which Gowers called "tonic atrophy." I do not propose to discuss this group to-night, but in order to differentiate other cases from it, it is necessary that I should emphasize some of its characters. The muscles, though wasting rapidly and though very flabby to palpation, perhaps absolutely pendulous, nevertheless respond with great briskness to percussion of their tendons. It might be expected that with the diminution of the muscle the tendon reflex would gradually be abolished, but this is not so; even when the muscle has wasted so as to be no longer palpable, a brisk tendon-jerk is still obtainable. This fact, which I note from my own observation, was described by Gowers, and has more recently been mentioned again in the writings of Raymond and Cestan, and of Collier. Fibrillation and excitability of the muscles to direct percussion always occur. To electrical tests these muscles usually show only a moderate diminution of excitability and some tendency to polar change, but never, I think, a complete reaction of degeneration. In a large proportion of cases, some bulbar palsy occurs, and this may be the first sign of the disease. Spasticity is very constant, and though in rare cases an extensor plantar response may not for a time be obtainable, some signs of pyramidal involvement probably always appear before the disease is far advanced; the degree of spasticity is, however, not usually severe. The sphincters are not affected, except perhaps in the latest stages of the disease. There is no objective sensory loss, and subjective sensory phenomena are uncommon, and when they do occur are slight.

At the pathological examination we find degeneration of the anterior horn cells, of the pyramidal tracts in their whole length, and frequently of the spino-cerebellar tracts; the posterior columns remain conspicuously intact. The disease is approximately, though not absolutely, a systemic degeneration.

As regards the ætiology of these cases, we can only call them idiopathic; but the point I wish to make is that they are not syphilitic. In a series of twenty cases, I have not once encountered a positive Wassermann reaction either in the blood or in the cerebro-spinal fluid; cells were found in the cerebro-spinal fluid only in three out of twelve cases in which they were looked for. In one case there was one cell per cubic millimetre, in another there were three and in another four, giving an average of decidedly less than one cell per cubic millimetre for the twelve cases.

But from time to time we meet with cases of progressive muscular atrophy that are associated with syphilis. The association is too frequent to be merely fortuitous, and, in any case, there are strong indications from the pathology that the atrophy is caused by the syphilis. In studying this group my main object has been to discover any clinical features by which the syphilitic cases might be recognized.

History.

The first suggestion in the literature of the subject of the causal relation of syphilis to progressive muscular wasting is probably that in the Clinical Lectures of Graves of Dublin, published in 1795. Graves had a patient, an officer, 60 years of age,

suffering from a muscular atrophy which had begun five years before in the right hand and had gradually spread up the forearm to the arm. Graves treated the patient with mercury and the progress of the disease ceased, leaving the physician wondering whether the cause of the disease was really that which the effect of the treatment suggested to his mind. In 1853, a case of progressive muscular atrophy in a syphilitic patient was published by Niepce, and the atrophy was attributed, in his paper, to syphilis. In 1879, Hammond, in reporting another case, suggested that the association of these diseases was not a mere coincidence. Gowers, writing in 1886, recognized syphilis as one of the causes of chronic muscular atrophy[1]:—

"The disease," he says, "sometimes succeeds syphilis and no other cause may be traceable. As with other degenerative diseases, an interval of years elapses between the primary disease and its nervous sequel . . . ". "That syphilis has some share in the causation of these cases is probable from its relation to other diseases, such as tabes."

Many cases indeed were recorded in which syphilis was mentioned in the history (as, for example, one of Aran's cases), but no significance was attached to its presence.

The beginning of more exact knowledge dates from the publication of a paper by Raymond in 1893. Raymond reported four cases of progressive muscular wasting in syphilitic subjects, and in one case he had been able to make a microscopic examination of the spinal cord. He saw that it was not merely a matter of atrophy of the anterior horn cells, but that there was a diffuse meningo-myelitis, with much arteritis, and he supposed that the degeneration of the anterior horn cells was secondary to vascular disease.

Ten years later, Léri, in a paper read before a congress of neurologists and alienists at Brussels, reported six cases, with two autopsies. His findings corresponded closely with those of Raymond—what he called a " diffuse vascular meningo-myelitis " was present in both the spinal cords he had examined. He established the fact that there was no disease that could be called " systemic," but that all the elements of the cord were involved, and he showed that all the findings were consistent with syphilis being the sole cause.

But the importance of Léri's paper goes much further than that, because after a careful examination of the literature he claimed that a particular clinical type was nearly always syphilitic; he thus stimulated interest in the subject, especially in his own country; since then sixty cases have been published, twenty-two of them complete with microscopical examination of the spinal cords, and most of them have been published in France. In this country we have had important papers by Dr. Kinnier Wilson, Drs. Tooth and Howell, and Dr. Gordon Holmes, while, under the title of " syphilitic poliomyelitis " cases have been shown before this Section by Dr. Wilfred Harris and Dr. Cecil Wall.

General Clinical Features.

I have encountered six cases of progressive muscular wasting in syphilitic subjects; two other cases which I have studied pathologically I am enabled to include through the kindness of Dr. Aldren Turner; in the available literature there are about 120 cases, of which I have found, so far, more than ninety, so that I have analysed for this paper a group of 100 cases.

An overwhelming majority of them show (in contrast to the idiopathic cases I have mentioned) the " atonic " type of atrophy—atrophy with loss of reflexes in the wasting muscles; these I have divided into two groups, (I) those in which the clinical picture was simply a progressive muscular wasting, and (II) those in which there was in addition one of the recognized syndromes of nervous syphilis—tabes or general paralysis of the insane. There remained three cases; one of them from my personal experience, showed the " tonic " type of atrophy but they were distinguished by other features from the idiopathic " tonic " group: these three I have separated as atypical.

[1] " Diseases of the Nervous System," 1886, i, p. 358.

Type I.—Uncomplicated progressive muscular wasting of the "atonic" type: sixty cases.

Type II.—Progressive muscular wasting of the "atonic" type associated with tabes or general paralysis of the insane: thirty-seven cases.

Atypical Cases.—Progressive muscular wasting of the "tonic" type, distinguished from the idiopathic group by Argyll-Robertson pupils, sensory loss or severe sphincter trouble: three cases.

Type I: Sixty Cases.

The chief diagnostic interest and difficulty attach to those cases in which few or none of the generally accepted clinical signs of nervous syphilis are present and this is the type that I shall chiefly consider.

The evidences of syphilis that I have accepted in cases of this group are: (1) A direct history of infection; (2) a positive Wassermann reaction either in the blood or cerebro-spinal fluid; (3) in a few cases syphilitic post-mortem findings.

Period after Infection.—The average period between the syphilitic infection and the onset of wasting, in those cases in which it could be ascertained, was seventeen years, with extremes of four and thirty-seven years. A few cases have been recorded in the subjects of hereditary syphilis.

Age and Sex.—The great majority of the patients are males—fifty males and ten females in the sixty cases of this group. The ages vary from 32 to 74, with an average of 48; nearly two-thirds of the cases (62 per cent.) begin between 34 and 50.

Typical Cases.

(*a*) J. S., draper, aged 46. Under the care of Dr. Aldren Turner in the National Hospital, 1923. Had gonorrhœa when 22, not aware of syphilis. When 30 thenar eminence of left hand wasted, three months later power of extension of the left fingers and wrist was lost and very soon similar wasting and weakness occurred on the right side. When examined at the age of 32 he had complete reaction of degeneration in the thenar muscles and the extensors in the forearm; the arm-jerks were still present; there was no spasticity of the lower limbs and no sphincter trouble. Four or five years later bladder troubles came on. When he was 40 his legs began to feel stiff and two years afterwards he had to give up business on account of difficulty in walking; by that time his hands were so feeble that he could not shave himself; before he was 45 he was totally unable to walk. Examination in the next year showed:—Great spasticity of the lower limbs with strong flexion spasms; plantar reflexes extensor, clonus at knees and ankles; upper limbs extremely wasted and quite powerless; the left biceps-jerk still just obtainable, all the other arm-jerks absent; difficulty with both sphincters; no sensory loss except diminution of vibration sense in the lower limbs; pupils equal and reacting normally to light; no bulbar symptoms. Wassermann reaction positive in blood and cerebro-spinal fluid. The patient died in June, 1923, aged 46; examination of the central nervous system revealed the typical changes to be described later.

(*b*) W. H., aged 54, wheelwright. Came to Maida Vale Hospital on September 15, 1924, complaining of severe pain in the left shoulder and upper arm of two months' duration. He suffers from long-standing deafness of obstructive origin. Examination showed extensive corneal opacities; the pupils were unequal but reacted well to light and accommodation; there was laryngeal stridor suggestive of abductor paralysis of the vocal cords but no such palsy was present. All movements at the left shoulder were weak and the arm could not be raised voluntarily to the horizontal; there was some wasting and brisk fibrillation in the left deltoid, biceps, triceps and pectoralis major; extensors of wrist slightly but definitely weak; left hand normal. All the other limbs were normal. Tendon reflexes lost in the left arm, elsewhere present and normal; plantar reflexes flexor; no sphincter trouble. Wassermann reaction positive in blood; cerebro-spinal fluid contained 42 lymphocytes per cubic millimetre.

(*c*) F. D., aged 58, carriage-cleaner. Seen at Maida Vale Hospital. Was healthy till summer 1921, then began to feel weakness of both legs: the legs got rapidly thinner and weaker and within twelve months the patient was unable to walk. When examined at the hospital in June, 1922, he had great wasting of all the muscles below the knee in both lower limbs; there was double foot-drop and practically no power of movement at either ankle: the ankle-jerks were absent, the plantar reflexes unobtainable; elsewhere the reflexes were

normal. Vibration sense was lost in the legs, but there was no other sensory loss. Pupils normal. Wassermann reaction negative in blood but positive in cerebro-spinal fluid. The disease has made no progress since antisyphilitic treatment was given.

These three cases illustrate well the main varieties of the clinical picture of the uncomplicated progressive muscular wasting of syphilitic subjects.

Mode of Onset.—In the majority of cases (78 per cent.) the first symptom complained of was weakness, but as some wasting appears to have been discovered at the first examination in all these cases, there is no reason to believe that any weakness preceded the atrophic paralysis.

In 22 per cent. of cases (thirteen out of sixty), the first symptom was pain. This was in most cases a constant, severe pain of several weeks' duration, often referred to the shoulder or half of the back of the neck on the side on which the wasting later appeared. In two cases lightning pains in one arm preceded wasting in that limb. I have not found any case of Type I which began definitely with spasticity in the lower limbs. I should expect, however, that this would sometimes occur, for it seems probable on pathological grounds that in some cases of syphilitic myelitis of the spastic, Erb type, the involvement of the lateral columns would be followed by degeneration of the anterior horn cells.

Wasting.—The wasting nearly always (93 per cent. of cases) began in one of three situations—the small muscles of the hand, the shoulder muscles or the muscles on the outer side of the leg.

(*a*) In three-fifths of the cases it began in the small muscles of the hand. It was usually noticed first in the thenar eminence, and the powers of adduction and of opposition of the thumb were lost; at the same time the interosseous spaces became hollow.

Within a few weeks, in many cases, weakness was felt, not as one might expect in the flexor, but, more often than not, in the extensor group in the forearm. A typical case at this stage shows fibrillation in the extensors and usually the tendon reflex of the supinator longus is already lost; in a few cases the supinator longus muscle has been relatively spared and has for a time retained its tendon-jerk. As the extensors waste, the wrist drops and the fingers become flexed at their intrinsic joints. The flexors then become affected, but they do not often lose their power with the same rapidity as the extensors. Subsequently fibrillation and weakness may begin in the biceps and triceps, but most characteristically these muscles are spared for a time and the disease jumps from the forearm to the deltoid, and it is only after the deltoid, and perhaps the scapular and pectoral muscles, are considerably wasted, that the biceps and triceps finally atrophy. These "jumps," though they do not, of course, occur in all cases, are very characteristic of the disease, and they have an anatomical counterpart.

Both upper limbs may be affected simultaneously and the wasting then progresses *pari passu*. But one limb may be entirely withered and its fellow remain intact.

(*b*) In the patient, W. H., whom you have seen this evening, the wasting began in the shoulder muscles. It first appeared in this situation in 22 per cent. of cases. The deltoid, the supra- and infraspinati and the upper part of the pectoralis major, all supplied from the same segment, usually become affected in quick succession. Wasting of the rhomboid and trapezius and of the muscles of the back of the neck may follow next, or the disease may spread down to the biceps and triceps, or with one of its "jumps" it may strike the extensors in the forearm. The flexors in the forearm and the small muscles of the hand may then escape for a considerable time.

In very many of these cases in which the wasting begins in the upper limbs, there is a direct spread to the muscles of the back and of the chest. The muscles at the back of the neck are often affected, and the patient becomes unable to hold up his head.

(*c*) In six cases out of sixty, one of which you have seen this evening, the wasting appears to have begun in the peronei or anterior tibial muscles. The intrinsic muscles of the foot are probably affected about the same time. The calf wastes next, and after that the disease spreads upwards over the thigh and hip. Sometimes wasting appears soon afterwards in the small hand muscles or in the deltoid. Both legs are usually affected, but I have seen one case in which the right leg was intact, although the left calf, thigh and hip were all involved. Cases beginning in this way may present a great resemblance to cases of the Charcot-Marie-Tooth type of atrophy. In the patient you have seen to-night I put against the latter diagnosis the following points: absence of family history of a similar disease (the patient has two grown-up sons, both healthy); the age of the patient—58—at the time of onset; the rapid progress for twelve months followed by arrest; in favour of a syphilitic origin were the presence of a positive Wassermann reaction in the cerebro-spinal fluid and the arrest or long intermission of the disease after treatment. In this connexion it is interesting to note that at least two cases of supposed Charcot-Marie-Tooth disease have been reported in which the patients had Argyll-Robertson pupils.

Further Course.—Wherever the wasting begins it may at any time become arrested. Some cases cease to progress when one arm has wasted, others may remain stationary for years with both arms rendered completely helpless, or as you have seen to-night, with both legs considerably wasted from the knees downward. The further progress shows considerable variation. In the first case I have described to you the patient became extremely spastic in his lower limbs, and developed severe sphincter trouble. Thirty-five per cent. of the cases of Type I showed some signs of spasticity. In Charcot's nomenclature they would have been called cases of amyotrophic lateral sclerosis beginning as progressive muscular atrophy. The spasticity which develops in these cases may in my experience be very severe, much more so than that usually seen in the idiopathic cases; when spasticity develops, wasting of the lower limbs rarely follows.

In 48 per cent. of cases, the knee-jerks were weak or abolished. Some of these patients in a later stage suffer from lightning pains, show Argyll-Robertson pupils, and develop the sensory loss met with in tabes. In others no gross tabetic manifestations develop. In either event the wasting may become generalized—wasting of the atonic type in all limbs. This occurred in 23 of the 100 cases which I am discussing. Sometimes as the wasting passes downwards from the shoulder girdle, death is brought about by paralysis of the muscles of respiration.

A point to which I would call attention is that death is rarely due to bulbar involvement, as it so often is in the idiopathic cases. Bulbar palsies are in fact strangely uncommon in these cases; in only two cases of the sixty of this type was the bulb affected.

Reflexes.—The tendon-jerks of the wasting muscles usually disappear early and commonly all those of the affected limb. In a number of cases they are retained for a considerable time, but they steadily decrease. The tendon reflexes of unaffected muscles are usually normal unless there are signs of spasticity or, on the other hand, of tabes. The superficial reflexes vary only with the development of spasticity.

Fibrillation.—Fibrillation would appear to be much less constant in these syphilitic (atonic) cases than in the idiopathic (tonic). Out of fifty-seven cases in which it was looked for, it was observed only in forty-one.

Sensation.—Except in the initial stage, to which I have already referred, subjective sensory phenomena are not common. The cases in which lightning pains occurred I have classed, almost without exception, as Type II.

The only objective loss I have observed has been a loss of vibration sense in the lower limbs. The responses to light touch, pain and thermal stimuli have in my experience always been normal. What is more surprising is that the muscle and joint sense is intact.

Pupils.—In all but one of the cases in my own experience the pupils have been equal and have reacted normally; in one case they were unequal. In the cases of Type I as a whole, the state of the pupils is recorded in thirty-eight; in fourteen cases they were normal; in two others they were unequal, but reacted well to light, in ten others they reacted badly and in twelve cases they showed the Argyll-Robertson phenomenon. Thus in at least 37 per cent. of cases the pupils showed no abnormality, in 5 per cent. they were merely unequal and in 58 per cent. the reaction to light was impaired or lost.

Sphincters.—The state of the sphincters is mentioned in twenty-nine cases out of the sixty: in eighteen they are given as normal, and probably we should add to this number most of the cases in which their condition is not alluded to. In eleven (18·3 per cent.) there were urinary difficulties, always in the direction of incontinence. Paralysis of the anal sphincter is uncommon.

Summary.—To sum up the clinical features, the atrophy in the syphilitic cases is of the "atonic" type, it begins with great constancy in one of three situations, it is frequently unilateral, fibrillation is less constant than in the idiopathic cases, bulbar symptoms are relatively uncommon, severe pain may occur at the onset of the wasting, the vibration sense may be impaired, bladder control may be impaired. Such characters, since pupil changes are so often absent, should of themselves suggest syphilis and lead to the examination of the blood and cerebro-spinal fluid.

Blood and Cerebro-spinal Fluid.—Of twenty-two cases of Type I in which the Wassermann test was done, the reaction in the blood was positive in fifteen and negative in seven. Of these seven cases five had a positive reaction in the cerebro-spinal fluid, in a sixth case the cerebro-spinal fluid reaction was not examined and in one case the reaction was negative both in blood and cerebro-spinal fluid.

In two cases in which the blood gave a positive result the cerebro-spinal fluid gave a negative reaction, but in one of these cases the reactions were done at different times and treatment had been administered for several weeks before the cerebro-spinal fluid was tested; there is only one case in which the cerebro-spinal fluid reaction was negative before treatment.

Of fifteen cases of Type I in which a cell-count was performed in the cerebro-spinal fluid it is stated in seven cases that lymphocytes were "abundant" or "numerous": in the remaining eight cases the numbers were 42, 19, 6, 75, 150, 2, 49, 6 per cubic millimetre, giving an average of forty-four. No other cells than lymphocytes have been found in any of the cases I have seen, nor are other cells mentioned in the reported cases.

Type II: Thirty-seven Cases.

Among 100 cases there were twenty-seven in which a progressive muscular atrophy was associated with tabes and ten in which it was associated with general paralysis of the insane.

I have found no reason, clinical or pathological, to suppose that the muscular atrophy in these cases has an origin in any way different from that in the other cases of which I have spoken, and the fact that the number of these cases, in which nervous syphilis is certainly present, is so great (37 per cent. of all cases) is a considerable argument in favour of the syphilitic origin of other cases.

Certain points in this group require mention.

We are accustomed to see in tabes a certain thinning of the musculature as a whole. This is quite different from the massive wasting of which I am speaking, and is hardly to be included in the heading of my paper.

Possibly as part of this generalized wasting there is noticed in some cases of tabes a flattening of the thenar and hypothenar eminences; Marie and Foix devoted a paper to showing that this atrophy was not progressive, a fact which must be borne in mind before diagnosing early progressive muscular atrophy in a tabetic subject.

Again, there occur in tabes paralyses of the ocular muscles, of the jaw muscles and sometimes of the pharynx or tongue. These are not usually progressive and I have not accepted such palsies as indications of a commencing progressive muscular atrophy.

With regard to the wasting in the cases of true progressive muscular atrophy in tabetics, the same sites are first attacked and the progress is the same as in the uncomplicated cases. The legs are, however, attacked first with greater frequency.

There were ten cases in my total of 100, in which progressive muscular atrophy was associated with general paralysis of the insane. In most of these the muscular wasting came on before the psychical disturbances; except in one case it began in the hands, shoulders, or the outer side of the legs.

Atypical Cases.

There were three cases, which are perhaps of some theoretical interest, in which the atrophy was of the "tonic" variety; but they are distinguished from the idiopathic "tonic" cases by sensory loss, sphincter troubles, or Argyll-Robertson pupils.

One is a case reported by Rose and Rendu in 1910: the subject was a syphilitic, had Argyll-Robertson pupils, and suffered from paræsthesiæ in the limbs; the wasting began typically in the hands, but the tendon-jerks of the supinator longus and of the triceps were increased, the biceps-jerk is not mentioned. The lower limbs showed signs of spasticity.

Another is a case reported by Oppenheim as long ago as 1885; the patient was a man whose wife had had seven miscarriages, and who himself suffered from lightning pains, and a girdle sensation, and had unequal pupils. At the post-mortem examination changes were found in the posterior columns as well as in the lateral columns and anterior horns.

A third case came under my own observation.

E. D., female, married, aged 64, admitted to St. Mary's Hospital under the care of Dr. Wilfred Harris on July 10, 1924. Her hands, forearms, scapular muscles, and lower limbs showed an advanced degree of wasting. The lower limbs were very spastic, and the arms had exaggerated tendon-jerks. Both sphincters were incontinent. There was a loss to all forms of sensation on the lower limbs, up to about the level of the distribution of the first lumbar segment, but not sharply limited.

The clinical picture here was that of a subacute spastic myelitis together with a progressive muscular wasting.

Such cases must be, I think, very uncommon, but they were referred to as a group in some remarks by Dr. Henry Head at the International Congress of Medicine in London in 1913.

Pathology.

(This section was illustrated by lantern slides.)

I have had the opportunity of studying the pathology of two cases, and from my own findings and the findings of others, I have drawn some general conclusions.

To the naked eye the central nervous system seldom shows any abnormality except some thickening of the meninges and possibly a thinning of the anterior roots.

In microscopic section very striking and diffuse changes are seen in the spinal cord. The anterior horn cells, in the segments corresponding to the atrophied muscles, are extremely wasted, sometimes not a single cell remains in a section where one should normally find fifty or sixty. Those that do remain usually show atrophic changes.

(A photograph was shown of a portion of a section at the level of the eighth cervical segment; the anterior horn on the side photographed showed only four cells, that on the other side none at all. These four cells all showed chromatolysis, especially at the periphery; the nucleus was eccentric and its outline was hazy.)

This is the commonest type of change, but occasionally cells have a swelling of the nucleus, with disappearance of the Nissl granules close to it (as in the *réaction à distance*). Here and there one meets with a clump of relatively healthy cells, side by side with a patch from which all the cells have disappeared. This is an indication, I think, of the patchy way in which the cord is affected, and may possibly be associated with the way in which muscles, apparently in the line of march of the disease, are for a time spared.

Here and there the anterior horns show numerous lymphocytes, sometimes collected together in clumps.

In Weigert-Pal sections nearly all cases show degeneration both in the posterior and in the lateral columns. The microscopic differences in the sections are much less than the clinical differences in the cases would lead one to expect; the amount of posterior column involvement tends to be less in the spastic cases, but in other cases, if there is considerable posterior column degeneration, it is surprising how complete may be the degeneration of the lateral columns without any signs of spasticity having shown themselves.

The lateral column degeneration is not a degeneration of the whole pyramidal tract, right up to the cortex, as it is in the idiopathic cases. The degeneration may be from the level of the greatest anterior horn lesions downwards, and in the medulla no pyramidal degeneration may be seen. A few cases have, however, been met with in which the pyramidal tract was degenerated in its whole length (for example, a case reported by Dr. Holmes in 1913). Very many sections show a degeneration of the fibres lying close to the outer margin, and this gives them what has been called a "syphilitic halo." The fine fibres in the grey matter undergo great destruction, the severity of which contrasts, to some extent, with the relative sparing of these fibres in the idiopathic, "tonic," atrophy. It is with the destruction of these fibres that I associate the abolition of the reflexes of the wasting muscles.

In looking for the underlying cause of the degenerations in the cord, we get most help from examination of the blood-vessels, meninges and ependyma. The small blood-vessels in the cord, and especially in the affected anterior horns, are extremely numerous; all the vessels, both in the cord and in the meninges, show considerable endarteritis and periarteritis, and in most cases infiltration of the perivascular spaces in the cord with lymphocytes. The same lymphocytic infiltration, perhaps greater in degree, is seen in the meninges. The meninges are, moreover, greatly thickened, and the pia is adherent to the cord and at places sends strands of fibrous tissue into the cord. In addition there are outgrowths of glia from the margin of the cord or brain-stem which break into the pia. These outgrowths are even more pronounced under the ependyma, and they are associated with a proliferative ependymitis, similar to that found in general paralysis of the insane. The overgrowth of glia is probably due to a toxic stimulation and so is an indication of the action of a toxin on the intrinsic tissues of the cord. I believe you will agree that everything points to the presence of syphilis and no other disease—the state of the vessels, the thickening of the meninges, and the degeneration of the posterior columns all have that significance. I do not consider that the state of the blood-vessels is sufficient, as many writers have thought, to account for all the destruction of anterior horn cells and of fibre tracts. There must be an infiltration of a syphilitic toxin or of organisms which the anterior horn cells are unable to withstand.

If we desire a pathological name for the disease I think we should accept Léri's term, "amyotrophic syphilitic myelitis."

I wish to express my great indebtedness to Dr. Greenfield for his help in the pathological investigations and for the preparation of the lantern slides.

Section of Neurology.

President—Dr. E. Farquhar Buzzard.

DISCUSSION ON THE ÆTIOLOGY AND TREATMENT OF SUBACUTE COMBINED DEGENERATION OF THE SPINAL CORD.

Pathogenesis and Treatment of Addison's (Pernicious) Anæmia and Subacute Combined Degeneration of the Spinal Cord.

(From a study of sixty-one consecutive cases—thirty-one private and thirty hospital.)

Dr. A. F. Hurst

said he believed that Addison's (so-called pernicious) anæmia (A.'s A.) and subacute combined degeneration (S.C.D.) of the spinal cord were different aspects of the same disease, and were due, as he had first suggested in a paper published in *Brain* in 1922 with Dr. J. R. Bell, of Melbourne, to infection of the intestines with streptococci, which reached the duodenum from the mouth owing to the presence of complete achlorhydria. The signs and symptoms of nervous disease, which occurred sooner or later in about 80 per cent. of cases of Addison's anæmia, and the pathological changes found after death, were indistinguishable from those of subacute combined degeneration of the cord. Although it was at first thought that the anæmia, which developed before death in almost all cases of subacute combined degeneration, was of a secondary character, he (Dr. Hurst), said he agreed with the views expressed by Dr. Collier in his most recent publications on the subject that it was really always Addisonian. The investigations of Dr. Price-Jones had shown that megalocytosis was the one essential feature in the blood-picture of Addison's anæmia; when this was recognized, it was found that even in cases of subacute combined degeneration with no obvious anæmia the earliest stage of the blood disease was already present, megalocytosis having been discovered in several of his (Dr. Hurst's) cases with 80, 90 and even 100 per cent. of hæmoglobin. The distinction between Addison's anæmia and subacute combined degeneration of the cord was thus quite artificial; in the accompanying tables cases were classified according to whether the most prominent symptoms at the time of the first consultation were due to the anæmia or to the nervous disorder.

The chart following these remarks gives a summary of Dr. Hurst's views of the pathogenesis of the two associated diseases, and Table I summarizes the evidence that achlorhydria is always or almost always present. This is generally due to an inborn error of function of the gastric glands, thus explaining the cases in which it was known to have preceded the onset of symptoms of anæmia or nervous disease by a number of years (Table III). Constitutional achylia is also often familial; this explains the frequent occurrence of Addison's anæmia either alone or associated with subacute combined degeneration or with achylia gastrica in other members of the family (Table II). Table IV summarizes the evidence that the intestinal infection is streptococcal.

He (Dr. Hurst) pointed out that the treatment summarized in the chart could not be expected to overcome the infection in all cases, as the diagnosis was often made much too late. But true prophylaxis could be instituted by giving hydrochloric acid and by eradicating oral sepsis in those individuals who are discovered to have achlorhydria when given a test-meal on account of digestive or other symptoms. Early diagnosis would be more common if the true significance of Hunterian glossitis were more often recognized. In such cases and in cases of slight anæmia and of suggestive nervous symptoms, such as paræsthesia of the extremities—even in the absence of abnormal physical signs—the blood should be examined for megalocytosis and a fractional test-meal given to find whether achlorhydria is present. The results of treatment had so far been very promising. The severity of anæmia did not matter, as several patients were now apparently well and hard at work, though two or three years ago they had had only 20 per cent. of hæmoglobin. Unfortunately the case was different with the nervous symptoms; only very little improvement could be hoped for, as the damage to the spinal cord had already been done. But if the disease is recognized early, before any actual incapacity is present, and if it is treated thoroughly, the infection can often be controlled and progress of the nervous symptoms arrested, so that the patient is able to continue his usual occupation.

CHART SUMMARIZING PATHOGENESIS AND TREATMENT.

Pathogenesis.	*Treatment.*
(1) ACHLORHYDRIA. (Table I.)	
(*a*) *Constitutional achylia gastrica*: Family history (Table II); history of achylia preceding anæmia and nervous symptoms (Table III); history of indigestion, especially diarrhœa, preceding other symptoms.	(1) Dil. HCl ℥ij in ℥x water or orangeade, t.d.s., first dose one hour before breakfast. To be continued for the rest of the patient's life, however fit he may become.
(*b*) Achylia, secondary to (i) atrophic gastritis, generally alcoholic; may be associated with cirrhosis (A. F. H.; Roth). (ii) cancer of stomach (A.'s A. (Brandes); S.C.D. (A. F. H.; Garvey and Stern)). (iii) gastrectomy (Hartman; Ellis; ? Moynihan).	
(*c*) Achlorhydria, secondary to (i) gastritis without atrophy (recoverable), generally alcoholic (A. F. H.). (ii) gastro-enterostomy (Campbell and Conybeare).	
(2) ORAL STREPTOCOCCAL INFECTION.	
(*a*) *Pyorrhœa alveolaris.* If edentulous, former dental infection may have led to (*b*), (*c*), (*d*), or latent duodenal infection.	(2) Eradication of oral and pharyngeal sepsis after preliminary vaccination.
(*b*) Residual streptococcal infection of jaw (streptococcal rarefying osteitis) (Hurst, Payne and Knott).	
(*c*) Tonsillitis.	
(*d*) *Hunterian glossitis* (85 per cent.).	
(3) ORAL STREPTOCOCCAL INFECTION IN THE PRESENCE OF ACHLORHYDRIA LEADS TO DUODENAL STREPTOCOCCAL INFECTION. (Table IV.)	(3) Autogenous (duodenal) streptococcal vaccination; sour milk or *B. acidophilus* cultures; charcoal. Splenectomy.
The streptococci may then develop specific properties and produce	
HÆMOLYTIC (and bone-marrow) TOXIN AND ADDISON'S ANÆMIA.	NEUROTOXIN. SUBACUTE COMBINED DEGENERATION.
i. *Hæmolytic toxin*:— Anæmia, microcytosis, normoblasts, polychromasia	Paræsthesia; Anæsthesia;
v. d. Bergh's test (direct −, indirect +); lemon-yellow colour; spleen +; increased Fe in organs.	Alteration in reflexes; Ataxia; paraplegia.
ii. *Bone-marrow toxin*:— megalocytosis; high colour index; megaloblasts; leucopænia.	
ADDISON'S ANÆMIA WITH S.C.D.	S.C.D. WITH ADDISON'S ANÆMIA.
Paræsthesia; (alteration in reflexes).	Megalocytosis; (v. d. B.'s test); (lemon-yellow colour).
(4) Arsenic; transfusion. Exercises.	

TABLE I.—FREQUENCY OF ACHLORHYDRIA.

(*a*) *New Lodge Clinic and Medico-Neurological Unit, Guy's Hospital*, 1920-1924. (*Fractional Test-meals.*)

	Number of cases	Achlorhydria	
		Number	Percentage
Normal (Bennett and Ryle, Guy's Physiological Department)	100	4	4
Various medical diseases	762	86	15
Carcinoma of stomach	25	15	60
Chronic appendicitis (Bonar, Guy's Surgical Wards)	65	22	33
Gall-stones (Bonar, Guy's Surgical Wards)	35	17	49
Rheumatoid arthritis (A. F. H., Faber, Kahlmeter, Coates and Gordon) ...	150	59	40
Addison's Anæmia	36	36	100
Subacute Combined Degeneration of the Cord	25	25	100
Tabes dorsalis	13	1	7·7
Disseminated sclerosis	10	0	0

(*b*) *Other Authors (mostly Ewald Test-meals).*

		Number of cases	Achlorhydria	
			Number	Percentage
Addison's anæmia	Ewald	75	75	98·3
	Zadek	50	50	
	Weinberg	105	105	
	Levine and Ladd [1]	108	105	
	Faber	76	71	
	Strieck [1]	165	163	
	Faber, 1898	1	1	
Subacute combined degeneration	Barker, 1919	2	2	100
	Vanderhof, 1923	29	29	
	Sheard, 1924	3	3	
	Gordon, 1924	1	1	
	Worster-Drought, 1925	6	6	

TABLE II.—FAMILIAL OCCURRENCE OF ACHYLIA GASTRICA, ADDISON'S ANÆMIA AND SUBACUTE COMBINED DEGENERATION OF THE SPINAL CORD.

I.—*Achylia Gastrica.*

11 recorded families (Albu 7; Udaonda 1; Jung 1; A. F. H. 2).

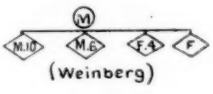
II.—*Addison's Anæmia.*

49 recorded families, varying between 2 and 6 in a family, in 1, 2 and 3 generations (Gulland and Goodall 5; Bramwell 1; Gilbert and Weil 1; Patek 1; Bartlett 1; Matthes 1; Roth 1; Faber 1; Levine and Ladd 9; Cabot 2; Schauman 6; Meulengracht 1; Mustelin 1; Gilford 2; Charles Hunter 3; Tscherning 1; Klein 1; Decastello 1; Minot 1; Panton, Maitland-Jones and Riddoch 1; Caccini; Morris and Falconer 3; A. F. H. 5.)

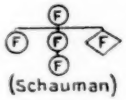
III.—*Addison's Anæmia and Achylia Gastrica.*

15 recorded families, in 2 of which more than one had Addison's anæmia (these are not included in No. II). (Martius 3; Queckenstedt 1; Weinberg 7; Schauman 1; Faber 1; A. F. H. 2.)

The most remarkable are the two following:—



(Weinberg)



(Schauman)

IV.—*Subacute Combined Degeneration of the Cord and Addison's Anæmia.*

5 recorded families. (A. F. H. 4; Willson 1.)

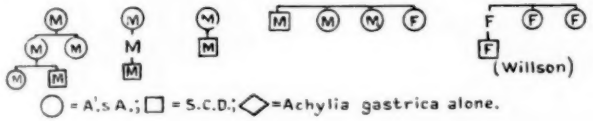


TABLE III.—HISTORY OF ACHYLIA GASTRICA PRECEDING ONSET OF SYMPTOMS OF ADDISON'S ANÆMIA OR SUBACUTE COMBINED DEGENERATION OF THE CORD.

Disease.	Number of Years.	Author.
A.'s A.	13, 10, 7, 3	Faber.
A.'s A.	12	Bie.
A.'s A.	7	Cobet and Morawitz.
A.'s A.	5	Kuttner.
A.'s A.	2	Chas. Hunter.
A.'s A.	6, 1	Levine and Ladd.
A.'s A.	10, 3	Wilkinson.
S.C.D.	1	Wilkinson.
S.C.D.	2	Szlapka.
S.C.D.	1	Hurst.

[1] These authors doubt the correctness of the diagnosis in the cases with free HCl present.

TABLE IV.—*Streptococcus longus* IN DUODENAL CONTENTS.

(a) By duodenal intubation (Lemnos, New Lodge Clinic, Guy's Hospital).

Disease	Number examined	Number present	Percentage	Notes	Bacteriologist
Normal...	4	0	0	When found, generally very scanty and slow growing. *In 19 with achlorhydria no streptococci found.*	C. J. Martin.
Infective jaundice ...	8	0	0 - 20		C. J. Martin.
Various diseases ...	133	29	21·8		F. A. Knott.
Addison's anæmia ...	28	21	75	In 4 of the 7 A.'s A. and in 1 of the 2 S.C.D. with no streptococci large doses of HCl had previously been given.	F. A. Knott.
S.C.D. of cord	13	11	85 - 78		R. D. Passey.
S.C.D. of cord	6	6	100		C. Worster-Drought.

(b) By direct puncture during abdominal operation (Guy's Hospital.)

Cholecystitis, duodenal ulcer, chronic appendicitis, and megacolon.	5	0	0	2 sterile; 2 a few *B. coli*; 1 a few *Staph. alb.*	W. H. Ogilvie and F. A. Knott.
Addison's anæmia...	2	2	100	Fairly abundant hæmolytic in one; very few, non-hæmolytic, in other.	F. A. Knott. A. Stokes.

N.B.—In 17 out of 19 of the cases of A.'s A. and S.C.D. in which the question was investigated, the duodenal streptococcus was hæmolytic; one exception was the only case of S.C.D. with no anæmia, and the other was S.C.D. with slight anæmia. (F. A. Knott.) In 5 out of 20 cases of other diseases in which streptococci were isolated from the duodenal contents they were found to have hæmolytic properties; this is about the average incidence of hæmolytic strains in the streptococci which are almost constantly found in the normal saliva. In 5 out of Worster-Drought's 6 cases the streptococci were hæmolytic.

Dr. H. J. MACBRIDE

said that he and Dr. Carmichael had collected the records of 199 cases of subacute combined degeneration which had been under treatment in the National Hospital for the Paralysed and Epileptic, Queen Square. In 53 of these the diagnosis was confirmed by autopsy. The average age at onset had been found to be 47½ years, the extremes being 26 and 70 years; 102 were males and 97 females. Heredity appeared to play no part in the ætiology. No definite predisposing cause had been identified and no attacks of previous anæmia had been noted. In 78 per cent. of the cases the primary symptoms had been neurological, such as stiffness or numbness of the legs, while in the remaining 22 per cent. the primary symptoms could be referred to the anæmia. Alimentary symptoms were uncommon; 12 per cent. of the patients had been edentulous, and 15 per cent. had been noted to have a sore tongue. In 14 cases free hydrochloric acid was found in the gastric juice by fractional analysis. A clinical resemblance to myxœdema had been observed in many cases, and some of these had taken large doses of thyroid extract, as much as 36 gr. a day, without signs of thyroid intoxication. He thought that Dr. Hurst's theory of the essential importance of achlorhydria was untenable, and he was inclined to think that some disturbance of endocrines was present. In treatment he had used arsenic, both by the mouth and by intravenous injections of novarsenobillon. He had not seen any benefit follow the treatment of oral sepsis nor the exhibition of large doses of dilute hydrochloric acid. In none of the cases which they had studied had any change in the neurological signs taken place as the result of treatment.

Dr. WILLIAM HUNTER

said that Dr. Hurst and Dr. Macbride had dealt with the subject matter of discussion from widely different points of view. He said he had recently done so at length in a paper read

before the Section of Medicine of this Society, entitled "The Nervous and Mental Disorders of Severe Anæmias in relation to their Infective Lesions, their Clinical Features, and their Blood Changes."[1]

This paper was based on the close study of 200 cases of pernicious anæmia—Addisonian anæmia—observed during the last thirty years. Two-thirds of these cases, or 66 per cent., were in males, the sex in which anæmia was least common, and over 80 per cent. were over the age of 40, the age at which ordinary forms of anæmia were least common, a remarkably peculiar feature of this disease, and the first of its many other remarkable features; the youngest being 25, the oldest being 73, and the average age being 57. All these cases presented nervous features of some kind or other, most commonly of the character of peripheral paræsthesiæ. But in 18 cases of the total 200 cases, or about 10 per cent., these nervous features were of the paralytic, spastic, and ataxic character present in subacute combined degeneration of the cord. Twelve of these cases (66 per cent.) were in males, and the same number and percentage over the age of 40; the lowest age being 30, the oldest being 67, and the average being 50. In these 18 cases he had seen the paralysis developing in every stage of the parent disease, from the first few months up to a period of five years after the parent disease had developed. He had seen cases in which the patients died from a period of eight months up to a period of five years after the onset of the nervous, paralytic features, and in the first to the sixth or seventh year of the parent disease. Lastly—and most interesting of all—among these 18 cases he had had two cases, still under observation, in which the original onset of the parent disease had dated back to 1916 and 1910. The paralytic features manifested themselves in 1922 and 1912 respectively. He had seen the patients pass through the severest attacks of their anæmia with blood-counts as low as 10 and 25 respectively; the patients were still under his observation, with definite features of subacute degeneration, the one in the ninth year of the parent disease, the other in the fourteenth; the latter patient had been in very good general and blood health for twelve years, the former was still in a state of instability, with blood-count at about 50 per cent. of the normal, although feeling very well.

As a result of his studies of these cases during their subacute and chronic course, he had seen the nervous condition in every stage of its progress from the first month up to the fourteenth year; accompanied by blood-changes of every degree from 10 per cent. of red cells up to 100 per cent.; with alimentary disturbances of the severest character—sickness, vomiting and diarrhœa—and with these all entirely absent; with glossitic features—sore tongue coming and going with great regularity every three or four weeks, over a period of two and a half years while the patient was under his observation, and with a history of sore tongue going back for six years previously to the time when his paralysis first began. He had seen this feature of sore tongue pass quite unrecognized, even in the case just referred to, although the patient had been under good observation. All of them presented in a striking manner the seasonal characters he had recently described in their onset, and, from time to time, in the phases of exacerbation through which they passed.

In all these respects he had no difficulty in reconciling the character of his cases with those of the 200 cases observed in the Queen Square Hospital, even in regard to the proportion of the cases (15 per cent.) in which the feature of sore tongue was noted to be present. That proportion, in a group of cases observed from 1900 onwards, was in his opinion a relatively high one, since it was only in the last five years that any attention had been given to this feature; the total references to this feature in the previous fifty years' history of the disease did not amount to more than ten or twelve; ten of these had been recorded by himself from among his own cases.

In one particular only—namely, in regard to the effect of treatment—did his experience differ from that described by Dr. Macbride, in none of whose cases had any change in the neurological signs taken place as the result of treatment. In his own experience, on the other hand, very decided improvement—sometimes marvellous improvement—in the degree of paralysis, in the powers of walking, in the degree of ataxia or spasticity, and in the cessation of other associated nervous features, had occurred as a result of removal of the infective factors associated with the parent disease. A case under his care in which the patient, in the course of two and a half years, had been twice at death's door with pernicious anæmia, and twice completely paralysed, had nevertheless twice greatly recovered, both from his anæmia and largely from his paralysis.

[1] *Proceedings*, 1922, xvi (Sect. Med.), pp. 1-42.

Significance of the Sore Tongue.

In relation to these nervous features, especially the combined degeneration now under discussion, by far the most interesting and informing lesions of the disease were the infective (streptococcal) lesions which he had described in the tongue, stomach and intestine. (A lantern demonstration of these lesions followed.)

Dr. F. A. KNOTT

said he considered that the most striking bacteriological feature of these cases of achlorhydria lay in the very large number of living organisms which evidently passed from the mouth and pharynx through the stomach into the duodenum. There being no direct evidence available as to whether these swallowed bacteria could produce a neuro-toxin, he would refer to their hæmolytic properties only. Streptococci predominated in the saliva of many people, but his own records certainly confirmed Dr. William Hunter's observation that the gastric contents in Addison's anæmia contained remarkably large numbers of living streptococci of the *Pyogenes longus* type, and that the same could be said of the duodenum. Among these particular streptococci he had found it possible to isolate, in nearly every case, strains which, when fresh, produced some degree of true hæmolysis of human blood. On the other hand, with only a few of these strains was the hæmolysin sufficiently abundant to allow of its isolation in bulk by filtration of cultures. But when considering this point in relation to the remote results of hæmolysin absorption from the small intestine, one had to remember that if the gastric acidity was constantly low, a very large number of living bacteria must reach the duodenum at practically all times, and, therefore, that the effect upon the blood might be related to minute doses of hæmolysin absorbed continuously over long periods.

With regard to the specificity of a streptococcus in these anæmias, the evidence was entirely indirect, namely, the constant finding of this class of organism, in large numbers, in the duodenum; but it seemed to be an important fact that from such cultures hæmolytic strains could generally be isolated, and this with greater frequency than from similar cultures of the average saliva. In some cases other hæmolysin producers had been detected, particularly staphylococci and occasionally coliform organisms, but on two occasions only was *Bacillus Welchii* found in the duodenum, and then in cases showing no anæmia.

In view of the presence of these hæmolytic duodenal organisms, whatever their type, one had at least to admit that in the majority of cases of Addisonian anæmia opportunity appeared to exist for absorption of greater or smaller amounts of bacterial hæmolysin from the intestine. That such direct absorption could occur seemed to be clearly indicated by the result of animal experiment. In rabbits, on three consecutive days, the stomach had been completely washed out with alkali through a rubber catheter, the animals receiving directly afterwards 10 c.c. of a strong suspension of hæmolytic duodenal micrococci run in through the catheter. In these animals the blood showed on the fourth day a definite fall in hæmoglobin content, the greatest fall being 15 per cent.

Dr. JAMES COLLIER

said that in all the many cases of subacute combined degeneration which he had seen, the blood-picture had been that of some stage of pernicious anæmia; but he laid stress on the fact that in one case which Dr. Greenfield had examined pathologically, and in which the spinal cord changes were typical, the blood-picture was that of chronic lymphatic leukæmia. He was of opinion that Dr. Hurst's teaching that peripheral subjective sensations in the limbs were indications of organic changes occurring within the nervous system, was highly dangerous, and that his ruling that the disappearance of such signs was proof of healing as the result of treatment was erroneous. He (Dr. Collier) had watched cases of pernicious anæmia in which peripheral subjective sensations had been marked over a number of years, up to the time of pathological verification, and he had been unable to find the least trace of any change in the nervous system. He was certain that tingling, numbness, &c., were of common occurrence in every variety of anæmia, and that these were not indicative of pathological changes in the nervous system. He (the speaker) had never seen any case under observation for pernicious anæmia develop subacute combined degeneration. In the latter malady the nervous symptoms seemed always to bring the patient under observation. He said that great care must be taken in determining matters of cure or arrest, since in rare cases great improvement might occur, almost to the disappearance of all symptoms, and yet fatal relapse would eventually follow. As an example, a woman had recently died in

the National Hospital in whose case he had diagnosed subacute combined degeneration more than ten years ago. Yet for eight years she did her work, and at one time had improved to such an extent that the extensor plantar reflexes were not obtainable. He was surprised at Dr. Macbride's remark that he had seen no temporary improvement in the condition of anæmia, for his own (Dr. Collier's) opinion was that this had been marked in some cases.

Dr. STANLEY BARNES

said that of the last twelve cases under his care at the General Hospital, Birmingham, only three conformed to the conditions which had been found in Dr. Hurst's cases. Of the other cases, in six there had been a subnormal acidity and some degree of anæmia, whilst in two cases the acid was normal, and in one a hyperacidity of the duodenal type was present. He thought that it was possible that under the heading of combined sclerosis two or more different types of cases were included, although from this series he had excluded all cases in which disseminated sclerosis was possible, or where gross vascular lesions existed, or in which the Wassermann reaction was positive. The association of some cases of combined sclerosis with achylia was too clearly defined to be a mere accident, and he came to the conclusion that Dr. Hurst was right in regarding achylia as the fundamental cause of the condition. He suggested that Addison's anæmia and combined sclerosis were two phases of one disease, according to whether the organs generally or the nervous system only was attacked by the toxin. He thought it was possible that the latter, gaining entrance to the body through lesions in the gastro-intestinal tract, could be absorbed either through the lymphatics and veins into the blood-system and give rise to Addison's anæmia, or become reabsorbed through the perineural lymphatics, and track along the sympathetic nerves upwards into the spinal cord and so give rise to combined sclerosis. He suggested that in further investigations an attempt should be made to see if any evidence of toxic damage or neuritis was present in the abdominal sympathetic nerves and their connexions.

Dr. ELLIS

said he, too, thought that Dr. Hurst's views must be held to be non-proven, especially as to the pathogenicity of the streptococci. He related the details of a case of great interest in which a man underwent a partial gastrectomy for a growth of the stomach in 1909 (proved to be carcinoma on section); now he presented the signs of subacute combined degeneration of the cord with a sore tongue, achlorhydria, and a blood-picture suggestive of pernicious anæmia.

Dr. HURST,

in a brief reply, maintained his own views, and said that he was unable to explain the statistics from the National Hospital.

Section of Neurology.[1]

President—Dr. E. Farquhar Buzzard.

Two Sisters from a Family Affected with Dystrophia Myotonica.

By J. L. Birley, C.B.E., M.D.

Family history.—Paternal grandfather, a teacher at Edinburgh University—healthy, and died of old age. All his children except two, namely, the patient's father and one daughter, died before 50, "most of them of phthisis." Father died, aged 57, of phthisis. In the last few years of his life he had difficulty in relaxing his grasp. He was a marine engineer and had his own business. All his children are in straitened circumstances.

Mother died, aged 62, of phthisis, contracted from her children. She was healthy, and her parents and all her brothers and sisters were healthy and lived to be old. She had eight children:—

(1) Female, died, aged 27, phthisis. Her hands were never developed properly and she could not relax her grasp.

(2) Female—patient, J., aged 45.

(3) Male, died, aged 36, phthisis. Affected like (2), could not open his hands quickly and was always weak.

(4) Male, married, unaffected, childless.

(5) Female, married, unaffected, but her sight is failing: has one healthy child.

(6) Female, married, unaffected, childless.

(7) Female, aged 34—Patient C.; has phthisis.

(8) Male, strong, died aged 19, suddenly, while on parade.

(2) Patient J., aged 45. Complains of having had difficulty in moving about for twelve years, and difficulty in getting up from chair: has to kneel as she cannot stoop down. Hands are stiff, but she does not complain about them. Recently her legs have felt numb.

Examination incomplete, but she shows the characteristic wasting of face, temporals, neck, forearms, and thighs. Myotonia present in flexors of wrist and fingers. Unilateral enlargement of thyroid. Articulation indistinct. Poor mentality. Early cataract.

(7) Patient C., aged 34. Four years chronic pulmonary tuberculosis with fibrosis. One year difficulty in walking, which is progressive; legs get tired. Never noticed anything wrong with her hands, and regarded as natural her inability to get up from the horizontal position unless she rolled over on her side and pushed herself up with her hands.

Examination: miserable physique, thin hair, atrophic breasts, normal menstruation. Fibroid phthisis left > right. Small gland right side of neck. Enlargement of right lobe of thyroid. Basal metabolism + 11 per cent. Poor mentality. Vision: right vision $\frac{6}{60}$, corrected $\frac{6}{9}$; left vision $\frac{6}{36}$; corrected $\frac{6}{9}$. Jaeger 1 in each eye. In each lens, more pronounced in the right eye, numerous extremely fine dust-like opacities are present in the cortex. Expressionless face, wasted temples, narrow high-arched palate, articulation indistinct, voice husky. Profound wasting of neck muscles, especially sterno-mastoids; upper trapezii wasted; head falls about. Musculature of shoulders and upper arms poor and muscles soft; right triceps weak. Wasted forearms, flexors weaker than extensors; myotatic irritability absent in flexors. Intrinsic muscles of hands fairly well preserved; finger movements good. All tendon reflexes in arms obtained with difficulty. Trunk very weak indeed. In the lower limbs the vasti are wasted and the other muscles are fairly normal except that they are soft and in places rather bulky. Moderate weakness of knee extensors and to a less extent of dorsi-flexors. Knee- and ankle-jerks feeble. Plantars flexor. Legs and feet feel numb; vibration is not felt, or at least is grossly diminished in the upper limbs and everywhere below the level of the third ribs. No other sensory disturbance.

Myotonia is obvious only in the hand grasps, but is demonstrable in frontalis, tongue, deltoids, hypothenar muscles and (?) plantar flexors.

[1] Clinical meeting held at the National Hospital for the Paralysed and Epileptic, Queen Square, Bloomsbury, March 12, 1925.

A Case of Dystrophia Myotonica.

By J. L. Birley, C.B.E., M.D.

Male, aged 39, a clerk in a responsible position.

Family history negative. Father healthy, drowned at sea, aged 50. Mother alive and well, aged 67. One brother aged 43, healthy, with three healthy children. Patient has four healthy children, aged 12 to 1. Both grandparents died aged 85. No history of cataract.

Past history good; always active and fairly athletic. Has had a "weak neck all his life." Four years ago he gave up sculling as he could not hold the oars properly. Three years ago he noticed wasting of the thighs, but he stoutly maintains that although he has occasionally fallen, his legs are not weak, and he frequently walks miles at a stretch. His arms have been getting thinner, but his writing is unaffected; he had recently noticed difficulty in relaxing his grip. No deterioration of vision or of sexual power.

On examination.—An intelligent man of pleasant disposition and cheerful temperament. Characteristic facies, expressionless, all muscles weak and wasted, temporal fossæ hollowed, drooping eyes. No ophthalmoplegia. Articulation defective and modulation poor. No retinal changes. In the peripheral cortex of each lens, especially the right, there are fairly numerous, extremely fine dust-like opacities and several vacuoles. Tongue fissured. All neck muscles profoundly weak and wasted, more particularly the sterno-mastoids. Shoulder-girdle and upper arms show no paresis, but musculature is flabby. Pronounced wasting of forearms, especially extensors, but flexors are much weaker than extensors and have lost their myotatic irritability. Intrinsic hand muscles fairly well preserved with good finger movements. All trunk muscles slightly weak. Musculature of lower limbs is fair with the exception of the quadriceps, which is very weak and wasted; the limb cannot be held extended at the knee when raised from the bed. Some weakness of hip extensors. The deep reflexes in both arms and legs, although diminished, are present. Plantars flexor. No sensory disturbance.

Myotonia is obvious in hand-grasps, and can be demonstrated in the wrist extensors and deltoids, but not in the tongue.

Report of Dr. Murray Levick on electrical reactions of facial, neck, forearm, and thigh muscles and deltoids: "No abnormal reaction detected; the response to faradism in some of them is weak, but not more weak than one would expect to find it in muscles that have wasted to that extent. There is no prolongation of the contraction following faradic stimulation of moderate strength after the current has ceased to flow. The response to interrupted galvanic stimulation with metronome interrupter is normal."

X-ray of skull: Very marked enlargement of frontal sinuses. Sella turcica open widely and rather larger than normal. Chest shows thickening at the lung roots.

Hair on scalp thin, bald in centre. Thyroid small. No obvious endocrine disturbance. Basal metabolism minus 9 per cent.

Hypopituitarism with Retinitis Pigmentosa and Polydactylism.

By Douglas McAlpine, M.D.

Boy, aged 15 years and 9 months, "always big and fat"; at age of 8 years began to have failure of vision, which has progressed. In December, 1924, he sustained a fracture of right leg; since then apathetic and apprehensive.

Present History.—Whooping-cough and measles in infancy, rheumatic fever at age of 5, no history of fits.

Family History.—Father and mother alive and well. Two brothers and four sisters alive and well.

General appearance suggests hypopituitarism. Weight 10 st. 13½ lb. Height 5 ft. 9 in. Subcutaneous fat increased in amount in region of breasts, pelvis and hips. Upper and lower limbs have female contour. Well-marked eyebrows, scant, downy hair on upper lip and limbs; scant axillary hair. Abundant pubic hair with typical feminine distribution. Genitals slightly underdeveloped. Lower jaw recedes; incisors upper jaw crowded together and inclined backwards.

High narrow palate. Supernumerary digit on each foot; small sessile mass growing from base of fifth digit of left hand.

Vision.—He can only perceive light and hand movements. Retinitis pigmentosa, with changes most marked in the macular regions. Optic discs: arteries small, but otherwise normal.

No evidence of organic disease in central nervous system. Psychical condition: apathetic; his answers to questions are variable in time and substance. Attention poor. No true dementia. X-ray of skull: sella turcica normal in size and appearance. Wassermann reaction (blood) negative. No glycosuria.

A Case of Facial Hemiatrophy: Lack of Development of the Breast on the same side.

By J. P. MARTIN, M.D.

PATIENT is a woman, now aged 20, in whom right-sided facial hemiatrophy commenced at the age of 15. The right breast has never developed, and there is now a deficiency of subcutaneous tissue on the right side of the chest extending down to the bottom of the pectoralis major muscle.

A Case for Diagnosis (Probably a Variant of Charcot-Marie Progressive Neurotic Amyotrophy—Peroneal Type of Tooth).

By J. P. MARTIN, M.D.

History.—Female, aged 13, was well till eighteen months ago, then she suffered from ichthyosis on the forearms. In the spring of 1924 she had four or five fits during a period of about three months: in these fits she became rigid and her head was drawn back: she did not "struggle" and did not lose consciousness.

Soon afterwards her hands began to waste.

The family doctor reports that an elder brother of the patient suffered from a similar disease: his illness began with ichthyosis, then his hands wasted; afterwards his lower limbs became spastic. He was sent to a hospital in Nottingham where he died. A post-mortem examination was held. The doctor was informed that death was due to caries of the cervical vertebræ.

Examination.—Wasting of hands. Slight weakness of dorsiflexion at the right ankle. Otherwise no loss of power. Reflexes all normal. No sensory loss. No changes in cranial nerve functions.

A Case of Tabes Dorsalis.

By N. L. LLOYD, L.R.C.P., M.R.C.S.

F. D., AGED 41. 1912: syphilis. 1921: had a tired feeling, especially in the calves of the legs after a day's work. Occasional stabbing pains in knee-joints.

March, 1922: Pupils A.-R., right larger than left. Ptosis of right eyelid. Vibration diminished over sacrum. Deep reflexes all present. Plantar reflexes not obtained. Cerebro-spinal fluid, cells 54 per c.mm., protein 0·04 per cent. Wassermann reaction strongly positive in cerebro-spinal fluid and blood.

October, 1922. Feeling less tired. Typical lightning pain in calves. Right pupil displaced slightly inwards. Both pupils slightly irregular. Right plantar reflex unobtainable. Left flexor. Ankle-jerks absent. Vibration sense diminished over external malleoli and sacrum.

March, 1923: Pupils A.-R., both displaced inward, right larger than left. Cerebro-spinal fluid, cells 9 per c.mm., protein 0·045 per cent. Wassermann reaction positive in cerebro-spinal fluid and blood.

February, 1925: Pupils fixed to both light and accommodation, right larger than left. both irregular and eccentric. Knee-jerks, right brisker than left; ankle-jerks, right absent, left present but weak. Cerebro-spinal fluid, cells 13 per c.mm., protein 0·025 per cent. Wassermann reaction still positive in cerebro-spinal fluid and blood. Gait normal. Sensation unimpaired.

He has been treated energetically for three years, receiving about 20 grm. of novarsenobillon intravenously, as well as mercury and iodide by mouth. During this time the ankle-jerks became lost, but one has now returned. The Wassermann reaction in the blood and cerebro-spinal fluid has remained "strongly positive." He is still able to walk twenty miles a day.

Case of Myoclonus Epilepticus.

Shown by W. G. Wyllie, M.D. (for Robert Hutchison, M.D.).

H. W., aged 1 year eight months, has "for several weeks" had attacks of twitching of the limbs, trunk, head and face, almost continuously when awake.

He was born at full term, sat up at six months, but makes no attempt to walk, and is backward in speaking.

Family History.—Mother a very nervous woman. Maternal grandmother in an asylum with dementia and chronic epilepsy. There is one other child, 3 weeks old.

In the attacks there are clonic movements of the arms and fingers, legs, head, eyes and eyelids, and lips. Consciousness is usually retained, and the child may continue to smile or to feed himself. In the more severe attacks the child's head gradually sinks down forwards, almost touching his feet, and the eyes assume a vacant look, as seen in *petit mal.*

Case of Post-encephalitic Myoclonus and Tachypnœa.

By Macdonald Critchley, M.D. (for James Collier, M.D.).

E. S., a butcher boy, aged 19. In March, 1924, had an attack of epidemic encephalitis, characterized by insomnia, delirium and myoclonus of the shoulder muscles. The patient's recovery was never complete; he became dull in mentality. Insomnia gave place to lethargy, and the twitching of the muscles persisted.

His present state is one of mild Parkinsonism. There is definite myoclonus in the upper halves of both trapezii, and to a less degree in the pectoralis, deltoid, biceps, and triceps of the left side. The upper bellies of the recti abdominis also show myoclonus. These movements cease during sleep.

The respiration is rapid (40 per minute), and each inspiration synchronizes with the myoclonic jerk. The breathing is noisy, especially during sleep. The tachypnœa, which persists throughout day and night, is not accompanied by any thoracic distress.

Case of Post-encephalitic Contractures and Paroxysmal Tachypnœa.

By Macdonald Critchley, M.D. (for James Collier, M.D.).

I. C., female, aged 23. Last June was attacked with acute epidemic encephalitis (diplopia; delirium). Patient has never made a complete recovery from this illness.

Gradual onset of Parkinsonism. Patient states that ever since last June her right hand has been contracted. At present the right hand is claw-like in attitude. Feeble flexion is possible, but extension is impossible. The right elbow is also contracted and cannot be extended past a right angle. The left elbow cannot be extended to more than 120°. Both legs are fixed in extension and the feet are drawn up in an exaggerated pes equinus attitude.

Patient's respiration is normal during the waking state, but when asleep the breathing becomes faster (38 per minute), regular, and shallow.

A Case of Migraine.

By W. Aldren Turner, C.B., M.D.

Five years ago patient became subject to severe headaches: on one occasion this was followed by transient hemianopia. For the past twelve months has had a series of sensory attacks, affecting the right side of the body. The attacks commence with a sensation of helplessness followed by pins and needles in the right hand. This sensation then passes

up and down the trunk and right leg; occasionally the right half of the face and tongue become affected. The attack is followed by flashes of light before the eyes. Since the onset of these "turns" the severe headaches have disappeared, being replaced by a dull pain in the left side of the face, aggravated by coughing, sneezing or stooping.

Physical examination revealed a brisk jaw-jerk. The right plantar response varies from day to day, but is usually extensor in type. There is no objective disturbance of sensation.

There is a gross error of refraction in both eyes; fields of vision normal; the left antrum of Highmore contains a mucocele; Wassermann reaction negative.

Case of Peroneal Muscular Atrophy with Pseudo-hypertrophy and Atrophic Sterno-mastoids.

Shown by REDVERS IRONSIDE, M.B. (for J. L. BIRLEY, C.B.E., M.D).

J. G., MALE, aged 34, single, railway porter. Patient states that at school he was always "short-winded" if he ran far. Six years ago he obtained employment as a porter and has been accustomed to lift 2½ cwt. loads. Four years ago he noticed weakness and wasting in the left ankle, and the left foot dropped. Eight months afterwards he noticed "quivering" of the calf muscles of the left leg. Gradually the weakness has spread to the right leg; since the wasting has been present both feet have become blue and cold with a subjective sensation of pins and needles. One year before the patient first noticed the wasting he sprained his left ankle and was in bed for eleven days. At the age of 9 years had tuberculous adenitis. No family history of similar disease ascertained; his parents were first cousins.

On examination: (1) Cyanosed and wasted "champagne-bottle calves," wasted thighs and wasting of the small muscles of both hands; (2) Atrophic sterno-mastoids, especially the sternal heads; (3) Pseudo-hypertrophy in vastus internus on the two sides; (4) Slight involvement of the lower facial musculature (patient cannot blow out his cheeks); (5) Absent ankle-jerks.[1] The abdominal reflexes are brisk and the plantar responses are flexor. In the lower limbs the weakness and wasting affect the calf muscles generally, the adductors of the thigh, the glutei slightly, the hamstrings and the ileo-psoas. Gait waddling, with slight lordosis. In rising from the extended position on the floor patient tends to "climb up his thighs." Intensely marked condition of medullated nerve fibres in both optic discs.

Case of Epilepsy for Diagnosis as to Cause.

By C. P. SYMONDS, M.D.

MALE. aged 25. In 1922 he began to have occasional epileptic attacks. These gradually increased in frequency and severity. In 1923 paroxysmal headaches commenced, associated with nausea. The fits were now sometimes preceded by left-sided paræsthesiæ. Occasionally he had dreamy states with visual hallucinations.

Condition in June, 1924.—Optic discs pale but not swollen. Visual acuity 6/6 both eyes. Slight weakness left palate and right tongue. Tendon-jerks brisker in left upper and lower limbs than in right. Wassermann reaction negative in blood and cerebro-spinal fluid (twice examined). Cerebro-spinal fluid, twice examined, showed (1) cells 45, protein 0·09 per cent.; (2) cells 51, protein 0·16 per cent.; colloidal benzoin curve 2.2.2.2.2. (paretic curve). X-ray of skull showed large sella turcica, sutures rather wide, some dimpling in frontal region.

Treated as for syphilis during next three months with mercury, iodides and intravenous N.A.B. Continued to have headaches and occasional fits.

Condition in October, 1924.—Increased pallor of optic discs with haziness of edges. General constriction of visual fields. Condition otherwise unchanged. November 6 to 8, given six applications of deep X-rays to region of interpeduncular space. These caused severe headaches and an increase in the number of fits.

Condition in March, 1925.—During the past four months there has been an average of one fit a week. Has occasionally had an aura of tingling in *right* face. Mental condition dull.

[1] Osseous sensibility to vibration diminished over tibial malleoli.

Slight proptosis. Optic discs pale and edges indistinct. Visual fields unchanged. Weakness right face, left palate, left grip. Left knee- and ankle-jerks brisker than right. Wassermann reaction negative in blood and cerebro-spinal fluid. Cerebro-spinal fluid shows cells 9, all lymphocytes, protein 0·1 per cent., chlorides 75 per cent., sugar present, benzoin curve 2.2.2.2.1.

Five Cases Exhibiting Involuntary Movements.

By S. A. Kinnier Wilson, M.D.

(1) Senile Chorea.

Female, aged 87 years. In 1913 involuntary movements of right arm commenced. They have progressively increased in range and intensity, and have spread to all four limbs. Mentally she remains as acute as when seen in 1920. No other member of family affected. Movements are choreic in nature and are most marked in the right arm and leg. Plantars flexor.

(2) ? Epilepsia Partialis Continua.

Male, aged 53 years. Eighteen months ago the little finger of left hand tended "to get in the way." Six months later tremor of finger movements and power in left hand became weak. Occasional spontaneous pain in fingers and knuckles. Left hand held in exaggerated accoucheur's position with thumb well adducted. Tone increased. Marked irregular tremor of fingers. Antagonists and agonists contract simultaneously and separately. Left face has less expression than right. Plantars flexor. No sensory change.

(3) ? Epilepsia Partialis Continua.

Female, aged 42 years. Eighteen months ago left hand suddenly became weak. Twelve months ago, on examination, hand flexed at wrist and fingers extended at metacarpal-carpal joints with œdema of back of hand. No tremor. Six months ago practically no voluntary movement possible in left hand. Slight, apparently regular tremor of fingers, which has been gradually progressive, has arisen in last six months. No other physical sign in central nervous system.

(4) Progressive Lenticular Degeneration.

Female, aged 25 years. Two and a half years ago tremor of left leg and right arm commenced, gradually progressing to other limbs. No history of acute illness. No history of jaundice. Sweating of face—marked emotional instability. Voice monotonous. No difficulty in swallowing. Ocular movements good. Tends to keep mouth open. Constant involuntary movements of arms and legs. Reflexes normal. Plantars flexor. Liver function tests show deficiency. No pigmentation of cornea. No family history of similar trouble.

(5) Tremor. ? Nature.

Male, aged 69 years. Twelve years ago patient noticed tremor of hands in volitional movement. This gradually progressed and now involves jaw. At rest no tremor of hands; on voluntary movement tremor of hands of paralysis agitans type. No rigidity of arms. No Parkinsonian stance. No immobility of face. Plantars both doubtfully extensor. Knee-jerks very active. Wassermann negative. No sensory disturbance.

See-Saw Movements of Eyelids.

By James Collier, M.D.

Female, aged 4. Since birth mother has noticed peculiarity of eyes.

On examination right pupil is small and inactive to light. Left, average size, reacting well to light and on accommodation. On deviation of eyes to right, right eyelid closes; deviation to left, right eyelid opens. No upward or downward movement of right eye. Perfect movement of left eye. Wassermann reaction negative.

A Case of Myotonia occurring in a Male aged 62 (with Cinematograph Demonstration).

By W. J. Adie, M.B., and Redvers Ironside, M.B.

W. W., clerk, aged 62. No family history of the disease can be ascertained and the patient himself has always been healthy and athletic.

Three months ago he had sensation of constant "pins and needles" in the hands and arms and feet and legs, and stiffness in the lower limbs on attempting to walk after sitting down or standing still. This stiffness lasted as long as 1½ to 2 minutes, then he could walk on quite easily.

The march of the disease has been rapid. Two weeks later he noticed difficulty in relaxing his grasp after seizing objects. One week later he began to experience a tight feeling round the eyes with inability to open the eyes for a few seconds when he screwed them up tightly.

After gazing at a clock above and to his left he has noticed double vision—he would see the clock with the image of "half a clock" beneath. Soon, when he began to speak, his voice was like a whisper, but as he went on talking it became normal again. The slightest cough caused a spasm of his thoracic musculature, rendering him completely apnœic with increasing cyanosis lasting 40 to 60 seconds. Lately he has had difficulty in opening his jaws after biting food and he is now quite unable to swallow fluids.

When he strains at stool he has painful cramps in his lower abdominal muscles. He is unable to roll over on his side in bed. These spasms are always induced by voluntary movement, and repetition of the movement makes it easy to perform. There are times when he knows "instinctively" (as he says) that he can perform a movement without its causing a spasm. Sudden movements and contact with cold water freeze, as it were, his limbs into a rigid condition. Since the onset the patient has become self-conscious, emotional and depressed. During the respiratory attacks he feels as if his death is approaching.

On examination the muscles generally are well developed and hard, especially the calf muscles. There is no muscular atrophy. Bilateral peripheral opacities are present in the lower and inner quadrants of both lenses. Some degree of spasm is almost constantly present in the face during conversation, causing narrowing of the palpebral fissures, stiffness of the lower face and drawing the corners of his mouth into a "risus." Myotonic phenomena very marked in the limbs, and myotonia in the tongue can be revealed by the dimpling on percussion. The characteristic electrical reactions are present. The reflexes are elicited. The patient presents besides signs of cardio-renal degeneration. Electro-cardiogram shows left ventricular preponderance. Examination of the larynx shows sluggish movement of the left arytænoid. Pharyngeal and œsophageal delay in swallowing a bismuth meal. Blood and cerebro-spinal fluid calcium normal.

Two Cases of Disseminated Sclerosis with Acute Posterior Column Lesions.

By C. M. Hinds Howell, M.D.

(1) G. T., male, aged 26. Store issuer. Five weeks ago noticed feeling of "pins and needles" in the soles of both feet, spreading to the legs. Ten days later there were "pins and needles," numbness and unsteadiness in both hands. The left leg has become stiff and he drags it slightly. No sphincter trouble or diplopia.

On examination there is slight left ptosis. With arms outstretched and eyes shut, wandering "pseudo-athetotic" movements occur. There is slight peripheral loss to cutaneous warmth and pressure pain in hands and feet. Sense of passive movement in joints absent in the fingers, elicited in the toes. Osseous sensibility to vibration absent in both radial styloids, diminished over the left tibial malleolus. There is astereognosis in both hands. The left plantar response is extensor.

(2) S. G., male, aged 19, clerk. Seven weeks ago noticed numbness in the feet and in the ring finger of the left hand, with dragging gait for a few days. Two weeks later his handwriting commenced to alter, became clumsy, and he felt numbness in both hands.

On examination there is slight peripheral loss to cutaneous warmth and pressure pain in both hands. Sense of passive movement in joints absent in the fingers, elicited normally in the toes. Osseous sensibility to vibration diminished over the radial styloids, slightly diminished over both tibial malleoli. Complete astereognosis in the palms of both hands. With arms outstretched and eyes shut, both upper limbs wander in space. Marked sensory ataxia of both upper limbs.

Case of Upper Brachial Plexus Root Lesion.

Shown by REDVERS IRONSIDE, M.B. (for C. M. HINDS HOWELL, M.D.).

A. M., MALE, aged 60 years. Innkeeper.

Four weeks ago, retiring to bed, patient had occasion to use the water-closet. On rising he fell, and his left arm plunged into the closet, the edge of the pan wedged in his left axilla and grazing his ribs on the left side. He lay there for forty minutes. Next morning his left upper limb was perfectly useless.

On examination of the left upper limb there is seen to be paralysis and wasting of the latissimus dorsi, the infraspinatus and subscapular muscles, the deltoid, triceps, and biceps, and pronators of the wrist. There is weakness of the supinator longus and the extensors of the fingers. The flexors of the fingers are less affected. Slight wasting of the first dorsal interosseous space. The intrinsic muscles of the hand function well except the abductor of the thumb. Patient grasps the observer's hand flexing the wrist to a right angle, with slight ulnar deviation of the hand.

No sensory loss apart from slight relative impairment of superficial sensation over C 5 and 6 root areas. The affected muscles react to direct percussion, and show partial R.D. Plantar responses are flexor.

Case of Quadriplegia, showing well-marked Postural Tonus.

By PHILIP CLOAKE, M.D.

J. C., AGED 39. Printing-machine minder. Good health, except for dysentery in the Army, until March, 1920. Paralysis of extensors of fourth and fifth digits of left hand. Pain left arm.

May, 1920: Sudden paresis and stiffness of left leg, and a few days later of left arm.

June, 1920: Right arm similarly affected, very sudden onset.

February, 1921: Attacks of pain running up the spine with a choking sensation and extreme flexion of the legs, lasting for thirty seconds and terminated by change of position. No loss of consciousness.

December, 1924: Right leg became stiff and weak.

Has had several courses of injections of N.A.B. without material improvement in the London Hospital and the West End Hospital for Nervous Diseases.

Family History.—Father and mother alive and well. Four brothers, one of whom committed suicide; four sisters, one of whom has been insane. Wife alive and well; three children said to be healthy; no miscarriages.

Mental State.—Somewhat unduly emotional.

Cranial Nerves.—Normal; except enophthalmos of left eye, exaggerated jaw-jerk.

Trunk.—Well-marked scoliosis, probably arising during this illness. Weakness of flexion of head and trunk. No loss of sensation. Abdominal reflexes absent.

Arms.—Moderate increase of postural tonus more in right arm than left, more in triceps than biceps and in flexors of wrist than extensors. Lengthening and shortening reactions present. Double drop-wrist and slight wasting of wrist extensors. Power at shoulders quite good. Flexion at elbow much stronger than extension. Grips weak; right more than left. Pronation very feeble; left worse than right. Gripping with either hand causes extension of other three limbs, and also adduction and pronation of other arm. Voluntary movements at the elbow and wrist cause associated symmetrical movements in opposite arm. Yawning causes extension of arms and fingers. Tonic neck influences not apparent. Supinator, biceps- and triceps-jerks brisk on both sides. Stimulation of palm causes extension and adduction of thumb.

Lower Limbs.—Marked increase of postural and contractile tonus in all muscles. Paresis in extension, left weaker than right. Progressive and spontaneous shortening of extensors of knees when flexed. Flexion of knees is associated with flexion and pronation of arms. Marked shortening reaction in quadriceps on eliciting knee-jerks, with steady increase of contractile tone until complete extension is reached. Plantars extensor. No flexion reflex. Hyperæsthesia of soles with impaired vibration sense in legs. Bladder unaffected. Constipation marked. Wassermann reaction in blood + +. Cerebro-spinal fluid: Wassermann + + +; globulin + + +. Protein, 0·1 per cent.; cells, 7 per c.mm.